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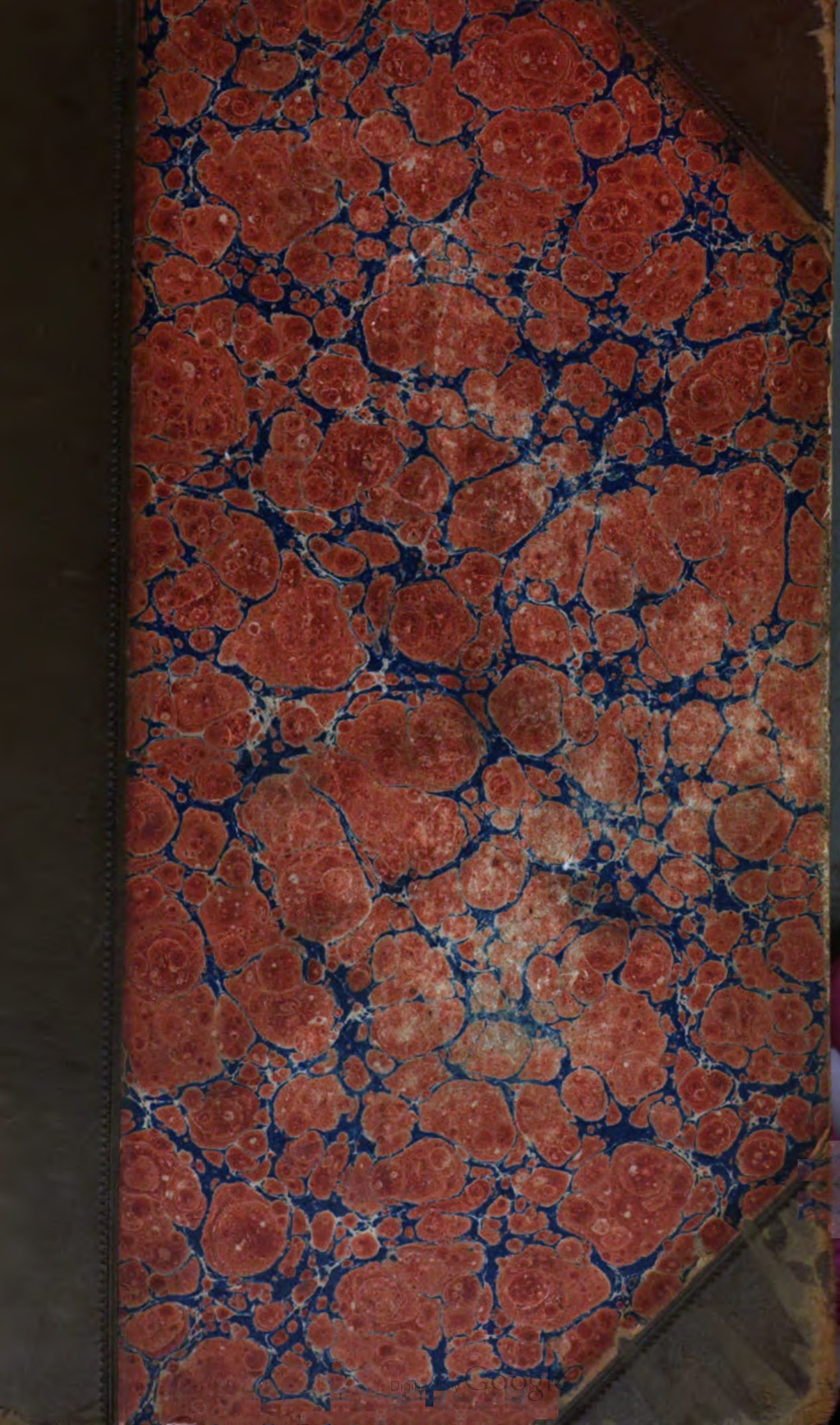

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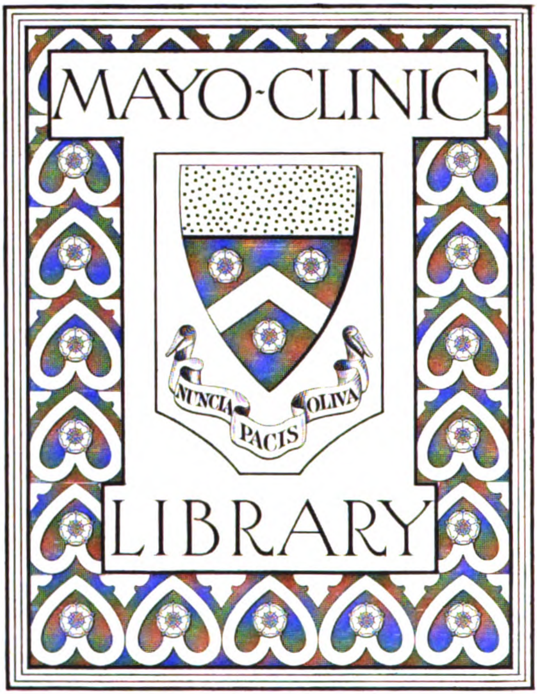
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NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

Royal Institution of Great Britain,

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS.

VOLUME VIII.
1875—1878.

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„ 12.—**W. Froude**, Esq.—Fundamental Principles of the Resistance of Ships .. ..

„ 19.—**C. T. Newton**, Esq.—Recent Discoveries at Olympia

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June 2.—**Professor Roscoe**—Recent Discoveries about Vanadium .. ..

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Royal Institution of Great Britain.

GENERAL MONTHLY MEETING,

Monday, November 1, 1875.

C. William Siemens, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Major-General W. S. S. Mulcaster

was *elected* Member of the Royal Institution.

The following Communication was read:—

"The Managers much regret having to announce to the Members the unexpected death of one of the most valued Members—Sir Charles Wheatstone.

"Sir Charles Wheatstone has been a Member of the Royal Institution since the year 1846. Previously to that time he had made a communication to the Evening Meetings on his researches in Acoustics; and from that date he has from time to time lent his valuable aid to the objects of the Institution, as one of the Managers, and by discoveries which have been exhibited on the Library Table, and formed subjects for Discourses at the Evening Meetings and the Morning Lectures.

"His high position as an original investigator in Physical Science, his extensive knowledge of all branches of Experimental Research, and his readiness to impart it, and general kindly disposition, are well known to a large body of the Members of the Royal Institution."

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Secretary of State for India—Synopsis of the Great Trigonometrical Survey of India, Vol. IV. 4to. 1875.
New Zealand Government—Census of New Zealand, March 1, 1874. fol. 1875.
Actuaries, Institute of—Journal, No. 99. 8vo. 1874.
American Academy of Arts and Sciences—Proceedings, Vol. X. 8vo. 1874–5.
American Philosophical Society—Vol. XV. Part 2. 4to. 1875.
Proceedings, Nos. 93, 94. 8vo. 1874–5.
Antiquaries, Society of—Proceedings, Vol. VI. No. 4. 8vo. 1875.
Asiatic Society of Bengal—Journal, 1874, Part 1, No. 4; Part 2, No. 4; 1875, Part 1, No. 1. 8vo.
Proceedings, 1874, No. 10; 1875, Nos. 1–5. 8vo.
Asiatic Society, Royal—Journal, Vol. VII. Part 2. 8vo. 1875.
Annual Report, 1875. 8vo.

Boston Society of Natural History—Memoirs, 3 Parts. 4to. 1874–5
Proceedings, Vol. XVII. 8vo. 1874.
Bourchier, Lady (*the Editor*)—Memoir of the Life of Admiral Sir Edward Codrington. 2 vols. 8vo. 1873.
British Architects, Royal Institute of—Sessional Papers, 1874–5, No. 13. 4to.
British Association for the Advancement of Science—Report of the 43rd Meeting at Belfast, 1874. 8vo. 1875.
British North American Boundary Commission—Report on Geology and Resource of the Region in the 49th Parallel, from the Lake of the Woods to the Rock Mountains. By G. M. Dawson and Major D. R. Cameron. 8vo. 1875.
Chemical Society—Journal for July–Oct., 1875. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vols. XL. XLI. 8vo. 187
Comitato Geologico d'Italia—Bollettini, 1874. Nos. 9–12. 1875: Nos. 1–6. 8vo.
Commissioners in Lunacy—Twenty-ninth Report. 8vo. 1875.
Cornwall Polytechnic Society, Royal—Forty-second Annual Report. 1874. 8vo.
Editors—American Journal of Science for July–Oct., 1875. 8vo.
Athenæum for July–Oct., 1875. 4to.
Chemical News for July–Oct., 1875. 4to.
Electrical News for July–Oct., 1875.
Engineer for July–Oct., 1875. fol.
Journal for Applied Science for July–Oct., 1875. fol.
Nature for July–Oct., 1875. 4to.
Nautical Magazine for July–Oct., 1875. 8vo.
Pharmaceutical Journal for July–Oct., 1875. 8vo.
Practical Magazine for July–Oct., 1875. 8vo.
Telegraph Journal for July–Oct., 1875. 8vo.
Elliot, the Lady—History of India by its own Historians, Vol. VI. 8vo. 1875.
Fiorentino, Sig. V. (*the Editor*)—Prose e Poesie Italiane della Raccolta Arborens 16to. Napoli, 1870.
Franklin Institute—Journal, Nos. 592–597. 8vo. 1875.
Geographical Society, Royal—Proceedings, Vol. XIX. Nos. 6, 7. 8vo. 1875.
Geological Society—Quarterly Journal, No. 123. 8vo. 1875.
Glasgow Philosophical Society—Proceedings, Vol. IX. No. 2. 8vo. 1875.
Hancock, C. F. jun. Esq. M.R.I.—D. Lloyd: State Worthies, or the Statesme and Favourites of England since the Reformation. 2nd ed. 1670.
Harlem, Société des Sciences—Archives Néerlandaises, Tome X. Liv. 1, 2. 8v 1875.
Hayden, F. V. Esq. United States Geologist—Bulletins, Second Series, Nos. 2, 8vo. 1875.
Contributions to the Fossil Flora of the Western Territories, Part 1. 4t 1874.
Irish Academy, Royal—Transactions, Vol. XXV. Science, Parts 5–19. 4to. 1874–
Proceedings, New Series, Vol. I. Nos. 9, 10; Vol. II. Nos. 1–3. 8vo. 1873–5.
Iron and Steel Institute—Journal, 1875, No. 1. 8vo.
Jackson, Lowis D. A. Esq. (*the Author*)—Hydraulic Manual, Part 1. Workin Tables and Text. 8vo. 1875.
Linnean Society—Transactions, Vol. XXIX. Part 3; Vol. XXX. Parts 2, Second Series: Botany, Vol. I. Part 1; Zoology, Vol. I. Part 1. 4to. 1874–
Journal, Nos. 80, 81. 8vo. 1875.
Mechanical Engineers' Institution, Birmingham—Proceedings, 1875: April, June July. Part 1. 8vo.
Medical and Chirurgical Society, Royal—Transactions, Vol. LVIII. 8vo. 1875.
Meteorological Office—Meteorological Committee of the Royal Society: Report fo 1874. 8vo. 1875.
Quarterly Weather Report, 1873, Part 4; 1874, Part 1. 4to. 1875.
R. H. Scott: Instructions for the Use of Meteorological Instruments. 8vo. 1875
Meteorological Office, Toronto—Meteorological, Magnetic, and other Observation in Canada in 1874. 8vo. 1875.
Meteorological Society—Quarterly Journal, New Series, No. 15. 8vo. 1875.

Müller, M. Albert, Esq. (the Author)—Ein Fund vorgeschichtlicher Steingeräthe bei Basel. (M 9) 4to. 1875.

Preussische Akademie der Wissenschaften—Monatsberichte: April, Mai, Juni, 1875. 8vo.

Regnault, M. A. (the Translator)—Chef d'Œuvres de Lord Byron, traduits en vers Français. 2 vols. 8vo. Paris. 1874.

Roma, Accademia dei Lincei—Atti, Anni VIII. IX. & XXVI. Sess. 5–8. 4to. 1874–5.

Royal Society of London—Proceedings, Nos. 162, 163. 8vo. 1875.

St. Petersburg, Académie des Sciences—7ᵉ Série. Tome XXI. Nos. 6–12; Tome XXII. Nos. 1–3. 4to. 1874–5.

Bulletins, Tome XIX. Nos. 4, 5; Tome XX. Nos. 1, 2. 4to. 1874.

Sands, Admiral B. F. (the Superintendent)—Washington Astronomical and Meteorological Observations for 1872. 4to. 1874.

Smithsonian Institution—Smithsonian Report for 1873. 8vo. 1874.

Statistical Society—Journal, Vol. XXXVIII. Parts 2, 3. 8vo. 1875.

Street, Mr. G.—Map of New South Wales. 1875.

Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, July–Oct., 1875. 8vo.

Taylor, Alfred S. M.D. F.R.S. M.R.I. (the Author)—On Poisons. 3rd ed. 12mo. 1875.

Thomas, W. Cave, Esq. (the Author)—The Revised Theory of Light. 12mo. 1875.

Tyndall, Professor, D.C.L. LL.D. F.R.S. (the Author)—Six Lectures on Light. 2nd ed. 16to. 1875.

Sound. 3rd ed. 16to. 1875.

United Service Institution, Royal—Journal, Nos. 81, 82. 8vo. 1875.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen. Jan.–Juni, 1875. 4to.

Victoria Institute—Transactions, No. 34. 8vo. 1875.

Zoological Society—Transactions, Vol. IX. Part 4. 4to. 1875.

Proceedings, 1875, Parts 2, 3. 8vo.

List of Vertebrated Animals. Supplement. 8vo. 1875.

GENERAL MONTHLY MEETING,

Monday, December 6, 1875.

C. William Siemens, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Arthur Cates, Esq. M.R.I. B.A.
The Very Rev. R. W. Church, M.A. Dean of St. Paul's.
Lionel Lawson, Esq.
Joseph Macpherson, Esq.
W. H. Preece, Esq. M. Inst. C.E.
Arnold George Rogers, Esq.
John Topham, M.D.
William Parkinson Wright, Esq.

were *elected* Members of the Royal Institution.

The Special Thanks of the Members were returned to Sir Wm. R. Grove for his Donation of Fifteen Guineas to provide a marble pedestal for the Bust of the Rev. John Barlow, presented by him in November, 1874.

The following Arrangements of the Lectures before Easter, 1876, were announced:—

Professor Tyndall, D.C.L. LL.D. F.R.S.—Six Lectures, adapted to a Juvenile Auditory, on Experimental Electricity; on Dec. 28 (Tuesday), 30, 1875; Jan. 1, 4, 6, and 8, 1876.

Professor Alfred H. Garrod—Twelve Lectures on the Classification of Vertebrated Animals; on Tuesdays, Jan. 18 to April 4.

Professor Gladstone, F.R.S.—Eight Lectures on the Chemistry of the Non-Metallic Elements; on Thursdays, Jan. 20 to March 9.

William Spottiswoode, Esq. LL.D. Treas. R.S. Sec. R.I.—Four Lectures on Polarized Light; on Thursdays, March 16 to April 6.

R. P. Pullan, Esq. M.R.I.B.A.—Three Lectures on his Excavations in Asia Minor; on Saturdays, Jan. 22, 29, and Feb. 5.

W. T. Thiselton Dyer, M.A. B.Sc. F.L.S. Assistant Director, Royal Gardens, Kew.—Four Lectures on the Vegetable Kingdom; the Boundaries and Connections of its Larger Groups; on Saturdays, Feb. 12 to March 4.

Professor G. Croom Robertson, M.A.—Three Lectures on the Human Senses; on Saturdays, March 11, 18, and 25.

Edward Dannreuther, Esq.—Two Lectures on Wagner and his Trilogy, on Saturdays, April 1 and 8.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The French Government—Documents Inédits sur l'Histoire de France: Cartulaires de l'Eglise Cathédrale de Grenoble. 4to. 1869.

Lords Commissioners of the Admiralty—Nautical Almanac for 1879. 8vo. 1875.

Agricultural Society, Royal—Journal, Second Series, Vol. XII. Part 2. 8vo. 1875.

Arnold, Thomas James, Esq.—Treatise on the Law of Municipal Corporations. 2nd ed. with Additions. By S. G. Johnston. 8vo. 1875.

Asiatic Society of Bengal—Journal, 1875, Part 1. No. 2. 8vo. Part 2. Extra Number. 8vo. 1875.

Proceedings, 1875, Nos. 6, 7, 8. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. XXXV. No. 9. 8vo. 1875.

Bavarian Academy, Royal—Sitzungsberichte. 1875. Heft 2. 8vo.

British Architects, Royal Institute of—Sessional Papers, 1875–6, No. 1. 4to.

British Museum Trustees—Catalogue of Oriental Coins, Vol. I. 8vo. 1875.

Cuneiform Inscriptions of Western Asia, Vol. IV. fol. 1875.

Comitato Geologico d'Italia—Bollettini, 1875, Nos. 7, 8. 8vo.

Devonshire Association for the Advancement of Literature, Science, and Art—Report and Transactions, Vol. VII. 8vo. 1875.

Douglas, R. K. Esq. (the Author)—The Language and Literature of China. (Two Lectures at the Royal Institution.) 16to. 1875.

Editors—American Journal of Science for Nov., 1875. 8vo.

Athenæum for Nov., 1875. 4to.

Chemical News for Nov., 1875. 4to.

Electrical News for Nov., 1875.

Engineer for Nov., 1875. fol.

Journal for Applied Science for Nov., 1875. fol.

Nature for Nov., 1875. 4to.

Nautical Magazine for Nov., 1875. 8vo.

Pharmaceutical Journal for Nov., 1875. 8vo.

Practical Magazine for Nov., 1875. 8vo.

Telegraph Journal for Nov., 1875. 8vo.

Franklin Institute—Journal, No. 598. 8vo. 1875.

Genève, Société de Physique—Mémoires, Vol. XXIV. Partie 1. 4to. 1874–5.

Geological Society—Quarterly Journal, No. 124. 8vo. 1875.

Geological Society of Ireland, Royal—Journal, Vol. IV. Part 2. 8vo. 1875.

Glasgow Philosophical Society—Proceedings, Vol. IX. No. 1. 8vo. 1874.

Harrison, Mr. W. H. (the Author)—Spirit People. 12mo. 1875.

Linnean Society—Proceedings, 1874–5. 8vo.

Liverpool Literary and Philosophical Society—Proceedings, No. 29. 8vo. 1875.

Meteorological Society—Quarterly Journal, New Series, No. 16. 8vo. 1875.

Planté, M. G. (the Author)—Recherches (from Comptes Rendus, 1875).

Preussische Akademie der Wissenschaften—Monatsberichte: Juli, Aug., 1875. 8vo.

Rigg, Rev. Arthur, M.A. M.R.I. (the Author)—Cantor Lectures. On Mechanism; on the Energies of Gravity, Vitality, Heat, &c.; and on Tools, &c. 8vo. 1872-3-5.

Report on Stove Competition. 8vo. 1874.

Royal Society of London—Philosophical Transactions for 1875. Vol. CLXV., Part 1. 4to. 1875.

Smee, A. H. Esq. (the Author)—Milk in Health and Disease. 12mo. 1875.

Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, Nov., 1875. 8vo.

Victoria Institute—Transactions, No. 35. 8vo. 1875.

Volpicelli, Professor P. (the Author)—Sull' Epoca della Completa Cecità del Galilei. 8vo. 1868.

Various Memoirs (from "Atti dei Nuovi Lincei di Roma," &c.). 1844–75. 4to.

Soluzione del Problema relative alle Corse del Cavallo. 4to. Roma. 1872.

Watford Natural History Society—Transactions, Vol. I. Parts 1, 2. 8vo. 1875.

WEEKLY EVENING MEETING,

Friday, January 21, 1876.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

Professor Tyndall, D.C.L. LL.D. F.R.S. M.R.I.

The Optical Condition of the Atmosphere, in its Bearings on Putrefaction and Infection.[*]

Six years ago I ventured to place before the Members of the Royal Institution a sketch of the Germ Theory of Epidemics, which, under the title of "Dust and Disease," was published in the Proceedings of the Institution.[†] I at that time found that London air, which is always thick with motes, and also with matter too fine to be described as motes, after it had been filtered by passing it through densely packed cotton-wool, or calcined by passing it through a red-hot platinum tube containing a bundle of red-hot platinum wires, or by carefully leading it over the top of a spirit-lamp flame, showed, when examined by a concentrated luminous beam, no trace of mechanically suspended matter. The particular portion of space occupied by such a beam was not to be distinguished from adjacent space.

The purely gaseous portion of our atmosphere was thus shown to be incompetent to scatter light.

I subsequently found that, to render the air thus optically pure, it was only necessary to leave it to itself for a sufficient time in a closed chamber, or in a suitably closed vessel. The floating matter gradually attached itself to the surrounding surfaces, leaving behind it air possessing no scattering power. Sent through such air, the most concentrated beam failed to render its track visible.

The parallelism of these results with those obtained in the excellent researches of Schwann, Schroeder and Dusch, Schroeder himself, and of the illustrious Pasteur, in regard to the question of "spontaneous generation," caused me to conclude that the power of scattering light and the power of producing life by the air would be found to go hand-in-hand.

This conclusion was strengthened by an experiment easily made and of high significance in relation to this question. It had been pointed out by Professor Lister, of Edinburgh, that air which has passed through the lungs is known to have lost its power of causing putrefaction. Such air may mix freely with the blood without risk of mischief; and that truly great scientific surgeon had the penetration to ascribe this immunity from danger to the filtering power of the lungs. Prior to my becoming acquainted with this hypothesis in 1869, I had virtually demonstrated its accuracy in the following way:

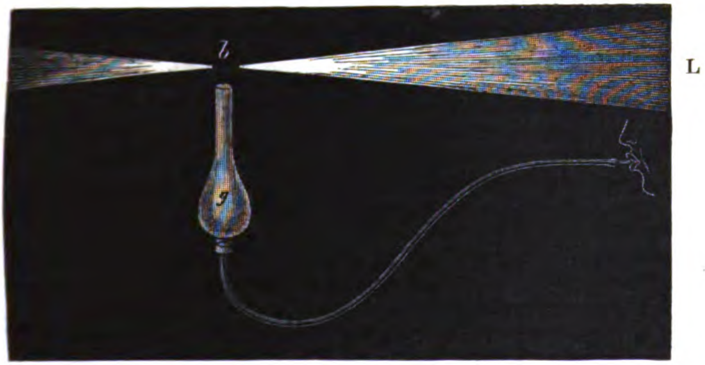
---

[*] Since the delivery of the discourse some new matter, dealing with questions subsequently discussed, has been added to this abstract.

[†] Vol. vi. p. 1.

Condensing in a dark room, and in dusty air, a powerful beam of light, and breathing through a glass tube (the tube actually employed was a lamp-glass, rendered warm in a flame to prevent precipitation) across the focus, a diminution of the scattered light was first observed. But towards the end of the expiration the white track of the beam was broken by a perfectly black gap, the blackness being due to the total absence from the expired air of any matter competent to scatter light. The deeper portions of the lungs were thus proved to be filled with optically pure air, which, as such, had no power to generate the organisms essential to the process of putrefaction.* The experimental arrangement is given in Fig. 1, where *g* represents the heated lamp-glass, and *b* the gap cut out of the beam issuing from the lamp at L.

FIG. 1.



I thought this simple method of examination could not fail to be of use to workers in this entangled field. They had hitherto proceeded less by sight than by insight, being in general unable to see the physical character of the medium in which their experiments were conducted. But the method has not been much turned to account, and this year I was so impressed by its possible importance, that I resolved to devote some time myself to the more complete demonstration of its utility.

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* "No putrefaction," says Cohn, "can occur in a nitrogenous substance if it be kept free from the entrance of new *Bacteria* after those which it may contain have been destroyed. Putrefaction begins as soon as *Bacteria*, even in the smallest numbers, are accidentally or purposely introduced. It progresses in direct proportion to the multiplication of the *Bacteria;* it is retarded when the *Bacteria* (for example, by a low temperature) develop a small amount of vitality, and is brought to an end by all influences which either stop the development of the *Bacteria* or kill them. All bactericidal media are therefore antiseptic and disinfecting."—Beiträge zur Biologie der Pflanzen. Zweites Heft, 1872, p. 203.

My principal stimulus, however, was the desire to free my mind, and if possible the minds of others, from the uncertainty and confusion which now beset the doctrine of "spontaneous generation." Pasteur has pronounced it "a chimera," and expressed the undoubting conviction that, this being so, it is possible to remove parasitic diseases from the earth. To the medical profession, therefore, and through them to humanity at large, this question, if the illustrious French philosopher be correct, is one of the last importance. But the state of medical opinion regarding it is not satisfactory. In a recent number of the 'British Medical Journal,' and in answer to the question, "In what way is contagium generated and communicated?" Messrs. Braidwood and Vacher reply that, notwithstanding "an almost incalculable amount of patient labour, the actual results obtained, especially as regards the manner of generation of contagium, have been most disappointing. Observers are even yet at variance whether these minute particles, whose discovery we have just noticed, and other disease germs, are always produced from like bodies previously existing, or whether they do not, under certain favourable conditions, spring into existence *de novo*."

With a view to the possible diminution of the uncertainty thus described, I have recently submitted to the Royal Society, and more especially to those who study the ætiology of disease, the following description of the mode of procedure followed in this inquiry, and of the results to which it has led.

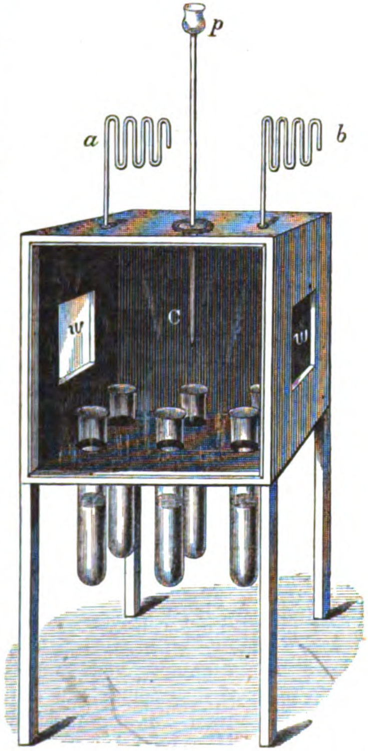
A number of chambers, or cases, were constructed, each with a glass front, its top, bottom, back and sides being of wood. At the back is a little door which opens and closes on hinges, while into the sides are inserted two panes of glass, facing each other. The top is perforated in the middle by a hole 2 inches in diameter, closed air-tight by a sheet of indiarubber. This sheet is pierced in the middle by a pin, and through the pin-hole is passed the shank of a long pipette ending above in a small funnel. A circular tin collar 2 inches in diameter and 1½ inch deep, surrounds the pipette, the space between both being packed with cotton-wool moistened by glycerine. Thus the pipette, in moving up and down, is not only firmly clasped by the indiarubber, but it also passes through a stuffing box of sticky cotton-wool. The width of the aperture closed by the indiarubber secures the free lateral play of the lower end of the pipette. Into two other smaller apertures in the top of the chamber are inserted, air-tight, the open ends of two narrow tubes, intended to connect the interior space with the atmosphere. The tubes are bent several times up and down, so as to intercept and retain the particles, carried by such feeble currents as changes of temperature might cause to set in between the outer and the inner air.

The bottom of the box is pierced sometimes with two rows, sometimes with a single row of apertures, in which are fixed, air-tight large test-tubes, intended to contain the liquid to be exposed to the action of the moteless air. The cases have varied in capacity from 1666 to 451 cubic inches. In the largest case twelve test-tubes were fixed, in the smallest three.

The arrangement for a case of six tubes is represented in Fig. 2, where *w*, *w*, are the windows through which the searching beam passes from the lamp outside; *p* is the pipette, and *a b* are the bent tubes connecting the inner with the outer air. The test-tubes passing through the bottom of the case are seen below.

On September 10th, 1875, the first case of this kind was closed. The passage of a concentrated beam across it through its two side windows then showed the air within it to be laden with floating matter. On the 13th it was again examined. Before the beam entered, and after it quitted the case, its track was vivid in the air, but within the case it vanished. Three days of quiet sufficed to cause all the floating matter to be deposited on the top, sides, and bottom, where it was retained by a coating of glycerine, with which the interior surface of the case had been purposely varnished. The test-tubes were then filled through the pipette, boiled for five minutes in a bath of brine or oil, and abandoned to the action of the moteless air. During dilution aqueous vapour rose from the liquid into the chamber, where it was for the most part condensed, the uncondensed portion escaping, at a low temperature, through the bent tubes at the top. Before the brine was removed, little stoppers of cotton-wool were inserted in the bent tubes, lest the entrance of the air into the cooling chamber should at first be forcible enough to carry motes along with it. As

Fig. 2.



soon, however, as the ambient temperature was assumed by the air within the case, the cotton-wool stoppers were removed.

We have here the oxygen, nitrogen, carbonic acid, ammonia, aqueous vapour, and all the other gaseous matters which mingle more or less with the air of a great city. We have them, moreover, "untortured" by calcination, and unchanged even by filtration or manipulation of any kind. The question now before us is, can air thus retaining all its gaseous mixtures, but self-cleansed from mechanically suspended matter, produce putrefaction in organic infusions freely exposed to its action? To this question both the animal and vegetable worlds return a decided negative.

Among vegetables experiments have been made with hay, turnips, tea, coffee, hops, repeated in various ways with both acid and alkaline infusions. Among animal substances are to be mentioned beef, mutton, pork, hare, rabbit, kidney, liver, fowl, pheasant, grouse, haddock, sole, salmon, cod, turbot, mullet, herring, whiting, eel, oyster, urine, which have been all subjected to repeated experiments.

The result is that infusions of these substances exposed to the common air of the Royal Institution laboratory, maintained at a temperature of from 60° to 70° Fahr., all fell into putrefaction in the course of from two to four days. No matter where the infusions were placed, they were infallibly smitten in the end. The number of the tubes containing infusions was multiplied till it reached 600, but not one of them escaped infection.

In no single instance, on the other hand, did the air, which had been proved moteless by the searching beam, even when raised to over 90°, manifest the least power of producing Bacterial life or the associated phenomena of putrefaction.* The power of developing such life in atmospheric air, and the power of scattering light, are thus proved to be indissolubly united.

The sole condition necessary to cause these long-dormant infusions to swarm with active life is the access of the floating matter of the air. After having remained for four months as pellucid as distilled water, the opening of the back door of the protecting case, and the consequent admission of the mote-laden air, sufficed in three days to render the infusions putrid and full of life.

In his published works, Dr. Bastian has frequently dwelt upon the necessity of employing strong infusions when investigating the phenomena of spontaneous generation. I would therefore refer to the fact, that in most of the experiments here described the infusion at starting was strong, and that it was permitted to evaporate with extreme slowness through the bent tubes at the top of the case, until its concentration became three or four fold what it had been at starting. Every experiment was thus converted into an indefinite number of experiments on infusions of different strengths. Never, in my opinion, was the requirement as to concentration more completely fulfilled, and never was the reply of Nature to experiment more definite and satisfactory. The temperatures, moreover, to which the infusions have been subjected, embrace those hitherto found effectual, extending indeed beyond them in both directions.* They reached from a lower limit of 50° to a higher limit of more than 100° Fahr. Still higher temperatures were applied in other experiments to be described subsequently. With regard to the number of the infusions, more than fifty moteless chambers, each with its system of tubes, have been tested. There is no shade of uncertainty in any of the results. In every instance we have, within the chamber, perfect limpidity and sweetness—without the chamber, putridity and its characteristic smells. In no instance is the least countenance lent to the notion that an infusion deprived by heat of its inherent life, and placed in contact with air cleansed of its visibly suspended matter, has any power whatever to generate life anew.

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* The temperatures, in some cases, reached 105° Fahr.

If it should be asked how I have assured myself that the protected liquids do not contain Bacteria, I would, in the first place, reply that with the most careful microscopic search I have been unable to find them. But much more than this may be affirmed. The electric or the solar beam is a far more powerful and searching test in this matter than the microscope. In the foregoing pages I have more than once described the clearness of my protected infusions, after months of exposure, as equal to that of distilled water. So far is this from being an exaggeration, that it falls short of the truth; for I have never seen distilled water so free from suspended particles as the protected infusions prove themselves to be. When for months a transparent liquid thus defies the scrutiny of the searching beam, maintaining itself free from every speck which could scatter light as a Bacterium scatters it—when, moreover, an adjacent infusion, prepared in precisely the same way, but exposed to the ordinary air, becomes first hazy, then turbid, and ends by wholly shattering the concentrated beam into irregularly scattered light, I think we are entitled to conclude that Bacteria are as certainly absent from the one as they are present in the other.

For the right interpretation of scientific evidence something more than mere sharpness of observation is requisite, very keen sight being perfectly compatible with very weak insight. I was therefore careful to have my infusions inspected by biologists, not only trained in the niceties of the microscope, but versed in all the processes of scientific reasoning. Their conclusion is that it would simply weaken the demonstrative force of the experiments to appeal to the microscope at all.

That Bacterial life arises from mechanically suspended particles is thus reduced to ocular demonstration. Let us inquire a little more closely into the character of the particles which produce the life. Pour Eau de Cologne into water, a white precipitate renders the liquid milky. Or, imitating Brücke, dissolve clean gum mastic in alcohol, and drop it into water, the mastic is precipitated, and milkiness produced. If the solution be very strong, the mastic separates in curds; but by gradually diluting the alcoholic solution we finally reach a point where the milkiness disappears, the liquid assuming, by reflected light, a bright cerulean hue. It is then, in point of fact, the colour of the sky, being due to a similar cause, namely, the scattering of light by particles, small in comparison to the size of the waves of light.

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* See 'Proc. Roy. Soc.' vol. xxi. p. 130, where a temperature of 70° is described as effectual.

When this liquid is examined by the highest microscopic power, it seems as uniform as distilled water. The mastic particles, though innumerable, entirely elude the microscope.* At right angles to a luminous beam passing among the particles they discharge perfectly polarized light. The optical deportment of the floating matter of the air proves it to be composed, in part, of particles of this excessively minute character. When the track of a parallel beam in dusty air is looked at horizontally through a Nicol's prism, in a direction perpendicular to the beam, the longer diagonal of the prism being vertical, a considerable portion of the light from the finer matter is extinguished. The coarser motes, on the other hand, flash out with greater force, because of the increased darkness of the space around them. It is among the finest ultra-microscopic particles that the matter potential as regards the development of Bacterial life is to be sought.

The existence of these particles, foreign to the atmosphere, but floating in it, is as certain as if they could be felt between the fingers, or seen by the naked eye. Supposing them to augment in magnitude until they come, not only within range of the microscope, but within range of the unaided senses. Let it be assumed that our knowledge of them under these circumstances remains as defective as it is now—that we do not know whether they are germs, particles of dead organic dust, or particles of mineral matter. Suppose a vessel (say a flower-pot) to be at hand filled with nutritious earth, with which we mix our unknown particles; and that in three days buds and blades of well-defined cresses and grasses appear above the soil. Suppose the experiment when repeated a hundred times to yield the same unvarying result. What would be our conclusion? Should we regard those living plants as the products of dead dust or mineral particles? or should we regard them as the offspring of living seeds? The reply is unavoidable. We should undoubtedly consider the experiment with the flower-pot as clearing up our pre-existing ignorance; we should regard the fact of their producing cresses and grasses as proof positive that the particles sown in the earth of the pot were the seeds of the plants which have grown from them. It would be simply monstrous to conclude that they had been "spontaneously generated."

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* Mr. Dallinger has failed to detect them with a magnifying power of fifteen thousand diameters.—'Pop. Sci. Rev.,' April 1876.

This reasoning applies word for word to the development of Bacteria from that floating matter which the electric beam reveals in the air, and in the absence of which no life has been generated. There seems no flaw in this reasoning; and it is so simple as to render it unlikely that the notion of Bacterial life developed from dead dust can ever gain currency among the members of the medical profession.

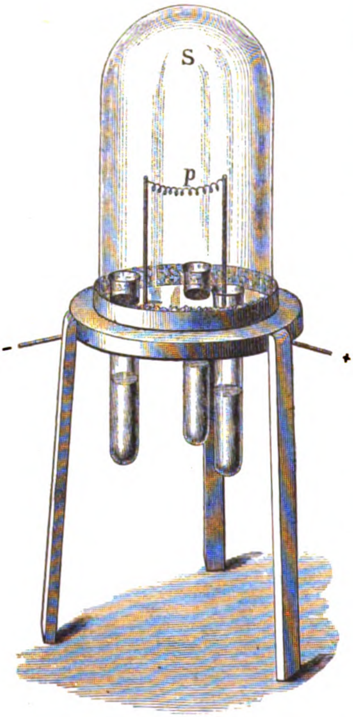
The uniform sterility of the boiled infusions thus far described, when protected from the floating matter of the air, proves that they do not contain germs capable of generating life. Dr. Bastian indeed affirms that a temperature of 140° Fahr. reduces, in all cases, such germs to a state of actual or potential death. But even in flasks which have been raised to a temperature of 212°, and hermetically sealed, putrefaction, and its associated Bacterial life, do, he alleges, most certainly arise; from which he infers that Bacteria are spontaneously generated. "We know," he says, "that boiled turnip or hay-infusions, exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with Bacteria and vibriones in the course of from two to six days."

We are here met by a difficulty at the outset. Dr. Bastian's proof of Bacterial death at 140° Fahr. consists in the observed fact, that when a certain liquid is heated to that temperature no life appears in it afterwards. In another liquid, however, he finds that life appears two days after it has been heated to 212°. Instead of concluding that in the one liquid the germs are killed and in the other not, he chooses to assume that 140° Fahr. is the death-temperature for both, and this being so, the life observed in the second liquid figures, in his inference, as a case of spontaneous generation. A great deal of Dr. Bastian's most cogent reasoning rests upon this foundation. Assumptions of this kind guide him in his most serious experiments. He finds, for example, that a mineral solution does not develop Bacteria when exposed to the air; and he concludes from this that an organic infusion also may be thus exposed without danger of infection. He exposes turnip-juice accordingly, obtains a crop of Bacteria, which, in the light of his assumption, are spontaneously generated. Such are the warp and woof of some of the weightiest arguments on this question which have been addressed to the Royal Society.

Granting, then, all that Dr. Bastian alleges regarding his experiments to be correct, the logical inference would be very different from his inference. But are his statements correct? This is the really important point, and to its examination I now address myself.

And first, with regard to filtered air. A group of twelve large test-tubes were caused to pass air-tight through a slab of wood. The wood was coated with cement, in which, while hot, a heated "propagating glass" resembling a large bell-jar was imbedded. The air within the jar was pumped out several times, air filtered through a plug of cotton-wool being permitted to supply its place. The test-tubes contained infusions of hay, turnip, beef, and mutton—three of each—twelve in all. After months of exposure they remained as clear and cloudless as they were upon the day of their introduction; while twelve similar tubes, prepared at the same time in precisely the same way and exposed to the ordinary air, were, in a few days, clogged with mycelium, mould, and Bacteria.

FIG. 3.



With regard to the calcined air, a similar propagating glass was caused to cover twelve other tubes filled with the same infusions. The "glass" was exhausted and carefully filled with air which had been slowly passed through a red-hot platinum tube, containing a roll of red-hot platinum gauze. Tested by the searching beam, the calcined air was found quite free from floating matter. Not a speck has invaded the limpidity of the infusions exposed to it, while twelve similar tubes placed outside have fallen into rottenness.

The experiments with calcined air took another form. Six years ago* I found that to render the laboratory air free from floating matter, it was only necessary to permit a platinum wire heated to whiteness to act upon it for a sufficient time. I availed myself of this mode of calcining the air on the present occasion. The apparatus employed is shown in Fig. 3. A glass shade S is placed upon a slab of wood mounted on a tripod, and through which passes three large test-tubes nearly filled with the infusion to be examined. A platinum spiral *p* unites the ends of two upright copper wires, which pass through the stand, and are marked + and − outside it. The shade is surrounded by a tin collar, with a space of about half an inch all round between it and the shade. This space is filled with cotton-wool firmly packed. Connecting the wires with a battery of fifteen cells, the spiral *p* was raised to whiteness, and was permitted to continue so for five minutes. Experiments previously executed had shown that this sufficed for the entire removal of the floating matter. When the spiral was heated a portion of the expanded air was driven through the cotton-wool packing below; and when the current was interrupted this air, returning into the shade, was prevented by the cotton-wool from carrying any floating matter with it.

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* 'Proc. Roy. Inst.' vol. vi. pp. 4 and 5.

The first three substances brought into contact with air calcined in this way were damson-juice, pear-juice, and infusion of yeast. They were boiled for five minutes, and for five months they have remained without speck or turbidity. Other tubes similarly boiled, and placed underneath shades containing the floating matter of the air, have long since fallen into mould and rottenness.

Turnip and hay-infusions rendered slightly alkaline have been mentioned as particularly prone to spontaneous generation. I wished to test this. On the 26th of November, therefore, four shades were prepared, two containing strong turnip-infusion and hay-infusion unneutralized, two containing infusions which had been rendered slightly superneutralized by caustic potash. The alleged spontaneous development of life was not observed. The tubes exhibit to this hour the clearness and colour which they showed on the day they were boiled. Hermetically sealed tubes, containing the same infusions, prepared on the same day, remain equally clear; while specimens exposed to the laboratory air have fallen into rottenness.

Finally, with regard to infusions wholly withdrawn from air, a group of test-tubes, containing different infusions, was boiled under a bell-jar first filled with filtered air, which was subsequently removed as far as possible by a good air-pump. They are now as pellucid as they were at the time of their preparation, more than three months ago, while a group of corresponding tubes exposed to the laboratory air have all fallen into rottenness.

On another form of experiment just adverted to, great weight has been laid—that of hermetically sealed tubes. On April 6th, 1875, a discussion on the "Germ Theory of Disease" was opened before the Pathological Society of London. The meeting was attended by many distinguished medical men, some of whom were profoundly influenced by the arguments, and none of whom disputed the facts brought forward against the theory on that occasion. The following important summary of these was then given by Dr. Bastian:—"With the view of settling these questions, therefore, we may carefully prepare an infusion from some animal tissue, be it muscle, kidney, or liver; we may place it in a flask whose neck is drawn out and narrowed in the blowpipe-flame, we may boil the fluid, seal the vessel during ebullition, and keeping it in a warm place, may await the result, as I have often done. After a variable time the previously heated fluid within the hermetically sealed flask swarms more or less plentifully with Bacteria and allied organisms."

Previous to reading this statement I had operated upon sixteen tubes of hay and turnip-infusions, and upon twenty-one tubes of beef, mackerel, eel, oyster, oatmeal, malt, and potato, hermetically sealed while boiling, not by the blowpipe, but by the far more handy spirit-lamp flame. In no case was any appearance whatever of Bacteria or allied organisms observed. The perusal of the discussion just referred to caused me to turn again to muscle, liver, and kidney, with a view of varying and multiplying the evidence. Fowl, pheasant, snipe, partridge, plover, wild duck, beef, mutton, heart, tongue, lungs, brains, sweetbread, tripe, the crystalline lens, vitreous humour, herring, haddock, mullet, codfish, sole, were all embraced in the experiments. There was neither mistake nor ambiguity about the result. On January 13th one hundred and thirty-nine of the flasks operated on were submitted to the Fellows of the Royal Society, and not one of this cloud of witnesses offered the least countenance to the assertion that liquids within flasks, boiled and hermetically sealed, swarm, subsequently, more or less plentifully with Bacteria and allied organisms.

In connection with these experiments, I have sought, to the best of my ability, to meet every condition and requirement laid down by others as essential to success. With regard to the question of temperature, 90° were generally attainable in our laboratory, while on certain days of mild weather without, and in favourable positions within, the temperature to which the infusions were subjected reached over 100° Fahr. As Dr. Bastian, however, has recently laid considerable stress on temperature, though most of his results were obtained with temperatures from 15° to 30° lower than mine,* I thought it desirable to meet this new requirement also. The sealed tubes, which had proved barren in the Royal Institution, were suspended in boxes copiously perforated, so as to permit of the free circulation of warm air, and placed under the supervision of an intelligent assistant in the Turkish Bath in Jermyn Street. The washing room of the establishment was found to be particularly suitable for our purpose; and here, accordingly, the boxes were suspended. From two to six days are allowed by Dr. Bastian for the generation of organisms in hermetically sealed tubes. Mine remained in the washing room for nine days. Thermometers placed in the boxes, and read off twice or three times a day, showed the temperature to vary from a minimum of 101° to a maximum of 112° Fahr. At the end of nine days the infusions were as clear as at the beginning.

They were then removed to another position where the temperature was a few degrees higher. Dr. Bastian mentions 115° as favourable to spontaneous generation. For fourteen days the temperature hovered about this point, falling once as low as 106°, reaching 116° on three occasions, 118° on one, and 119° on two.

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* 'Proc. Roy. Soc.' vol. xxi. p. 130. Also 'Beginnings of Life,' vol. i. p. 354.

The result was quite the same as that recorded a moment ago. The higher temperatures proved perfectly incompetent to develop life.*

Fifty-six observations, including both the maximum and minimum thermometers, were taken while the tubes occupied their first position in the washing room, and seventy-four while they occupied the second position. The whole record, carefully drawn out, is before me, but I trust the statement of the major and minor limits of temperature will suffice.

Dr. Bastian's demand for these high temperatures is, as already remarked, quite recent. Prior to my communication to the Royal Society on January 13, he had successfully worked with temperatures lower than those within my reach in Albemarle Street. There I followed his directions, adhered strictly to his prescriptions, but, taking care to boil and seal my liquids aright, his results refused to appear in my experiments. On learning this, he raised an objection as to temperature, and made a new demand. With this I have complied, but his position is unimproved.

With regard to the question of concentration, which was always brought into prominence, I have already referred to the great diversity in this particular presented by all my infusions, through their slow evaporation. But more than a general conformity to prescribed conditions was observed here also. The strength of an infusion is regarded as fixed by its specific gravity, and I have worked with infusions of precisely the same specific gravity as those employed by Dr. Bastian. This I was specially careful to do in relation to the experiments described and vouched for, I fear incautiously, by Dr. Burdon Sanderson in vol. vii. p. 180 of 'Nature.' It will there be seen that though failure attended some of his efforts, Dr. Bastian did satisfy Dr. Sanderson that in boiled and hermetically sealed flasks Bacteria sometimes appear in swarms. With purely liquid infusions I have vainly sought to reproduce the evidence which convinced Dr. Sanderson. Hay and turnip-infusions, of accurately the same character and strength as those employed on the occasion referred to, were prepared, boiled in an oil bath, carefully sealed up, and subjected to the proper temperatures. In multiplied experiments they remained uniformly sterile. I am therefore compelled to infer that the instances in which Dr. Bastian failed to obtain Bacteria in his hermetically sealed tubes were illustrations of correct experimenting, the appearance of life in other cases being the result of errors of manipulation.

The evidence furnished by this mass of experiments, that Dr. Bastian must have permitted errors either of preparation or observation to invade his work is, I submit, very strong. But to err is human; and in an inquiry so difficult and fraught with such momentous issues, it is not error, but the persistence in error by any of us, for dialectic ends, that is to be deprecated. Let me here show, by one or two illustrations, the risks of error to which I have been exposed. On October 21st I opened the back door of a case containing six test-tubes filled with an infusion of turnip which had remained perfectly clear for three weeks, while three days sufficed to crowd six similar tubes exposed to mote-laden air with Bacteria. With a small pipette I took specimens from the pellucid tubes, and placed them under the microscope. The first tube examined showed no signs of life. This was the result expected; but I was by no means prepared for the deportment of the second tube. Here the exhibition of life was monstrously copious. There were numerous globular organisms, which revolved, rotated, and quivered in the most extraordinary manner. There were also numbers of lively Bacteria darting to and fro. An experimenter who ponders his work and reaches his conclusions slowly cannot immediately relinquish them; and in the present instance some time was required to convince me that I had made no mistake. I could find none, and was prepared to accept the conclusion that in the boiled infusion, despite its clearness, life had appeared.

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* My thanks are due to the managers of the bath for their obliging kindness in this matter.

But in a protected turnip-infusion, which had been examined on October 13th, no trace of life could be found. Perfect transparency was then accompanied by an utter absence of life. Indeed, the selfsame action upon light that enabled the Bacteria to show themselves in the microscope must, one would think, infallibly produce turbidity. Why, moreover, should life be absent from the first member of the present group of tubes? I searched this again, and found in it scanty but certain signs of life. This augmented my perplexity. A third tube also showed scanty traces of life. Reverting to the second tube, where life had been so copious, I found that in it the organisms had become as scanty as in the others. I confined myself for a time to the three tubes of the first row of the six, going over them again and again; sometimes finding an organism here and there, but sometimes finding nothing. The first extraordinary exhibition of life it was found impossible to restore. In my difficulty I took specimens from the three tubes, and sent them to Professor Huxley, with a request that he would be good enough to examine them.

On the 22nd my search was extended to the whole of the tubes. Early in the day lively Bacteria were found in one of them; later on, not one of the six yielded to my closest scrutiny any trace of life. On the evening of the 22nd I received a note from Mr. Huxley, stating that a careful examination of the specimens sent to him revealed no living thing.

Pipettes had been employed to remove the solution from the test-tubes. They were short pieces of narrow glass tubing, drawn out to a point, with a few inches of indiarubber tubing attached to them. This was found convenient for bending, so as to reach the bottom of the test-tubes. Suspicion fell upon this indiarubber. I washed it, and examined the washing water, but found no life. Distilled water had been used to cleanse the pipettes, and on the morning of the 23rd I entered the laboratory intending to examine it. Before dipping the pipette into the water I looked at its point. The tiniest drop had remained in it by capillary attraction from the preceding day. This I blew on to a slide, covered it, and placed it under the microscope. An astonishing exhibition of life was my reward. Thus on the scent, I looked through my pipettes, and found two more with the smallest residual drops at the ends; both of them yielded a field rampant with life. The Bacteria darted in straight lines to and fro, bending right and left along the line of motion, wriggling, rotating longitudinally, and spinning round a vertical transverse axis. Monads also galloped and quivered through the field. From one of these tiny specks of liquid I obtained an exhibition of life not to be distinguished from that which had astonished me on the 21st.

Obviously the phenomenon then observed was due to the employment of an unclean pipette. Equally obvious is it that in inquiries of this nature the experimenter is beset with danger, the grossest errors being possible when there is the least lack of care.

Three tubes containing infusions of turnip, hay, and mutton, were boiled on November 2nd under a jar-bell containing air so carefully filtered that the most searching examination by a concentrated beam failed to reveal a particle of floating matter. At the present time every one of the tubes is thick with mycelium and covered with mould. Here surely we have a case of spontaneous generation. Let us look to its history.

After the air has been expelled from a boiling liquid it is difficult to continue the ebullition without "bumping." The liquid remains still for intervals, and then rises with sudden energy. It did so in the case now under consideration, and one of the tubes boiled over, the liquid overspreading the resinous surface in which the bell-jar was imbedded. For three weeks the infusions had remained perfectly clear. At the end of this time, with a view of renewing the air of the jar, it was exhausted, and refilled by fresh air which had passed through a plug of cotton-wool. As the air entered, two small spots of penicillium resting on the fluid which had boiled over attracted attention. I at once remarked that the experiment was a dangerous one, as the entering air would probably detach some of the spores of the penicillium and diffuse them in the bell-jar. This was, therefore, filled very slowly, so as to render the disturbance a minimum. Next day, however, a tuft of mycelium was observed at the bottom of one of the three tubes, namely, that containing the hay-infusion. It has by this time grown so as to fill a large portion of the tube. For nearly a month longer the two tubes containing the turnip and mutton-infusions maintained their transparency unimpaired. Late in December the mutton-infusion, which was in dangerous proximity to the outer mould, showed a tuft upon its surface. The turnip-infusion continued bright and clear for nearly a fortnight longer. The recent cold weather caused me to add a third gas-stove to the two which had previously warmed the room in which the experiments are conducted. The warmth played upon one side of the bell-jar, causing currents within it; and the day after the lighting of the stove the turnip-infusion gave birth to a tuft of mycelium. In this case the small spots of penicillium might have readily escaped attention; and had they done so we should have had three cases of "spontaneous generation" far more striking than many that have been adduced.

In further illustration of the danger incurred in this field of inquiry, I may refer to the excellent paper of Dr. Roberts on Biogenesis, in the 'Philosophical Transactions' for 1874. Dr. Roberts fills the bulb of an ordinary pipette up to about two-thirds of its capacity with the infusion to be examined. In the neck of the pipette he places a plug of dry cotton-wool. He then hermetically seals the neck and dips the bulb into boiling water or hot oil, where he permits it to remain for the requisite time. Here we have no disturbance from ebullition, and no loss by evaporation. The bulb is removed from the hot water and permitted to cool. The sealed end of the neck is then filed off, the cotton-wool alone interposing between the infusion and the atmosphere.

The arrangement is beautiful, but it has one weak point. Cotton-wool free from germs is not to be found, and the plug employed by Dr. Roberts infallibly contained them. In the gentle movement of the air to and fro as the temperature changed, or by any shock, jar, or motion to which the pipette might be subjected, we have certainly a cause sufficient to detach a germ now and then from the cotton-wool which, falling into the infusion, would produce its effect. Probably, also, condensation occurred at times in the neck of the pipette, the water of condensation carrying back from the cotton-wool the seeds of life. The fact of fertilization being so rare as Dr. Roberts found it to be is a proof of the care with which his experiments were conducted. But he did find cases of fertilization after prolonged exposure to the boiling temperature; and this caused him to come to the conclusion that under certain rare conditions spontaneous generation may occur. He also found that an alkalized hay-infusion was so difficult to sterilize that it was capable of withstanding the boiling temperature for hours without losing its power of generating life. Careful experiments have been made with this infusion. Dr. Roberts is certainly correct in assigning to it superior nutritive power. But in the present inquiry five minutes' boiling sufficed to completely sterilize the liquid.

I shall hardly be charged with any desire to limit the power and potency of matter in regard to life. But holding the opinions I do hold on this question, it is all the more incumbent on me to affirm that, as far as experimental inquiry has hitherto penetrated, life has never been proved to appear independently of antecedent life.

With regard to the general diffusion of germs in the atmosphere, the notions entertained by distinguished writers rendered it desirable to place the point beyond question. At Down, Mr. Darwin, and Mr. Francis Darwin; at High Elms, Sir John Lubbock; at Sherwood, near Tunbridge Wells, Mr. Siemens; at Pembroke Lodge, Richmond Park, Mr. Rollo Russell; at Heathfield Park, Miss Hamilton; at Greenwich Hospital, Mr. Hirst; at Kew, Dr. Hooker; and at the Crystal Palace, Mr. Price, kindly took charge of infusions, every one of which was invaded, many of them by astounding swarms of organisms.

To obtain more definite insight regarding the diffusion of atmospheric germs, a square wooden tray was pierced with one hundred holes, into each of which was dropped a short test-tube. On October 23rd, thirty of these tubes were filled with an infusion of hay, thirty-five with an infusion of turnip, and thirty-five with an infusion of beef. The tubes, with their infusions, had been previously boiled, ten at a time, in an oil-bath. One hundred circles were marked on paper so as to form a map of the tray, and every day the state of each tube was registered upon the corresponding circle. In the following description the term "cloudy" is used to denote the first stage of turbidity; distinct, but not strong. The term "muddy" is used to denote thick turbidity.

One tube of the hundred was first singled out and rendered muddy. It belonged to the beef group, and it was a whole day in advance of all the other tubes. The progress of putrefaction was first registered on October 26th; the "map" then taken may be thus described:

Hay.—Of the thirty specimens exposed, one had become "muddy" —the seventh in the middle row reckoning from the side of the tray nearest the stove. Six tubes remained perfectly clear between this muddy one and the stove, proving that differences of warmth may be overridden by other causes. Every one of the other tubes containing the hay-infusion showed spots of mould upon the clear liquid.

Turnip.—Four of the thirty-five tubes were very muddy, two of them being in the row next the stove, one four rows distant, and the remaining one seven rows away. Besides these, six tubes had become clouded. There was no mould on any of the tubes.

Beef.—One tube of the thirty-five was quite muddy, in the seventh row from the stove. There were three cloudy tubes, while seven of them bore spots of mould.

As a general rule, organic infusions exposed to the air during the autumn remained for two days or more perfectly clear. Doubtless from the first germs fell into them, but they required time to be hatched. This period of clearness may be called the "period of latency," and indeed it exactly corresponds with what is understood by this term in medicine. Towards the end of the period of latency, the fall into a state of disease is comparatively sudden; the infusion passing from perfect clearness to cloudiness more or less dense in a few hours.

Thus the tube placed in Mr. Darwin's possession was clear at

8.30 A.M. on October 19th, and cloudy at 4.30 P.M. Seven hours, moreover, after the first record of our tray of tubes, a marked change had occurred. Instead of one, eight of the tubes containing hay-infusion had fallen into uniform muddiness. Twenty others had produced Bacterial slime, which had sunk to the bottom, every tube containing the slime being covered by mould. Three tubes only remained clear, but with mould upon their surfaces. The muddy turnip-tubes had increased from four to ten; seven tubes were clouded, while eighteen of them remained clear, with here and there a speck of mould on the surface. Of the beef, six were cloudy and one thickly muddy, while spots of mould had formed in the majority of the remaining tubes. Fifteen hours subsequent to this observation, viz. on the morning of October 27th, all the tubes containing hay-infusion were smitten, though in different degrees, some of them being much more turbid than others. Of the turnip-tubes, three only remained unsmitten, and two of these had mould upon their surfaces. Only one of the thirty-five beef-infusion remained intact. A change of occupancy, moreover, had occurred in the tube which first gave way. Its muddiness remained grey for a day and a half, then it changed to bright yellow green, and it maintained this colour to the end. On the 27th every tube of the hundred was smitten, the majority with uniform turbidity; some, however, with mould above and slime below, the intermediate liquid being tolerably clear. The whole process bore a striking resemblance to the propagation of a plague among a population, the attacks being successive and of different degrees of virulence.

From the irregular manner in which the tubes are infected, we may infer that, as regards *quantity*, the distribution of the germs in the air is not uniform. The singling out, moreover, of one tube of the hundred by the particular Bacteria that develop a green pigment, shows that, as regards *quality*, the distribution is not uniform. The same absence of uniformity was manifested in the struggle for existence between the Bacteria and the penicillium. In some tubes the former was triumphant; in other tubes of the same infusion the latter was triumphant. It would seem also as if a want of uniformity as regards *vital vigour* prevailed. With the self-same infusion the motions of the Bacteria in some tubes were exceedingly languid, while in other tubes they resembled a rain of projectiles, being so rapid and violent as to be followed with difficulty by the eye. Reflecting on the whole of this, I conclude that the germs float through the atmosphere in groups or clouds, with spaces more sparsely filled between them. The touching of a nutritive fluid by a Bacterial cloud would naturally have a different effect from the touching of it by the interspace between two clouds. But as in the case of a mottled sky, the various portions of the landscape are successively visited by shade, so, in the long run, were the various tubes of the tray touched by the Bacterial clouds, the final fertilization or infection of them all being the consequence. These results connect themselves with the experiments of Pasteur on the non-continuity of the cause of so-called spontaneous generation, and with other experiments of my own.[*]

On the 9th of November a second tray containing one hundred tubes filled with an infusion of mutton was exposed to the air. On the morning of the 11th six of the ten nearest the stove had given way to putrefaction. Three, of the row most distant from the stove, had yielded, while here and there over the tray particular tubes were singled out and smitten by the infection. Of the whole tray of one hundred tubes, twenty-seven were either muddy or cloudy on the 11th. Thus, doubtless, in a contagious atmosphere, are individuals successively struck down. On the 12th all the tubes had given way, but the differences in their contents were extraordinary. All of them contained Bacteria, some few, others in swarms. In some tubes they were slow and sickly in their motions, in some apparently dead, while in others they darted about with rampant vigour. These differences are to be referred to differences in the germinal matter, for the same infusion was presented everywhere to the air. Here also we have a picture of what occurs during an epidemic, the difference in number and energy of the Bacterial swarms resembling the varying intensity of the disease. It becomes obvious from these experiments that of two individuals of the same population, exposed to a contagious atmosphere, the one may be severely, the other lightly attacked, though the two individuals may be as identical as regards susceptibility as two samples of one and the same mutton-infusion. Experiments with other trays are described in the full account of this investigation, and calculations are made which prove the error of the assertion that the germs are but scantily distributed through the air. There are billions of them in every ordinary London room.

The parallelism of these actions with the progress of infectious disease may be traced still further. The 'Times' of January 17th contained a letter on typhoid fever, signed "M.D.," in which occurred the following remarkable statement:—"In one part of it (Edinburgh), congregated together and inhabited by the lowest of the population, there are, according to the Corporation return for 1874, no less than 14,319 houses or dwellings—many under one roof, on the 'flat' system—in which there are no house connections whatever with the street sewers, and, consequently, no water-closets. To this day, therefore, all the excrementitious and other refuse of the inhabitants is collected in pails or pans, and remains in their midst, generally in a partitioned-off corner of the living room, until the next day, when it is taken down to the streets and emptied into the Corporation carts. Drunken and vicious though the population be, herded together like sheep, and with the filth collected and kept for twenty-four hours in their very midst, it is a remarkable fact that typhoid fever and diphtheria are simply unknown in these wretched hovels."

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[*] In hospital practice the opening of a wound during the passage of a Bacterial cloud would have an effect very different from the opening of it in the interspace between two clouds. Certain caprices in the behaviour of dressed wounds may possibly be accounted for in this way. Under the heading, "Nothing new under the Sun," Professor Huxley has just sent me the following remarkable extract:—"Uebrigens kann man sich die in der Atmosphäre schwimmenden Thierchen wie Wolken denken, mit denen ganz leere Luftmassen, ja ganze Tage völlig reinen Luftverhältnisse wechseln." (Ehrenberg, 'Infusions Thierchen,' 1838, p. 525.) The coincidence of phraseology is surprising, for I knew nothing of Ehrenberg's conception. My "clouds," however, are but small miniatures of his.

This case has its analogue in the following experiment, which is representative of a class. On November 30th a quantity of animal refuse, embracing beef, fish, rabbit, hare, was placed in two large test-tubes opening into a protecting chamber containing six tubes. On December 13th, when the refuse was in a state of noisome putrefaction, infusions of whiting, turnip, beef, and mutton were placed in the other four tubes. They were boiled and abandoned to the action of the foul "sewer gas" emitted by their two putrid companions. On Christmas Day the four infusions were limpid. The end of the pipette was then dipped into one of the putrid tubes, and a quantity of matter, comparable in smallness to the pock-lymph held on the point of a lancet, was transferred to the turnip. Its clearness was not sensibly affected at the time; but on the following day it was turbid throughout. On the 27th a speck from the infected turnip was transferred to the whiting; on the 28th disease had taken entire possession of the whiting. To the present hour the beef and mutton tubes remain as limpid as distilled water. Just as in the case of the living men and women in Edinburgh, no amount of fetid gas had the power of propagating the plague, as long as the organisms which constitute the true contagium did not gain access to the infusions.

The rapidity of development in an infusion infected by either a speck of liquid containing Bacteria or a drop of water is extraordinary. On January 4th a thread of glass almost as fine as a hair was dipped into a cloudy turnip-infusion, and the tip only of the glass fibre was introduced into a large test-tube containing an infusion of red mullet. Twelve hours subsequently the perfectly pellucid liquid was cloudy throughout. Precisely the same experiments were made with herring, with the same result. At this season of the year several days' exposure to the air are needed to produce the same effect. On December 31st a strong turnip-infusion was prepared by digesting in distilled water at a temperature of 120° Fahr. The infusion was divided between four large test-tubes, in one of which it was left unboiled, in another boiled for five minutes, and in the two remaining ones boiled, and after cooling infected with one drop of beef-infusion containing Bacteria. In twenty-four hours the unboiled tube and the two infected ones were cloudy, the unboiled tube being the most turbid of the three. The infusion here was peculiarly limpid after digestion; for turnip it was quite exceptional, and no amount of searching with the microscope sufficed to reveal in it at first the trace of a living Bacterium; still germs were there which, suitably nourished, passed in a single day into Bacterial swarms without number. Five days did not suffice to produce an effect approximately equal to this in the boiled tube, which was uninfected but exposed to the common laboratory air.

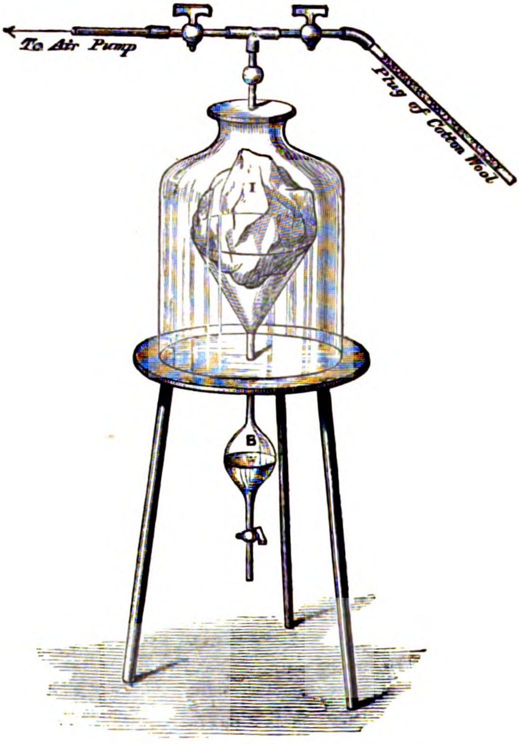
We are here brought face to face with a question of extreme importance, which it will be useful to clear up. "Potential germs" and "hypothetical germs" have been spoken of with scorn, because the evidence of the microscope as to their existence was not forthcoming. Sagacious writers had drawn from their experiments the perfectly legitimate inference that in many cases the germs exist, though the microscope fails to reveal them. Such inferences, however, have been treated as the pure work of the imagination, resting, it was alleged, on no real basis of fact. But in the concentrated beam we possess what is virtually a new instrument, exceeding the microscope indefinitely in power. Directing it upon media which refuse to give the coarser instrument any information as to what they hold in suspension, these media declare themselves to be crowded with particles—not hypothetical, not potential, but actual, and myriadfold in number—showing the microscopist that there is a world beyond even his range.

The infection of clear infusions by others containing visible Bacteria has been just referred to. But for the infection to be sure it is not necessary that the Bacteria should be visible. Over and over again I have repeated the experiments of Dr. Burdon Sanderson on the infective power of ordinary distilled water, in which the microscope fails to reveal a Bacterium. The water, for example, furnished to the Royal Institution laboratory by Messrs. Hopkin and Williams is sensibly as infectious as an infusion swarming with Bacteria.

Perhaps the severest experiment of this kind ever made was one executed by Dr. Sanderson with water prepared by myself. In 1871 I sought anxiously and assiduously for water free from suspended particles. The liquid was obtained in various degrees of purity, but never entirely pure. Knowing the wonderful power of extrusion, as regards foreign matter, brought into play by water in crystallizing, the thought occurred to me of examining the liquid derived from the fusion of the most transparent ice. Mr. Cottrell, at my request, arranged the following apparatus for me:—Through the plate of an air-pump (Fig. 4) passed air-tight the shank of a large funnel. A small glass bulb, B, furnished with a glass stopcock, was attached to the shank of the funnel below. Prior to being put together all parts of the apparatus had been scrupulously cleansed. In the funnel was placed a block of ice, I, selected for its transparency, having a volume of 1000 cubic inches or thereabouts, and over the ice was placed an air-tight receiver. Several times in succession the air was removed from this receiver, its place on each occasion being taken by other air carefully filtered through cotton-wool. The transparent ice was thus surrounded by moteless air.

The ice was now permitted to melt; its water trickled into the small glass bulb below, which was filled and emptied a great number of times. From the very heart of the block of ice the water was finally taken and subjected to the scrutiny of the concentrated beam. It proved to be the purest liquid I had ever seen—probably the purest human eye had ever seen; but still it contained myriads of ultra-microscopic particles. The track of the beam through it was of the most delicate blue, the blue light being perfectly polarized. It could be wholly quenched by a Nicol's prism, the beam then passing through the liquid as through a vacuum. A comparison of the light with that scattered by such mastic particles as those above referred to, proved the suspended particles of the ice-water to be far smaller than those of the mastic. No microscope, therefore, could come near them.* Such water, however, was proved by Dr. Sanderson to be as infectious as the water from any ordinary tap.

FIG. 4.



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* I have endeavoured to convey some notion of the smallness of these scattering particles in 'Fragments of Science,' 1876, pp. 441, 442, 443.

Infinitesimal as these particles are, however, they may be separated by mechanical means from the liquid in which they are held in suspension. Filters of porous earthenware, such as the porous cells of Bunsen's battery, have been turned to important account in the researches of Dr. Zahn, Professor Klebs, and Dr. Burdon Sanderson. In various instances it has been proved that, as regards the infection of living animals, the porous earthenware intercepts the contagium. For the living animal, organic infusions, or Pasteur's solution, may be substituted. Not only are ice-water, distilled water, and tap water thus deprived of their powers of infection, but, by plunging the porous cell into an infusion swarming with Bacterial life, exhausting the cell, and permitting the liquid to be slowly driven through it by atmospheric pressure, the filtrate is not only deprived of its Bacteria, but also of those ultra-microscopic particles which appear to be as potent for infection as the Bacteria themselves. The precipitated mastic particles before described, which pass unimpeded through an indefinite number of paper filters, are wholly intercepted by the porous cell.

These germinal particles abound in every pool, stream, and river. All parts of the moist earth are crowded with them. Every wetted surface which has been dried by the sun or air contains upon it the particles which the unevaporated liquid held in suspension. From such surfaces they are detached and wafted away, their universal prevalence in the atmosphere being thus accounted for. Doubtless they sometimes attach themselves to the coarser particles, organic and inorganic, which are left behind along with them; but they need no such rafts to carry them through the air, being themselves endowed with a power of flotation commensurate with their extreme smallness, and the specific lightness of the matter of which they are composed.

There cannot, moreover, be a doubt that the germs in the air differ widely among themselves as regards *preparedness* for development. Some are fresh, others old; some are dry, others moist. Infected by such germs, the same infusion would require different lengths of time to develop Bacterial life. This remark applies to and probably explains the different degrees of rapidity with which epidemic disease acts upon different people. In some the hatching period, if it may be called such, is long, in some short, the differences depending upon the different degrees of preparedness of the contagium.

Such is an outline of the present inquiry, as far as it is now complete. It gives me pleasure to refer to the untiring patience, the admirable mechanical skill, the veracity in thought, word, and deed displayed throughout by my assistant, Mr. John Cottrell, who was zealously aided by his junior colleague, Mr. Frank Valter.

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 28, 1876.

The Hon. Sir William Robert Grove, M.A. D.C.L. F.R.S.
Just. C. P., Manager, in the Chair.

Professor Huxley, LL.D. F.R.S.

The Border Territory between the Animal and the Vegetable Kingdoms.[*]

The discourse began with an examination of the distinctions between the animal and the vegetable kingdoms set forth by Cuvier in the second edition of the 'Règne Animal,' published in 1828. He characterizes animals by their possession of (1) Mobility and an alimentary cavity or reservoir of food; (2) A circulatory system; (3) A body of more complex chemical composition than plants, with an additional element, nitrogen; and (4) Respiration—that is, the absorption of oxygen and the exhalation of carbonic acid.

The progress of biological science and the application of the microscope have abolished all these distinctions.

1. Innumerable plants and free plant cells are now known to pass the whole or part of their lives in an actively locomotive condition; and their movements are, to all appearance, as spontaneous as those of animals. Many animals of even complex structure, which live parasitically within others, are wholly devoid of an alimentary cavity. The males of most rotifers have no digestive apparatus; and amidst the lowest forms of animal life the speck of gelatinous protoplasm, which constitutes the whole body, has no permanent digestive cavity or mouth, but takes in its food anywhere, and digests, so to speak, all over its body.

2. Cuvier himself practically gives up his second distinctive mark when he admits that it is wanting in the simpler animals.

3. It is now established that nitrogen is as essential a constituent of vegetable as of animal living matter; and that the latter is, chemically speaking, just as complicated as the former.

4. The green plant decomposes carbonic acid and exhales oxygen, while the animal absorbs oxygen and exhales carbonic acid; yet the difference vanishes with the sunshine, even in the case of the green plant, which, in the dark, absorbs oxygen and gives out carbonic acid like any animal. While those plants, such as the fungi, which contain no chlorophyll and are not green, absorb oxygen and give out carbonic acid.

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[*] The full discourse is given in 'Macmillan's Magazine,' February, 1876.

The researches of Schwann and Schleiden in 1837 and the following years, founded the modern science of histology, or that branch of anatomy which deals with the ultimate visible structure of organisms, as revealed by the microscope; and the rapid improvement of methods of investigation, and the energy of a host of accurate observers, have given greater and greater breadth and firmness to Schwann's great generalization, that a fundamental unity of structure obtains in animals and plants; and that however diverse may be the fabrics, or *tissues*, of which their bodies are composed, all these varied structures result from the metamorphoses of morphological units (termed *cells*, in a more general sense than that in which the word "cells" was at first employed), which are not only similar in animals and in plants respectively, but present a close fundamental resemblance when those of animals and those of plants are compared together.

The contractility which is the fundamental condition of locomotion, has not only been discovered to exist far more widely among plants than was formerly imagined, but, in plants, the act of contraction has been found to be accompanied, as Dr. Burdon Sanderson's interesting investigations have shown, by a disturbance of the electrical state of the contractile substance comparable to that which was found by Du Bois Reymond to be a concomitant of the activity of ordinary muscle in animals.*

The speaker then said that he knew not where we can hope to find any absolute distinction between animals and plants, unless we return to their mode of nutrition, and inquire whether certain differences of a more occult character than those imagined to exist by Cuvier, and which certainly hold good for the vast majority of animals and plants, are of universal application.

A bean may be supplied with water in which salts of ammonia and certain other mineral salts are dissolved in due proportion; with atmospheric air containing its ordinary minute dose of carbonic acid; and with nothing else but sunlight and heat. Under these circumstances, with proper management, the bean will thrust forth its radicle and its plumule; the former will grow down into roots, the latter grow up into the stem and leaves of a vigorous bean plant; and this plant will, in due time, flower and produce its crop of beans, just as if it were grown in the garden or in the field. The bean has taken in the raw materials of its fabric and has manufactured them into bean stuffs by the help of its green colouring matter, or chlorophyll, which, under the influence of sunlight, has the power of decomposing carbonic acid, setting free the oxygen and laying hold of the carbon which it contains. It obtained two of the absolutely indispensable elements of its substance from two distinct sources;—the watery solution, in which its roots are plunged, contains nitrogen but no carbon; the air, to which the leaves are exposed, contains carbon, but its nitrogen is in the state of a free gas, in which condition the bean can make no use of it:* and the chlorophyll is the apparatus by which the carbon is extracted from the atmospheric carbonic acid—the leaves being the chief laboratories in which this operation is effected.

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* 'Proceedings of Royal Institution,' vol. vii., p. 332.

But it by no means follows that the manufacturing power of plants depends on their chlorophyll, and its interaction with the rays of the sun. Pasteur has proved that the lowest fungi, devoid of chlorophyll, possess high manufacturing powers if supplied with a different kind of raw material. A single spore of the common mould, Penicillium, sown in a saucer full of water, in which tartrate of ammonia, with a small percentage of phosphates and sulphates is contained, and kept warm, whether in the dark or exposed to light, will, in a short time, give rise to a thick crust of mould, which contains many million times the weight of the original spore. Thus we have a very wide basis of fact for the generalization that plants are essentially characterized by their manufacturing capacity—by their power of working up mere mineral matters into complex organic compounds.

Contrariwise, there is a no less wide foundation for the generalization that animals, as Cuvier puts it, depend directly or indirectly upon plants for the materials of their bodies; that is, either they are herbivorous, or they eat other animals which are herbivorous. But for what constituents of their bodies are animals thus dependent upon plants? Certainly not for their horny matter; nor for chondrin, the proximate chemical element of cartilage; nor for gelatine; nor for syntonin, the constituent of muscle; nor for their nervous or biliary substances; nor for their amyloid matters; nor, necessarily, for their fats. It can be experimentally demonstrated that animals can make these for themselves. But that which they cannot make, but must, in all known cases, obtain directly or indirectly from plants, is the peculiar nitrogenous matter protein.

Here is the last hope of finding a sharp line of demarcation between plants and animals; for there is a border territory between the two kingdoms, a sort of no-man's land, the inhabitants of which certainly cannot be discriminated and brought to their proper allegiance in any other way.

The speaker then stated, that while examining under the microscope a drop of infusion of hay, at the request of Dr. Tyndall, he observed, in the first place, multitudes of bacteria moving about with their ordinary intermittent spasmodic wriggles; as to the vegetable nature of which there is no doubt. But other active organisms, very much larger, attaining in fact the comparatively gigantic dimensions of $\frac{1}{3000}$ of an inch or more, incessantly crossed the field of view. Each of these had a body shaped like a pear, the small end being slightly incurved and produced into a long curved filament, or *cilium*, of extreme tenuity. Behind this, from the concave side of the incurva-

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* It is purposely assumed that the air with which the bean is supplied in the case stated contains no ammoniacal salts.

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* 'Proceedings of Royal Institution,' vol. vii., p. 332.

But it by no means follows that the manufacturing power of plants depends on their chlorophyll, and its interaction with the rays of the sun. Pasteur has proved that the lowest fungi, devoid of chlorophyll, possess high manufacturing powers if supplied with a different kind of raw material. A single spore of the common mould, Penicillium, sown in a saucer full of water, in which tartrate of ammonia, with a small percentage of phosphates and sulphates is contained, and kept warm, whether in the dark or exposed to light, will, in a short time, give rise to a thick crust of mould, which contains many million times the weight of the original spore. Thus we have a very wide basis of fact for the generalization that plants are essentially characterized by their manufacturing capacity—by their power of working up mere mineral matters into complex organic compounds.

Contrariwise, there is a no less wide foundation for the generalization that animals, as Cuvier puts it, depend directly or indirectly upon plants for the materials of their bodies; that is, either they are herbivorous, or they eat other animals which are herbivorous. But for what constituents of their bodies are animals thus dependent upon plants? Certainly not for their horny matter; nor for chondrin, the proximate chemical element of cartilage; nor for gelatine; nor for syntonin, the constituent of muscle; nor for their nervous or biliary substances; nor for their amyloid matters; nor, necessarily, for their fats. It can be experimentally demonstrated that animals can make these for themselves. But that which they cannot make, but must, in all known cases, obtain directly or indirectly from plants, is the peculiar nitrogenous matter protein.

Here is the last hope of finding a sharp line of demarcation between plants and animals; for there is a border territory between the two kingdoms, a sort of no-man's land, the inhabitants of which certainly cannot be discriminated and brought to their proper allegiance in any other way.

The speaker then stated, that while examining under the microscope a drop of infusion of hay, at the request of Dr. Tyndall, he observed, in the first place, multitudes of bacteria moving about with their ordinary intermittent spasmodic wriggles; as to the vegetable nature of which there is no doubt. But other active organisms, very much larger, attaining in fact the comparatively gigantic dimensions of $\frac{1}{3000}$ of an inch or more, incessantly crossed the field of view. Each of these had a body shaped like a pear, the small end being slightly incurved and produced into a long curved filament, or *cilium*, of extreme tenuity. Behind this, from the concave side of the incurvation, proceeded another long cilium, so delicate as to be discernible only by the use of the highest powers and careful management of the light. In the centre of the pear-shaped body a clear round space could occasionally be discerned, but not always; and careful watching showed that this clear vacuity appeared gradually, and then shut up and disappeared suddenly, at regular intervals. Such a structure is of common occurrence among the lowest plants and animals, and is known as a *contractile vacuole.* The little creature sometimes propelled itself with great activity, with a curious rolling motion, by the lashing of the front cilium, while the second cilium trailed behind; sometimes it anchored itself by the hinder cilium and was spun round by the working of the other, its motions resembling those of an anchor buoy in a heavy sea. Sometimes, when two were in full career towards one another, each would appear dexterously to get out of the other's way; sometimes a crowd would assemble and jostle one another. These organisms the speaker stated were what are commonly called "monads"; they are curiously like a much larger form of monad named by Dujardin Heteromita. The speaker, therefore, called them *Heteromita lens.* As he was unable to devote to his Heteromita the attention required to make out its whole history, he referred to some remarkable observations published by Messrs. Dallinger and Drysdale * on certain Monads. Of the four monads found by them in an infusion of cod's-head, one very closely resembles *Heteromita lens* in every particular, except that it has a separately distinguishable central particle or "nucleus," which is not certainly to be made out in *Heteromita lens;* and that nothing is said by them of the existence of a contractile vacuole in this monad, though they describe it in another. Their Heteromita, however, multiplied rapidly by fission. A single Heteromita would give rise to a thousand like itself in the course of an hour, to about a million in two hours, and to a number greater than the generally assumed number of human beings now living in the world in three hours; or, if we give each Heteromita an hour's enjoyment of individual existence, the same result will be obtained in about a day.

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[*] It is purposely assumed that the air with which the bean is supplied in the case stated contains no ammoniacal salts.

Sometimes another mode of fission occurs. The body becomes rounded and quiescent, or nearly so; and while in this resting state, divides into two portions, each of which is rapidly converted into an active Heteromita. Another very remarkable kind of multiplication is termed *conjugation*, and the setting free of thousands of excessively minute granules or germs.

But these observations throw no light on the problem we are trying to solve—Is it an animal or is it a plant?

In order to show that it is possible to bring forward very strong arguments in favour of regarding Heteromita as a plant, the speaker gave details of the development of the obscure and almost microscopic mould termed *Peronospora infestans*—the fungus which is the cause of the potato disease—the whole movement of whose zoospores has a deceptive likeness to the voluntary changes of place which are observed in microscopic animals, and which affords an example of an organism, which, in one stage of its existence, is truly a "Monad," indistinguishable by any important character from the Heteromita, and extraordinarily like it in some respects, and yet which can be traced, step by step, through a series of metamorphoses until it assumes the features of an organism, which is as much a plant as an oak or an elm is. A green plant (Coleochæte) was shown to pass through a monad stage; while Chlamydomonas, and the common Volvox, or so-called "Globe animalcule," were described as running through a cycle of forms of just the same simple character as those of Heteromita.

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* "Researches in the Life-history of a Cercomonad: a Lesson in Biogenesis;" and "Further Researches in the Life-history of the Monads."—'Monthly Microscopical Journal,' 1873.

The resemblance between Chlamydomonas and Heteromita is of the closest description; and on the face of the matter there is no ground for refusing to admit that Heteromita may be related to Chlamydomonas, as the colourless fungus is to the green alga. Volvox may be compared to a hollow sphere, the wall of which is made up of coherent Chlamydomonads, and which progresses with a rotating motion effected by the paddling of the multitudinous pairs of cilia which project from its surface. Each Volvox-monad has a contractile vacuole like that of *Heteromita lens*; and moreover possesses a red pigment spot like the simplest form of eye known among animals. The methods of fissive multiplication and of conjugation observed in the monads of this locomotive globe are essentially similar to those observed in Chlamydomonas; and though a hard battle has been fought over it, Volvox is now finally surrendered to the Botanists.

Thus there is really no reason why Heteromita may not be a plant; and this conclusion would be very satisfactory, if it were not equally easy to show that there is really no reason why it should not be an animal. For there are numerous organisms presenting the closest resemblance to Heteromita, and, like it, grouped under the general name of "Monads," which, nevertheless, can be observed to take in solid nutriment, and which therefore have a virtual, if not an actual, mouth and digestive cavity, and thus come under Cuvier's definition of an animal. Numerous forms of such animals have been described by Ehrenberg, Dujardin, H. James Clark, and other writers on the Infusoria. In another infusion of hay in which the *Heteromita lens* occurred, there were innumerable infusorial animalcules belonging to the well-known species *Colpoda cucullus*. Full-sized specimens of this animalcule attain a length of between $\frac{1}{300}$ or $\frac{1}{400}$ of an inch, so that it may have ten times the length and a thousand times the mass of a Heteromita. In shape it is not altogether unlike Heteromita. The small end, however, is not produced into one long cilium, but the general surface of the body is covered with small actively vibrating ciliary organs, which are only longest at the small end. At the point which answers to that from which the two cilia arise in Heteromita, there is a conical depression, the mouth; and in young specimens a tapering filament, which reminds one of the posterior cilium of Heteromita, projects from this region. The body consists of a soft granular protoplasmic substance, the middle of which is occupied by a large oval mass called the "nucleus"; while, at its hinder end, is a "contractile vacuole," conspicuous by its regular rhythmic appearances and disappearances. Obviously, although the Colpoda is not a monad, it differs from one only in subordinate details. Moreover, under certain conditions, it becomes quiescent, encloses itself in a delicate case or *cyst*, and then divides into two, four, or more portions, which are eventually set free and swim about as active Colpodæ.

But this creature is an unmistakable animal, and full-sized Colpodæ may be fed as easily as one feeds chickens. It is only needful to diffuse very finely ground carmine through the water in which they live, and, in a very short time, the bodies of the Colpodæ are stuffed with the deeply coloured granules of the pigment. And if this were not sufficient evidence of the animality of Colpoda, there comes the fact that it is even more similar to another well-known animalcule, Paramecium, than it is to a monad. But Paramecium is so huge a creature compared with those hitherto discussed—it reaches $\frac{1}{120}$ of an inch or more in length—that there is no difficulty in making out its organization in detail; and in proving that it is not only an animal, but that it is an animal which possesses a somewhat complicated organization. For example, the surface layer of its body is different in structure from the deeper parts. There are two contractile vacuoles, from each of which radiates a system of vessel-like canals; and not only is there a conical depression continuous with a tube, which serve as mouth and gullet, but the food ingested takes a definite course and refuse is rejected from a definite region. Nothing is easier than to feed these animals, and to watch the particles of indigo or carmine accumulate at the lower end of the gullet. From this they gradually project, surrounded by a ball of water, which at length passes with a jerk, oddly simulating a gulp, into the pulpy central substance of the body, there to circulate up one side and down the other, until its contents are digested and assimilated. Nevertheless this complex animal multiplies by division, as the monad does, and, like the monad, undergoes conjugation. It stands in the same relation to Heteromita on the animal side, as Coleochæte does on the plant side. Start from either, and such an insensible series of gradations leads to the monad that it is impossible to say at any stage of the progress—here the line between the animal and the plant must be drawn.

There is reason to think that certain organisms which pass through a monad stage of existence, such as the Myxomycetes, are, at one time of their lives, dependent upon external sources for their protein-matter, or are animals; and at another period manufacture it, or are plants. And seeing that the whole progress of modern investigation is in favour of the doctrine of continuity, it is a fair and probable speculation—though only a speculation—that, as there are some plants which can manufacture protein out of such apparently intractable mineral matters as carbonic acid, water, nitrate of ammonia, and metallic salts; while others need to be supplied with their carbon and nitrogen in the somewhat less raw form of tartrate of ammonia and allied compounds; so there may be yet others, as is possibly the case with the true parasitic plants, which can only manage to put together materials still better prepared—still more nearly approximated to protein—until we arrive at such organisms as the Psorospermiæ and the Panhistophyton, which are as much animal as vegetable in structure, but are animal in their dependence on other organisms for their food.

The singular circumstance observed by Meyer, that the Torula of yeast, though an indubitable plant, still flourishes most vigorously when supplied with the complex nitrogenous substance, pepsin; the probability that the Peronospora is nourished directly by the protoplasm of the potato plant; and the wonderful facts which have recently been brought to light respecting insectivorous plants, all favour this view; and tend to the conclusion that the difference between animal and plant is one of degree rather than of kind; and that the problem whether, in a given case, an organism is an animal or a plant, may be essentially insoluble.

[T. H. H.]

WEEKLY EVENING MEETING,

Friday, February 4, 1876.

C. WILLIAM SIEMENS, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

WILLIAM HENRY PREECE, Esq. M.R.I. M. Inst. C.E. &c.

The Applications of Electricity to the Protection of Life on Railways.

IT is proposed in the following discourse to establish three propositions, viz.:

1st. That railway travelling is dangerous.
2nd. That railway travelling is safe.
3rd. That the danger is potential, and the safety actual; and that the one has been converted into the other by the operations of scientific thought, and by the applications of scientific skill.

1. The first proposition is self-evident, and scarcely needs proof. No one has stood upon a station platform when an express train has rushed madly by without feeling that there was but a rivet, a bolt, or a rod between life and death. A broken tyre or rail would hurl dozens into eternity; a disordered permanent way would maim hundreds; the mistaken motion of a handle, the failure of a signal, or the transmission of erroneous instructions, would spread terror throughout the land. There is no sensation so great as that of a dreadful railway accident. It affects everyone. All are travellers by railway, and natural selfishness makes us read with horror and dismay of the death of *units* in a railway train, while we pursue our breakfast with comparative calmness during the recital of *hundreds* smothered to death in a colliery explosion, or sent to eternity in a watery grave.

2. But is not the fact that, though we have just read the harrowing accounts of a dreadful collision in the north, we instantly entrust our precious bodies in a railway carriage to the south, a proof that there is also safety in railway travelling? Have we not faith in our railway managers, and is not this faith evidence of safety? How many of those present have been in an accident? But, after all, ideas of safety are but relative. Compare accidents on railways with accidents in the old coaching days. Take the loss of life at sea, the accidents in the hunting field, in boating, in bathing, by lightning, &c., and compare them with those on railways.

In 1873, 17,246 persons met with violent deaths in England and Wales, which is an average of 750 per million, or 1 in 1354. The causes of these deaths are thus analyzed:

TABLE I.—VIOLENT DEATHS in ENGLAND and WALES for the YEAR 1873.

Cause of Death.	No.
Injuries in mines	990
Mechanical injuries (not on railways or in mines) ..	6070
Chemical injuries	2784
Asphyxia	5193
Violence (unclassified)	919
Railways	1290

Some of these may be further analyzed as follows:

TABLE II.—ANALYSIS of TABLE I.

Cause of Death.	No.
Mechanical Injuries—	
Fall from scaffold (ladder)	165
„ „ window	70
„ downstairs	456
„ in ships and boats	134
„ from height	500
„ in walking	93
„ (not stated how)	530
„ of heavy substances on	509
Horse or other animals	269
„ conveyance	1250
Machinery	1132
Fight	5
Blow, &c.	124
Gunshot wounds	185
Chemical Injuries—	
Burns	1064
Scalds	701
„ (drinking hot water)	50
Lightning	21
Sunstroke	96
Exposure to cold	138
Asphyxia—	
Drowned	3232
Suffocated by food	94
„ bedclothes	611
Hanged, strangled, and executed	581
Murder, manslaughter, and suicide	228

Let us take accidents to railway passengers from causes within, and beyond, their own control:

TABLE III.—ACCIDENTS to RAILWAY PASSENGERS, from Causes within and beyond their own Control.

Date.	Within own control.	Beyond own control.	Total.
1871	45	12	57
1872	127	24	151
1873	120	40	160
1874	125	86	211
Average	104	41	145

This is an average of forty-one persons killed annually from causes beyond their own control, and it shows, in fact, that the railway companies are in reality more mindful of the lives of their passengers than the passengers are of their own lives.

These latter accidents can be classified as follows:

TABLE IV.—ACCIDENTS to RAILWAY PASSENGERS in 1874, from Causes within their own Control.

Cause of Accident.	No.
From falling between carriages and platforms	49
Getting out of or into trains in motion	22
Crossing the line at stations	33
Falling down stairs at stations	2
Falling out of carriages during travelling of trains ..	9
Other accidents	10
Total	125

This, however, is not the death-roll from all causes on all railways of the United Kingdom during the year 1874. The total number of persons recorded at the Board of Trade as having been killed was 1424. Of these 211 were passengers, and, of the remainder, 788 were officers or servants of the railway companies, or of contractors, and 425 were trespassers, or suicides, or others who met with accidents at level crossings or from miscellaneous causes.

1874 was, however, a very exceptional year, for no less than 71 passengers were killed in the three fearful accidents on the Great Western at Shipton, on the Great Eastern at Thorpe, and on the North British at Bowness Junction. Taking the following periods, the proportion of passengers killed from causes beyond their own control to passenger journeys made was:

Table V.—Proportion of Passengers Killed to Journeys Made.

3 years ending	1849	.. ..	1 in	4,782,188 journeys made.
4 ,,	1859	.. ..	1 ,,	8,708,411 ,,
4 ,,	1869	.. ..	1 ,,	12,941,170 ,,
3 ,,	1873	.. ..	1 ,,	20,089,660 ,,

Taking the average length of each journey at 10 miles, one passenger is killed, from causes beyond his own control, for every 200,896,000 miles travelled. If a person travelled 10 hours a day at the rate of 30 miles an hour for each of the 365 days of the year he would probably be killed in 1835 years. Hence, in a relative sense, we may consider that railway travelling is safe.

3. How is this potentiality of danger converted into comparative actuality of safety? Freedom from accident depends upon the perfection of the road, of the rolling stock, of the signals, and, above all, of the men. But none of these elements are perfect. Accidents have been analyzed into—

Table VI.—Percentage Analysis of Railway Accidents.

Defective permanent way	18 per cent.
,, rolling stock	13 ,,
,, signals	28 ,,
,, human machinery	41 ,,

They have also been classified as follows:

Table VII.—Classification of Railway Accidents, 1870-1-2-3-4.

1870.	1871.	1872.	1873.	1874.	Nature of Accident.
9	19	21	24	18	From engines or vehicles meeting with, or leaving the rails in consequence of, obstructions, or from defects in connection with the permanent way or works.
10	22	17	23	13	From boiler explosions, failures of axles, wheels, tyres, or from other defects in the rolling stock.
..	2	7	5	..	From trains entering stations at too great speed.
61	9	22	18	9	From collisions between engines and trains following one another on the same line of rails, excepting at junctions, stations, or sidings.
18	19	32	20	22	From collisions at junctions.
Included in the above 61.	63	91	98	75	From collisions within fixed signals at stations or sidings, &c.
3	2	5	3	6	From collisions between trains, &c., meeting in opposite directions.
1	..	..	3	1	From collisions at level crossings of two railways.
14	12	34	36	17	From passenger trains being wrongly run or turned into sidings, or otherwise through facing points.
6	11	9	11	7	On inclines.
9	12	8	6	..	Miscellaneous.
131	171	246	247	168	

Zeal and anxiety, the necessary evils of a state of tension due to increasing traffic; want of punctuality; late arrivals of the public; and variable weather, become an absolute source of danger. Every accident is traceable to its cause. Purely inexplicable accidents are unknown. Hence, though considerable improvements in the mode of working have been made—as are indicated in the continued progressive increase shown in ratio of killed to journeys made in Table V. —further improvements are certain. But all improvements bring their own evils, and the greatest of these is human fallibility. The body will tire, and the brain will get out of gear. Pure wilfulness, carelessness, or mischief, are extremely rare. Who does not make a mistake? In the year 1874, 4,400,000 letters out of 967,000,000, or one in 220, found their way to the Returned Letter Office. 89,540 undelivered letters contained valuables, and bank-notes, bills, &c., the value of which alone amounted to 565,000*l.*; 337 of these had no addresses; 61,000 postage stamps were found loose in the different post-offices, and 20,000 letters were posted without any address at all.

How then is the comparative safety of railway travelling produced? By taking advantage of the lessons taught by experience, and by applying the means suggested by scientific thought and inventive skill to remedy defects. Failure has thus led to improvement. Every accident has been a lesson learnt, and bitterly have those suffered who have not profited by such writings on the wall. The particulars evidenced by each accident have been carefully and systematically recorded in the reports of the inspecting officers of the Board of Trade, and thus by recording past experience, the materials are collected for carefully generalizing the laws of railway working and for establishing a true science of steam locomotion.

Telegraphy, or the art of conveying information by certain pre-concerted signals to the ear and to the eye, is the chief aid of the railway engineer. Thus, at every railway station, level crossing, or junction, signal posts are erected which convey to the approaching engine-driver by exposing discs, bars, or semaphore arms in different positions by day, or lamps displaying different colours by night, the fact that the line is clear for him to proceed or obstructed so that he must stop. The favourite signal by day—the survival of the fittest—is the arm, which, when at right angles, implies *danger*, and when at an angle of 45°, *safety*, and

> "White means right: red means wrong:
> Green means slowly go along,"

teaches the young railway lad the rule of the road by night. The character of every train is indicated by its *head lights* and its presence to an approaching train by its *tail lamps*. Should thick weather prevent the sight of the signals, detonating fog signals announce the contiguity of danger. The marshalling of trains in station yards and platforms is produced by whistles and flags by day and lamps by night, all forming a species of telegraphic language between the fixed station and the moving train.

Where telegraphy is required to reach distances beyond the sphere of the ear or the eye, electricity is employed, and the electric telegraph becomes of prime and essential use, not only in regulating the traffic on double and single lines, but in securing safety. Special trains are moved about by its means, delays are remedied, breaksdown rendered harmless, runaway engines have been overtaken by its aid, passengers' luggage recovered, but, above all, irregularities are by its means rapidly announced, and the evils of unpunctuality rendered innocuous.

The greatest element of safety on railways is, however, the Block System.

The block system arose out of the multiplication of trains, and the necessity for increased speed. Necessity, the mother of invention, brought it into existence.

By it trains travelling upon the same line of rails are kept apart by a certain and invariable interval of *space*, instead of by an uncertain and variable interval of *time*.

The practice under the time system is to exhibit the danger signal for five minutes, and the caution signal for five minutes more, after a train or engine has been despatched from or past any station, junction, level crossing, or siding. Trains are thus said to be kept apart by fixed periods of five minutes, and if the caution signals were properly regarded, by an interval of time even longer than that. The safety of the train is entirely the responsibility of the driver. Immunity from accident is dependent upon his keeping a clear look-out. If engines ran at regular and fixed speeds, if time tables could be adhered to, if the line were not crowded with traffic, if the driver could always ensure a good view before him, if signals were near together and they were properly regarded, then a rigid interval of time might be maintained between following trains; but none of these elements of safety are constant. Fast expresses follow slow goods trains, now through a thick fog, now up a wet incline, at one moment in bright sunshine, at the next in a thick snowstorm; creeping mineral trains break down in a long interval between two stations; passengers rush in at the very last minute, detain the train, and prevent the time tables from being adhered to; trains are so frequent at some places that the five minutes' interval cannot be adhered to; obstructions to view arise from curves or cuttings, or from atmospheric causes; long lengths of line are unprotected by any signal at all, and signals themselves are too frequently neglected. Hence, the system is brimful of elements of danger, and the inexorable logic of facts has shown that the time interval is illusory and the system unsafe.

But when trains, however rapidly or slowly they may be running, however much punctuality has been infringed, however crowded with traffic the line may be, are invariably kept apart by an interval of one or two miles, collision between them becomes impossible. This is the *Block system*, which has, very improperly, been divided into two classes, the *absolute* and the *permissive*. The former is the block system proper, the latter is not a "block" system at all, but a system introduced, not to secure the safety of trains, but to increase the capacity of the line for the transmission of increasing traffic. It is, doubtless, an improvement on the time system, but it bears little affinity to the block, and should certainly not be included in the same category.

The block system is effectually carried out by means of electricity. Communication is maintained between station and station by means of bells rung by currents sent to announce the approach and departure of the trains. Permanent signals are raised and lowered, indices are moved to one position or another to indicate the presence or absence of danger, or the fact of the line being obstructed or clear. Indicators are moved to repeat back the signals made to check accuracy in working, and to render futile the errors or carelessness of the hasty or thoughtless. Safety is secured and accuracy in working is maintained by checks and by counterchecks.

The block system on single lines is additionally used to protect trains from *advancing* as well as from *succeeding* trains. Before a train is allowed to leave A the line at B is blocked in advance, and when it leaves it is blocked behind at A, so that it is thoroughly protected in both directions during the period it is running from A to B.

But apart from the protection which electricity imparts to railway travelling, and the facility it offers for adjusting and regulating the traffic, there are innumerable purposes for which the telegraph is employed to facilitate business and to secure efficiency. The distribution of correct time, the collection of spare trucks and coaches, the relief of staff, the supply of assistance in cases of accident and danger, and—not least—the reparation of the error and thoughtlessness of passengers.

It is used on some lines to establish an effective means of communication between passenger and guard; and perhaps one of its most useful applications is to record in the signal-box, before the signalman's eyes, the position of the signal arm by day and the condition of the light by night, which is hidden from his sight by the formation of the line, buildings, darkness, fog, or steam. Electric repeaters are one of the greatest elements of safety in working railways.

The operation of scientific thought has introduced many mechanical elements of safety into railway working, which are as ingenious as they are effective.

Improved permanent way, the interlocking of signals and points; the concentration of levers in well-constructed cabins; effective brake power; perfect tyre fastenings; better coupling arrangements, and superior engine and rolling stock, have all aided to secure that simplicity in working and safety in travelling which undoubtedly exist.

But, as the principal element of danger in railway travelling consists in the fallibility of the human machine, it must not be forgotten that we owe our immunity from accident as much to the careful selection, education, and supervision of the staff and the maintenance of good discipline, as to the appliances of scientific skill. Science cannot be devoted to a nobler purpose than to the protection of human life, and the records of experience show that it has earned well-deserved laurels in rendering the dangers of railway travelling potential and its safety actual.

[W. H. P.]

GENERAL MONTHLY MEETING,

Monday, February 7, 1876.

The **Duke of Northumberland**, D.C.L. President, in the Chair.

John Robertson Adams, Esq.
Arthur Brewin, Esq. F.R.A.S.
Ernest De la Rue, Esq.
Charles Fletcher, Esq.
Thomas Matthew Gisborne, Esq.
Mrs. Mitchell,
Martin Archer-Shee, jun. Esq.
Francis Lys Smith, Esq.
Mrs. Theodore Williams,
Alexander Brown Thorburn, Esq.

were *elected* Members of the Royal Institution.

The Special Thanks of the Members were returned to **Samuel Scott**, Esq. M.R.I. for his Donation of Five Guineas to aid the General Objects of the Institution.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Lords Commissioners of the Admiralty—Nautical Almanac for 1879. 8vo. 1875.
Greenwich Observations, 1873. 4to. 1875.
The French Government—Documents Inédits sur l'Histoire de France: Cartulaires de l'Eglise Cathédrale de Grenoble. 4to. 1869.
Asiatic Society, Royal—Journal, New Series, Vol. VIII. Part 1. 8vo. 1875.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVI. Nos. 1, 2. 8vo. 1875-6.
Memoirs, Vol. XLI. 4to. 1875.
British Museum Trustees—Catalogue of Birds, Vol. II. 8vo. 1875.
Catalogue of Additional MSS. 1854-60. 8vo. 1875.
Catalogue of Spanish MSS. Vol. I. 8vo. 1875.
Alcock, Lieut.-Col. M.R.I.—The Militia, &c. 8vo. 1867-75.
Attfield, Professor J. Ph.D. F.C.S.—Chemistry, General, Medical, and Pharmaceutical. 6th Ed. 12mo. 1876.
Basel Naturforschende Gesellschaft—Verhandlungen. Sechster Theil, 2tes Heft. 8vo. 1875.

British Architects, Royal Institute of—Sessional Papers, 1875–6. Nos. 1–4. 4to.

Calcutta Meteorological Office—Bengal Meteorological Reports, &c. fol. 1867–74.

Chemical Society—Journal for Nov. Dec. 1875. 8vo.

Civil Engineers' Institution—Minutes of Proceedings, Vol. XLII. 8vo. 1875.

Clockmakers' Company—Catalogue of Books, MSS., Clocks, &c., of the Worshipful Company of Clockmakers, deposited in the Free Library of the City of London. 8vo. 1875.

Comitato Geologico d'Italia—Bollettini, 1875: Nos. 11, 12. 8vo.

Editors—American Journal of Science for Dec. 1875, Jan. 1876. 8vo.
  Athenæum for Dec. 1875, Jan. 1876. 4to.
  Chemical News for Dec. 1875, Jan. 1876. 4to.
  Electrical News for Dec. 1875, Jan. 1876.
  Engineer for Dec. 1875, Jan. 1876. fol.
  Journal for Applied Science for Dec. 1875, Jan. 1876. fol.
  Nature for Dec. 1875, Jan. 1876. 4to.
  Nautical Magazine for Dec. 1875, Jan. 1876. 8vo.
  Pharmaceutical Journal for Dec. 1875, Jan. 1876. 8vo.
  Practical Magazine for Dec. 1875, Jan. 1876. 8vo.
  Quarterly Journal of Science, Jan. 1876. 8vo.
  Telegraph Journal for Dec. 1875, Jan. 1876. 8vo.

Franklin Institute—Journal, Nos. 599, 600. 8vo. 1875–6.

Geographical Society, Royal—Proceedings, Vol. XX. No. 1. 8vo. 1876.

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WEEKLY EVENING MEETING,

Friday, February 11, 1876.

The Hon. Sir William Robert Grove, M.A. Ph.D. F.R.S.
Just. C. P., Manager, in the Chair.

W. Crookes, Esq. F.R.S.

The Mechanical Action of Light.

To generate motion has been found a characteristic common, with one exception, to all the phases of physical force. We hold the bulb of a thermometer in our hands, and the mercury expands in bulk, and, rising along the scale, indicates the increase of heat it has received. We heat water, and it is converted into steam, and moves our machinery, our carriages, and our ironclads. We bring a loadstone near a number of iron filings, and they move towards it, arranging themselves in peculiar and intricate lines; or we bring a piece of iron near a magnetic needle, and we find it turned away from its ordinary position. We rub a piece of glass with silk, thus throwing it into a state of electrical excitement, and we find that bits of paper or thread fly towards it, and are, in a few moments, repelled again. If we remove the supports from a mass of matter it falls, the influence of gravitation being here most plainly expressed in motion, as shown in clocks and water-mills. If we fix pieces of paper upon a stretched string, and then sound a musical note near it, we find certain of the papers projected from their places. Latterly, the so-called "sensitive flames," which are violently agitated by certain musical notes, have become well known as instances of the conversion of sound into motion. How readily chemical force undergoes the same transformation is manifested in such catastrophes as those of Bremerhaven, in the recent deplorable coal-mine explosions, and indeed in every discharge of a gun.

But light, in some respects the highest of the powers of nature, has not been hitherto found capable of direct conversion into motion, and such an exception cannot but be regarded as a singular anomaly.

This anomaly the researches which I am about to bring before you have now removed; and, like the other forms of force, light is found to be capable of direct conversion into motion, and of being—like heat, electricity, magnetism, sound, gravitation, and chemical action—most delicately and accurately measured by the amount of motion thus produced.

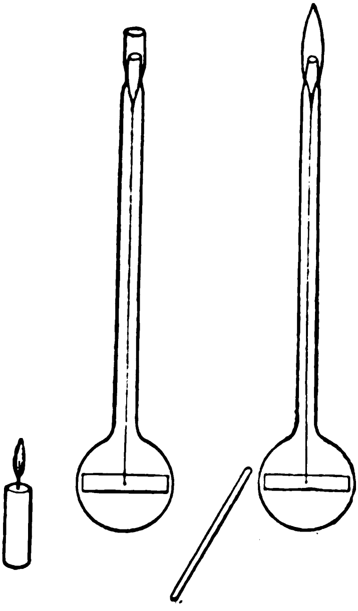
My research arose from the study of an anomaly.

It is well known to scientific men that bodies appear to weigh less when they are hot than when they are cold; the explanation given being, that the ascending currents of hot air buoy up the body, so to speak. Wishing to get rid of this and other interfering actions of the air during a research on the atomic weight of thallium, I had a balance constructed in which I could weigh in a vacuum. I still, indeed, found my apparatus less heavy when hot than when cold. The obvious explanations were evidently not the true ones: *obvious* explanations seldom are true ones, for simplicity is not a characteristic of nature.

An unknown disturbing cause was interfering, and the endeavour to find the clue to the apparent anomaly has led to the discovery of the mechanical action of light.

I was long troubled by the apparent lawlessness of the actions I obtained. By gradually increasing the delicacy of my apparatus I could easily get certain results of motion when hot bodies were brought near them, but sometimes it was one of attraction, at others of repulsion, whilst occasionally no movement whatever was produced.

FIG. 1.



I will try to reproduce these phenomena in this apparatus (Fig. 1). Here are two glass bulbs, each containing a bar of pith about 3 inches long and half an inch thick, suspended horizontally by a long fibre of cocoon silk. I bring a hot glass rod, or a candle, towards one of them, and you see that the pith is gradually attracted, following the candle as I move it round the bulb. That seems a very definite fact; but look at the action in the other bulb. I bring the candle, or a hot glass rod, near the other bar of pith, and it is strongly *repelled* by it—much more strongly than it was attracted in the first instance.

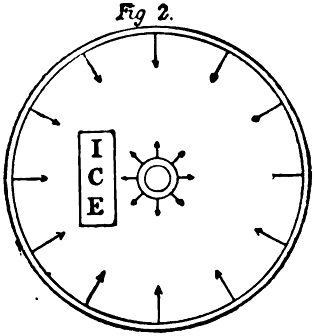
Here, again, is a third fact. I bring a piece of ice near the pith bar which has just been repelled by the hot rod, and it is attracted, and follows the rod round as a magnetic needle follows a piece of iron.

The repulsion by radiation is the key-note of these researches. The movement of a small bar of pith is not very distinct, except to those near, and I wish to make this repulsion evident to all. I have therefore arranged a piece of apparatus by which it can be seen by all present. I will, by means of the electric light, project an image of a pendulum suspended *in vacuo* on the screen. You see that the approach of a candle gives the bob a veritable push, and, by alternately obscuring and uncovering the light, I can make the pendulum beat time to my movements.

What then is the cause of the contradictory action in these two bulbs—attraction in one, and repulsion in the other? It can be explained in a few words. Attraction takes place when air is present, and repulsion when air is absent.

Neutrality, or no movement, is produced when the vacuum is insufficient. A minute trace of air in the apparatus interferes most materially with the repulsion, and for a long time I was unaware of the powerful action produced by radiation in a "perfect" vacuum.

Fig 2.



It is not at first sight obvious how ice or a cold body can produce the opposite effect to heat. The law of exchanges, however, explains this perfectly. The pith bar and the whole of the surrounding bodies are incessantly exchanging heat-rays; and under ordinary circumstances the income and expenditure of heat are in equilibrium. Let me draw your attention to the diagram (Fig. 2) illustrating what takes place when I bring a piece of ice near the apparatus. The centre circle represents my piece of pith; the arrows show the influx and efflux of heat. A piece of ice brought near cuts off the influx of heat from one side, and therefore allows an excess of heat to fall on the pith from the opposite side. Attraction by a cold body is therefore seen to be only repulsion by the radiation from the opposite side of the room.

The later developments of this research have demanded the utmost refinement of apparatus. Everything has to be conducted in glass vessels, and these must be blown together till they make one piece, for none but fused joints are admissible. In an investigation depending for its successful prosecution on manipulative dexterity, I have been fortunate in having the assistance of my friend Mr. Charles Gimingham. All the apparatus you see before you are the fruits of his skilful manipulation, and I now want to draw your attention to what I think is a masterpiece of glass-working—the pump which enables me so readily to produce a vacuum unattainable by ordinary means.

The pump here at work is a modification of the Sprengel pump, but it contains two or three valuable improvements. I cannot attempt to describe the whole of the arrangements, but I will rapidly run over them as illuminated by the electric light. It has a triple fall tube in which the mercury is carried down, thus exhausting with threefold rapidity; it has Dr. McLeod's beautiful arrangement for measuring the residual gas; it has gauges in all directions, and a small radiometer attached to it to tell the amount of exhaustion that I get in any experiments; it has a contrivance for admitting oil of vitriol into the tubes without interfering with the progress of the exhaustion, and it is provided with a whole series of most ingenious vacuum-taps devised by Mr. Gimingham. The exhaustion produced in this pump is such that a current of electricity from an induction-coil will not pass across the vacuum. This pump is now exhausting a torsion balance, which will be described presently. Another pump, of a similar kind but less complicated, is exhausting an apparatus which has enabled me to pass from the mere exhibition of the phenomena to the obtaining of quantitative measurements.

A certain amount of force is exerted when a ray of light or heat falls on the suspended pith, and I wished to ascertain—

First. What were the actual rays—invisible heat, luminous, or ultra-violet—which caused this action?
Secondly. What influence had the colour of the surface on the action?
Thirdly. Was the amount of action in direct proportion to the amount of radiation?
Fourthly. What was the amount of force exerted by radiation?

I required an apparatus which would be easily moved by the impact of light on it, but which would readily return to zero, so that measurements might be obtained of the force exerted when different amounts of light acted on it. At first I made an apparatus on the Zöllner's horizontal pendulum. For a reason that will be explained presently, I am unable to show you the apparatus at work, but the principle of it is shown in the diagram (Fig. 3). The pendulum represented by this horizontal line has a weight at the end. It is supported on two fibres of glass, one stretched upwards and the other stretched downwards, both firmly fastened at the ends, and also attached to the horizontal rod (as shown in the figure) at points near together, but not quite opposite to one another.

It is evident that if there is a certain amount of pull upon each of these fibres, and that the pull can be so adjusted as to counteract the weight at the end and keep it horizontal, the nearer the beam approaches the horizontal line the slower its rate of oscillation. If I relax the tension, by throwing the horizontal beam downwards, I get a more rapid oscillation sideways. If I turn the levelling screw so as to raise the beam and weight, the nearer it approaches the horizontal position the slower the oscillation becomes, and the more delicate is the instrument. Here is the actual apparatus that I tried to work with. The weight at the end is a piece of pith; in the centre is a glass mirror, on which to throw a ray of light, so as to enable me to see the movements by a luminous index. The instrument, enclosed in glass and exhausted of air, was mounted on a stand with levelling screws, and with it I tried the action of a ray of light falling on the pith. I found that I could get any amount of sensitiveness that I liked; but it was not only sensitive to the impact of a ray of light, it was immeasurably more so to a change of horizontality. It was, in fact, too delicate for me to work with. The slightest elevation of one end of the instrument altered the sensitiveness, or the position of the zero-point, to such a degree that it was impossible to try any experiments with it in such a place as London. A person stepping from one room to another altered the position of the centre of gravity of the house. If I walked from one side of my own laboratory to the other, I tilted the house over sufficiently to upset the equilibrium of the apparatus. Children playing in the streets disturbed it. Professor Rood, who has worked with an apparatus of this kind in America, finds that an elevation of its side equal to $\frac{1}{36000000}$ part of an inch is sufficient to be shown on the instrument. It was therefore out of the question to use an instrument of this construction, so I tried another form (shown in Fig. 4), in which a fine glass beam, having discs of pith at each end, is suspended horizontally by a fine glass fibre, the whole being sealed up in glass and perfectly exhausted. To the centre of oscillation a glass mirror is attached.

Fig 3.

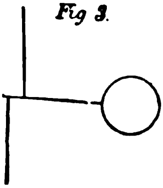
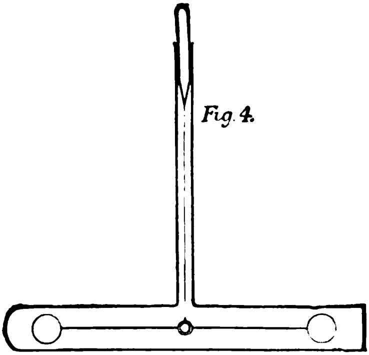


Fig. 4.



Now a glass fibre has the property of always coming back to zero when it is twisted out of its position. It is almost, if not quite, a perfectly elastic body. I will show this by a simple experiment. This is a long glass fibre hanging vertically, and having a horizontal bar suspended on it. I hold the bar, and turn it half round; it swings backwards and forwards for a few times, but it quickly comes back to its original position. However much twist, however much torsion, may be put on this, it always returns ultimately to the same position. I have twisted glass fibres round, and kept them in a permanent state of twist more than a hundred complete revolutions, and they always came back accurately to zero. The principle of an instrument that I shall describe farther on depends entirely on this property of glass.

Instead of using silk to suspend the torsion beam with, I employ a fibre of glass, drawn out very fine before the blowpipe. A thread of glass of less than the thousandth of an inch in thickness is wonderfully strong, of great stiffness, and of perfect elasticity, so that however much it is twisted round short of the breaking point, it untwists itself perfectly when liberated. The advantage of using glass fibres for suspending my beam is, therefore, that it always returns accurately to zero, after having tried an experiment, whilst I can get any desired amount of sensitiveness by drawing out the glass fibre sufficiently fine.

Here, then, is the torsion apparatus sealed on to a Sprengel pump. You will easily understand the construction by reference to the diagram (Fig. 4). It consists of a horizontal beam suspended by a glass fibre, and having discs of pith at each end coated with lampblack. The whole is enclosed in a glass case, made of tubes blown together, and by means of the pump the air is entirely removed. In the centre of the horizontal beam is a silvered mirror, and a ray from the electric light is reflected from it on to a scale in front, where it is visible as a small circular spot of light. It is evident that an angular movement of the torsion beam will cause the spot of light to move to the right or to the left along the scale. I will first show you the wonderful sensitiveness of the apparatus. I simply place my finger near the pith disc at one end, and the warmth is quite sufficient to drive the spot of light several inches along the scale. It has now returned to zero, and I place a candle near it. The spot of light flies off the scale. I now bring the candle near it alternately from one side to the other, and you see how perfectly it obeys the force of the candle. I think the movement is almost better seen without the screen than with it. The fog, which has been so great a detriment to everyone else, is rather in my favour, for it shows the luminous index like a solid bar of light swaying to and fro across the room. The warmth of my finger, or the radiation from a candle, is therefore seen to drive the pith disc away. Here is a lump of ice, and on bringing it near one of the discs the luminous index promptly shows a movement of apparent attraction.

With this apparatus I have tried many experiments, and amongst others I have endeavoured to answer the question, "Is it light, or is it heat, that produces the movement?" for that is a question that is asked me by almost everyone; and a good many appear to think that if the motion can be explained by an action of heat, all the novelty and the importance of the discovery vanish. Now this question of light or heat is one I cannot answer, and I think that when I have explained the reason you will agree with me that it is unanswerable.

There is no physical difference between light and heat. Here is a diagram of the visible spectrum (Fig. 5). The spectrum, as scientific men understand it, extends from an indefinite distance beyond the red

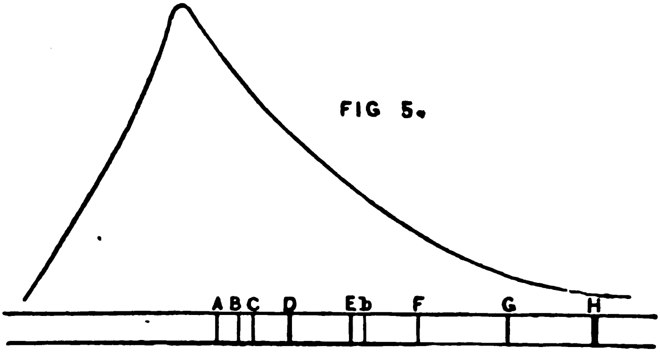


FIG 5.

to an indefinite distance beyond the violet. We do not know how far it would extend one way or the other if no absorbing media were present; but, by what we may call a physiological accident, the human eye is sensitive to a portion of the spectrum situated between the line A in the red to about the line H in the violet. But this is not a physical difference between the luminous and non-luminous parts of the spectrum; it is only a physiological difference. Now, the part at the red end of the spectrum possesses, in the greatest degree, the property of causing the sensation of warmth, and of dilating the mercury in a thermometer, and of doing other things which are conveniently classed among the effects of *heat;* the centre part affects the eye, and is therefore called *light;* whilst the part at the other end of the spectrum has the greatest energy in producing *chemical action.* But it must not be forgotten that any ray of the spectrum, from whatever part it is selected, will produce all these physical actions in more or less degree. A ray here, at the letter C for instance in the orange, if concentrated on the bulb of a thermometer, will cause the mercury to dilate, and thus show the presence of *heat;* if concentrated on my hand I feel *warmth;* if I throw it on the face of a thermo-pile it will produce a current of *electricity;* if I throw it upon a sensitive photographic plate it will produce *chemical action;* and if I throw it upon the instrument I have just described, it will produce *motion.* What, then, am I to call that ray? Is it light, heat, electricity, chemical action, or motion? It is neither. All these actions are inseparable attributes of the ray of that particular wave-length, and are not evidences of separate identities. I can no more split that ray up into five or six different rays each having different properties, than I can split up the element iron, for instance, into other elements, one possessing the specific gravity of iron, another its magnetic properties, a third its chemical properties, a fourth its conducting power for heat, and so on. A ray of light of a definite refrangibility is one and indivisible, just as an element is, and these different properties of the ray are mere functions of that refrangibility, and inseparable from it. Therefore when I tell you that a ray in the ultra-red pushes the instrument with a force of 100, and a ray in the most luminous part has a dynamic value of about half that, it must be understood that the latter action is not due to heat-rays which accompany the luminous rays, but that the action is one purely due to the wave-length and the refrangibility of the ray employed. You now understand why it is that I cannot give a definite answer to the question, "Is it heat or is it light that produces these movements?" There is no physical difference between heat and light; so, to avoid confusion, I call the total bundle of rays which come from a candle or the sun, *radiation*.

I found, by throwing the pure rays of the spectrum one after the other upon this apparatus, that I could obtain a very definite answer to my first question, "What are the actual rays which cause this action?"

The apparatus was fitted up in a room specially devoted to it, and was protected on all sides, except where the rays of light had to pass, with cotton-wool and large bottles of water. A heliostat reflected a beam of sunlight in a constant direction, and it was received on an appropriate arrangement of slit, lenses, prisms, &c., for projecting a pure spectrum. Results were obtained in the months of July, August, and September; and they are given in the figure (Fig. 5) graphically as a curve, the maximum being in the ultra-red and the minimum in the ultra-violet. Taking the maximum at 100, the following are the mechanical values of the different colours of the spectrum:

Ultra-red	100
Extreme red	85
Red	73
Orange	66
Yellow	57
Green	41
Blue	22
Indigo	8½
Violet	6
Ultra-violet	5

A comparison of these figures is a sufficient proof that the mechanical action of radiation is as much a function of the luminous rays as it is of the dark heat-rays.

The second question, namely, "What influence has the colour of the surface on the action?" has also been solved by this apparatus.

In order to obtain comparative results between discs of pith coated with lampblack and with other substances, another torsion apparatus was constructed, in which six discs *in vacuo* could be exposed one after the other to a standard light. One disc always being lampblacked pith, the other discs could be changed so as to get comparisons of action. Calling the action of radiation from a candle on the lampblacked disc 100, the following are the proportions obtained:

Lampblacked pith	100
Iodide of palladium	87·3
Precipitated silver	56
Amorphous phosphorus	40
Sulphate of baryta	37
Milk of sulphur	31
Red oxide of iron	28
Scarlet iodide of mercury and copper	22
Lampblacked silver	18
White pith	18
Carbonate of lead	13
Rock-salt	6·5
Glass	6·5

This table gives important information on many points: one more especially—the action of radiation on lampblacked pith is 5½ times what it is on plain pith. A bar like those used in my first experiment, having one half black and one half white, exposed to a broad beam of radiation, will be pushed with 5½ times more strength on the black than on the white half, and if freely suspended will set at an angle greater or less according to the intensity of the radiation falling on it.

This suggests the employment of such a bar as a photometer, and I have accordingly made an instrument on this principle: its construction is shown in the diagram (Fig. 6). It consists of a flat bar of pith, A, half black and half white, suspended horizontally in a bulb by means of a long silk fibre. A reflecting mirror, B, and small magnet, C, are fastened to the pith, and a controlling magnet, D, is fastened outside, so that it can slip up and down the tube, and thus increase or diminish sensitiveness. The whole is completely exhausted and then enclosed in a box lined with black velvet, with apertures for the rays of light to pass in and out. A ray of light from a lamp, F, reflected from the mirror, B, to a graduated scale, G, shows the movements of the pith bar.

The instrument fitted up for a photometric experiment is in front of me on the table. A beam from the electric light falls on the little mirror, and is thence reflected back to the screen, where it forms a spot of light, the displacement of which to the right or the left shows the movement of the pith bar. One end of the bar is blacked on each side, the other end being left plain. I have two candles, E E, each 12 inches off the pith bar, one on each side of it. When I remove the screens, H H, the candle on one side will give the pith a push in one direction, and the candle on the other side will give the pith a push in the opposite direction, and as they are the same distance off they will neutralize each other, and the spot of light will not move. I now take the two screens away; each candle is pushing the pith equally

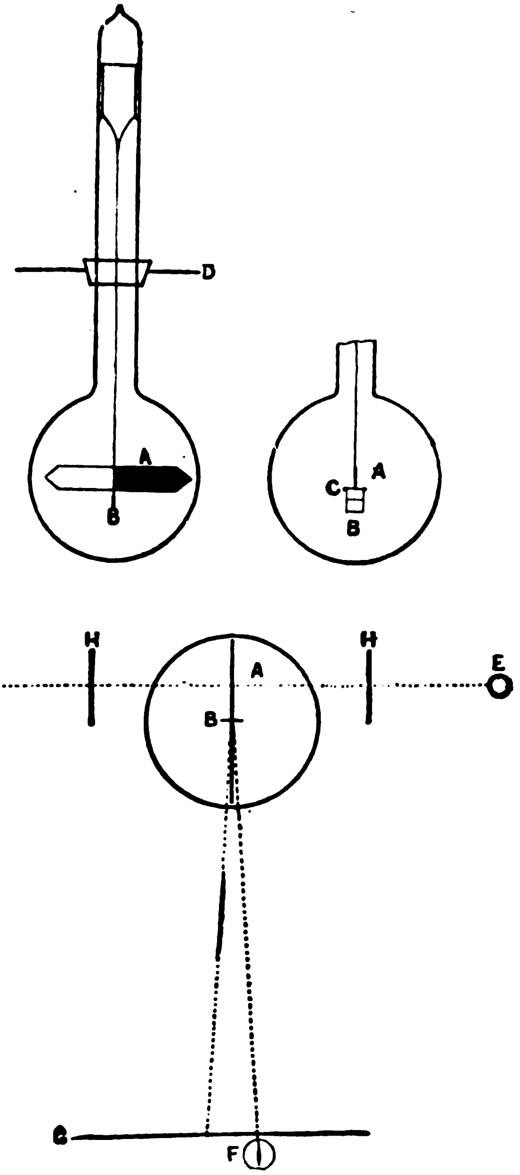

A
D
A
B
C
A
B
H
H
E
E
A
B
G
F

FIG. 6.

in opposite directions, and the luminous index remains at zero. When, however, I cut one candle off, the candle on the opposite side exerts its full influence, and the index flies to one end of the scale. I cut the other one off and obscure the first, and the spot of light flies to the other side. I obscure them both, and the index comes quickly to zero. I remove the screens simultaneously, and the index does not move.

I will retain one candle 12 inches off, and put two candles on the other side 17 inches off. On removing the screens you see the index does not move from zero. Now the square of 12 is 144, and the square of 17 is 289. Twice 144 is 288. The light of these candles, therefore, is as 288 to 289. They therefore balance each other as nearly as possible. Similarly I can balance a gas-light against a candle. I have a small gas-burner here, which I place 28 inches off on one side, and you see it balances the candle 12 inches off. These experiments show how conveniently and accurately this instrument can be used as a photometer. By balancing a standard candle on one side against any source of light on the other, the value of the latter in terms of a candle is readily shown; thus in the last experiment the standard candle 12 inches off is balanced by a gas-flame 28 inches off. The lights are therefore in the proportion of 12^2 to 28^2, or as 1 to 5·4. The gas-burner is therefore equal to about 5½ candles.

In practical work on photometry it is often required to ascertain the value of gas. Gas is spoken of commercially as of so many candle-power. There is a certain "standard" candle which is supposed to be made invariable by Act of Parliament. I have worked a great deal with these standard candles, and I find them to be among the most variable things in the world. They never burn with the same luminosity from one hour to the other, and no two candles are alike. I can now, however, easily get over this difficulty. I place a "standard" candle at such a distance from the apparatus that it gives a deflection of 100 degrees on the scale. If it is poorer than the standard, I bring it nearer; if better, I put it farther off. Indeed, any candle may be taken; and if it be placed at such a distance from the apparatus that it will give a uniform deflection, say of 100 divisions, the standard can be reproduced at any subsequent time; and the burning of the candle may be tested during the photometric experiments by taking the deflection it causes from time to time, and altering its distance, if needed, to keep the deflection at 100 divisions. The gas-light to be tested is placed at such a distance on the opposite side of the pith bar that it exactly balances the candle. Then, by squaring the distances, I get the exact proportion between the gas and the candle.

Before this instrument can be used as a photometer or light measurer, means must be taken to cut off from it all those rays coming from the candle or gas which are not actually luminous. A reference to the spectrum diagram (Fig. 5) will show that at each end of the coloured rays there is a large space inactive, as far as the eye is concerned, but active in respect to the production of motion—strongly so at the red end, less strong at the violet end. Before the instrument can be used to measure luminosity, these rays must be cut off. We buy gas for the light that it gives, not for the heat it evolves on burning, and it would therefore never do to measure the heat and pay for it as light.

It has been found that a clear plate of alum, whilst letting all the light through, is almost, if not quite, opaque to the heating rays below the red. A solution of alum in water is almost as effective as a crystal of alum; if, therefore, I place in front of the instrument glass cells containing an aqueous solution of alum, the dark heat-rays are filtered off.

But the ultra-violet rays still pass through, and to cut these off I dissolve in the alum solution a quantity of sulphate of quinine. This body has the property of cutting off the ultra-violet rays from a point between the lines G and H. A combination of alum and sulphate of quinine, therefore, limits the action to those rays which affect the human eye, and the instrument, such as you see it before you, becomes a true photometer.

This instrument, when its sensitiveness is not deadened by the powerful control magnet I am obliged to keep near it for these experiments, is wonderfully sensible to light. In my own laboratory a candle 36 feet off produces a decided movement, and the motion of the index increases inversely with the square of the distance, thus answering the third question, "Is the amount of action in direct proportion to the amount of radiation?"

The experimental observations and the numbers which are required by the theoretical diminution of light with the square of the distance, are sufficiently close, as the following figures show:

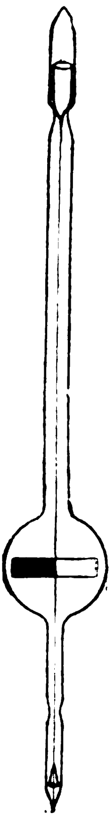
Candle	6	feet off	gives a deflection of	218·0°
„	12	„	„	54·0°
„	18	„	„	24·5°
„	24	„	„	13·0°
„	10	„	„	77·0°
„	20	„	„	19·0°
„	30	„	„	8·5°

The effect of two candles side by side is practically double, and of three candles three times that of one candle.

In the instrument just described the candle acts on a pith bar, one end of which is blacked on each side. But suppose I black the bar on alternate halves and place a light near it sufficiently strong to drive the bar half round. The light will now have presented to it another black surface in the same position as the first, and the bar will be again driven in the same direction half round. This action will be again repeated, the differential action of the light on the black and white surfaces keeps the bar moving, and the result will be rotation.

Here is such a pith bar, blacked on alternate sides, and suspended in an exhausted glass bulb (Fig. 7). I project its image on the screen, and the strong light which shines on it sets it rotating with considerable velocity. Now it is slackening speed, and now it has stopped altogether. The bar is supported on a fibre of silk, which has twisted round till the rotation is stopped by the accumulated torsion. I put a water screen between the bar and the electric light to cut off some of the active rays, and the silk untwists, turning the bar in the opposite direction. I now remove the water, and the bar revolves rapidly as at first.

FIG. 7



From suspending the pith on a silk fibre to balancing it on a point the transition is slight; the interfering action of torsion is thereby removed, and the instrument rotates continuously under the influence of radiation. Many of these little pieces of apparatus, to which I have given the name of radiometers, are on the table, revolving with more or less speed. The diagram (Fig. 8) shows their construction, which is very simple. They are formed of four arms of very fine glass, supported in the centre by a needle-point, and having at the extremities thin discs of pith lampblacked on one side, the black surfaces all facing the same way. The needle stands in a glass cup, and the arms and discs are delicately balanced so as to revolve with the slightest impetus.

Here are some rotating by the light of a candle. This one is now rather an historical instrument, being the first one in which I saw rotation. It goes very slowly in comparison with the others, but it is not bad for the first instrument of the sort that was ever made.

I will now, by means of a vertical lantern, throw on the screen the projection of one of these instruments, so as to show the movement rather better than you could see it on the table. The electric light falling vertically downwards on it, and much of the power being cut off by water and alum screens, the rotation is slow. I bring a candle near and the speed increases. I now lift the radiometer up, and place it full in the electric light, projecting its image direct on the screen, and it goes so rapidly that if I had not cut out the four pieces of pith of different shapes you would have been unable to follow the movement.

The speed with which a sensitive radiometer will revolve in the sun is almost incredible; and the electric light such as I have in this lantern cannot be far short of full sunshine. Here is the most sensitive instrument I have yet made, and I project its image on the screen, letting the full blaze of the electric light shine upon it.

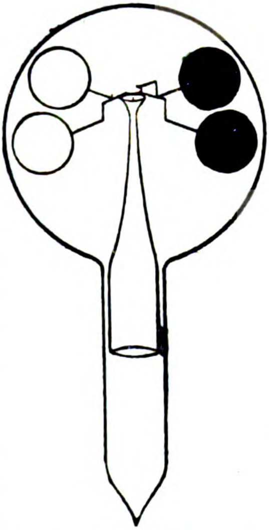
Nothing is seen but an undefined nebulous ring, which becomes at times almost invisible. The number of revolutions per second cannot be counted, but they must be several hundreds, for one candle has made it spin round forty times a second.

I have called the instrument the radiometer, because it will enable me to measure the intensity of radiation falling on it by counting the revolutions in a given time; the law being that the rapidity of revolution is inversely as the square of the distance between the light and the instrument.

When exposed to different numbers of candles at the same distance off, the speed of revolution in a given time is in proportion to the number of candles; two candles giving twice the rapidity of one candle, and three, three times, &c.

The position of the light in the horizontal plane of the instrument is of no consequence, provided the distance is not altered; thus two candles, 1 foot off, give the same number of revolutions per second, whether they are side by side or opposite to each other. From this it follows that if the radiometer is brought into a uniformly lighted space it will continue to revolve.

FIG 8.



It is easy to get rotation in a radiometer without having the surfaces of the discs differently coloured. Here is one having the pith discs blacked on both sides. I project its image on the screen, and there is no movement. I bring a candle near it, and shade the light from one side, when rapid rotation is produced, which is at once altered in direction by moving the shade to the other side.

I have arranged here a radiometer so that it can be made to move by a very faint light, and at the same time its rotation is easily followed by all present. In this bulb is a large six-armed radiometer carrying a mirror in its centre. The mirror is almost horizontal, but not quite so, and therefore when I throw a beam of electric light vertically downwards on to the central mirror, the light is reflected off at a slight angle, and as the instrument rotates its movement is shown by the spot of light travelling round the ceiling in a circle. Here again the fog helps us, for it gives us an imponderable beam of light moving round the room like a solid body, and saving you the trouble of looking up to the ceiling. I now set the radiometer moving round by the light of a candle, and I want to show you that coloured light does not very much interfere with the movement. I place yellow glass in front, and the movement is scarcely diminished at all. Very deep coloured glass, you see, diminishes it a little more. Blue and green glass make it go a little slower, but still do not diminish the speed one-half. I now place a screen of water in front: the instrument moves with diminished velocity, rotating with about one-fourth its original speed.

Taking the action produced by a candle flame as	100
Yellow glass reduces it to	89
Red " " "	71
Blue " " "	56
Green " " "	56
Water " " "	26
Alum " " "	15

I now move the candle a little distance off, so as to make the instrument move slower, and bring a flask of boiling water close to it. See what happens. The luminous index no longer moves steadily, but in jerks. Each disc appears to come up to the boiling water with difficulty, and to hurry past it. More and more sluggishly do they move past, until now one has failed to get by, and the luminous beam, after oscillating to and fro a few times, comes to rest. I now gradually bring the candle near. The index shows no movement. Nearer still. There is now a commencement of motion, as if the radiometer was trying to push past the resistance offered by the hot water; but it is not until I have brought the candle to within a few inches of the glass globe that rotation is recommenced. On these pith radiometers the action of dark heat is to repel the black and white surfaces almost equally, and this repulsion is so energetic as to overcome the rotation caused by the candle, and to stop the instrument.

With a radiometer constructed of a good conductor of heat, such as metal, the action of dark heat is different. Here is one made of silvered copper, polished on one side and lampblacked on the other. I have set it moving with a candle slightly the normal way. Here is a glass shade heated so that it feels decidedly warm to the hand. I cover the radiometer with it, and the rotation first stops, and then recommences the reverse way. On removing the hot shade the reverse movement ceases, and normal rotation recommences.

If, however, I place a hot glass shade over a pith radiometer the arms at once revolve the normal way, as if I had exposed the instrument to light. The diametrically opposite behaviour of a pith and a metal instrument when exposed to the dark heat radiated from a hot glass shade is very striking. The explanation of the action is not easy, but it depends on the fact that the metal is one of the best conductors of heat, whilst pith is one of the worst.

One more experiment with this metallic radiometer. I heat it strongly with a spirit lamp, and the arms spin round rapidly. Now the whole bulb is hot, and I remove the lamp: see what happens. The rotation quickly diminishes. Now it is at rest; and now it is spinning round just as fast the reverse way. I can procure this reverse movement only with difficulty with a pith instrument. The action is due to the metal being a good conductor of heat. As it absorbs heat it moves one way; as it radiates heat it moves the opposite way.

At first I made these instruments of the very lightest material possible, some of them not weighing more than half a grain; and where extreme sensitiveness is required lightness is essential. But the force which carries them round is quite strong enough to move a much greater weight. Thus the metallic instrument I have just experimented with weighs over 13 grains, and here is one still heavier, made of four pieces of looking-glass blacked on the silvered side, which are quickly sent round by the impact of this imponderable agent, and flash the rays of light all round the room when the electric lamp is turned on the instrument.

Before dismissing this instrument, let me show one more experiment. I place the looking-glass and the metal radiometer side by side, and, screening the light from them, they come almost to rest. Their temperature is the same as that of the room. What will happen if I suddenly chill them? I pour a few drops of ether on each of the bulbs. Both instruments begin to revolve. But notice the difference. Whilst the movement in the case of the metal radiometer is direct, that of the looking-glass instrument is reverse. And yet to a candle they both rotate the same way, the black being repelled.

Now, having found that this force would carry round a comparatively heavy weight, another useful application suggested itself. If I can carry round heavy mirrors or plates of copper, I can carry round a magnet. Here, then (Fig. 9), is an instrument carrying a magnet, and outside is a smaller magnet, delicately balanced in a vertical position, having the south pole at the top and the north pole at the bottom. As the inside magnet comes round, the outside magnet, being delicately suspended on its centre, bows backwards and forwards, and, making contact at the bottom, carries an electric current from a battery to a Morse instrument. A ribbon of paper is drawn through the "Morse" by clockwork, and at each contact—at each revolution of the radiometer—a record is printed on the strip of paper by dots; close together if the radiometer revolves quickly, farther apart if it goes slower.

Here the inner magnet is too strong to allow the radiometer to start with a faint light without some initial impetus. Imagine the instrument to be on the top of a mountain away from everybody, and I wish to start it in the morning. Outside the bulb are a few coils of insulated copper wire, and by depressing the key for an instant I pass an electric current from the battery through them. The interior magnet is immediately deflected from its north-south position, and the impetus thus gained enables the light to keep up the rotation. In a proper meteorological instrument I should have an astatic combination inside the bulb, so that a very faint light would be sufficient to start it, but in this case I am obliged to set it going by an electric current. I have placed a candle near the magnetic radiometer. I now touch the key; the instrument immediately responds; the paper unwinds from the Morse instrument, and on it you will see dots in regular order. I put the candle 8 inches off, and the dots come wide apart. I place it 5¾ inches off, and two dots come where one did before. I bring the candle 4 inches from the instrument, and the dots become four times as numerous (Fig. 10), thus recording automatically the intensity of the light falling on the instrument, and proving that in this case also the radiometer obeys the law of inverse squares.

FIG. 9.

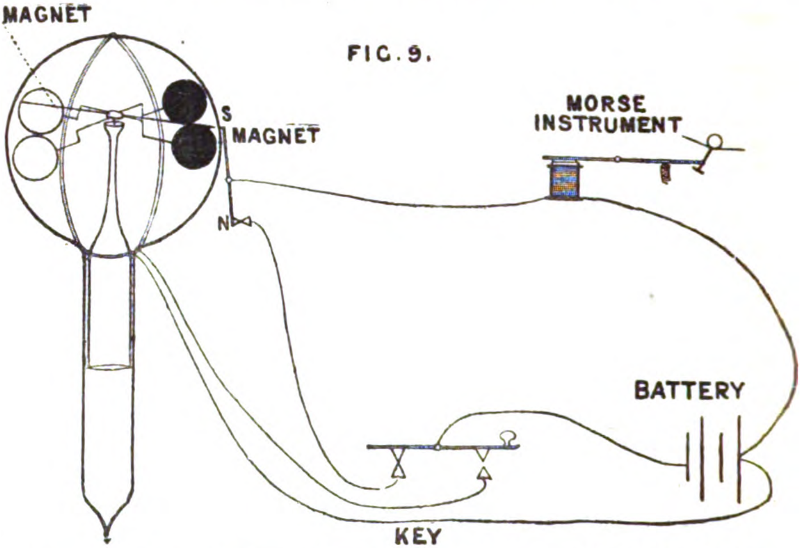
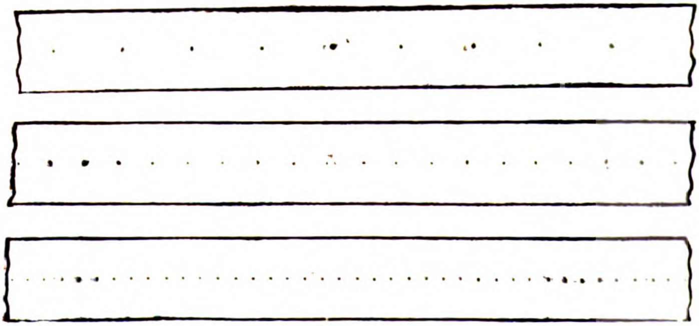


FIG. 10.



This instrument, the principle of which I have illustrated to-night, is not a mere toy or scientific curiosity, but is capable of giving much useful information in climatology. You are well aware that the temperature, the rainfall, the atmospheric pressure, the direction and force of the wind, are now carefully studied in most countries, in order to elucidate their sanitary condition, their animal and vegetable productions, and their agricultural capabilities. But one most important element, the amount of light received at any given place, has been hitherto but very crudely and approximately estimated, or rather guessed at. Yet it cannot be denied that sunlight has its effect upon life and health, vegetable, animal, and human, and that its relative amount at any place is hence a point of no small moment. The difficulty is now overcome by such an instrument as this. The radiometer may be permanently placed on some tall building, or high mountain, and, by connecting it by telegraphic wires to a central observatory, an exact account can be kept of the proportion of sunlight received in different latitudes, and at various heights above the sea level. Furthermore, our records of the comparative temperature of different places have been hitherto deficient. The temperature of a country depends partly on the amount of rays which it receives direct from the sun, and partly on the atmospheric and oceanic currents, warm or cold, which sweep over or near it. The thermometer does not discriminate between these influences; but the radiometer will enable us now to distinguish how much of the annual temperature of a place is due to the direct influence of the sun alone, and how much to the other factors above referred to.

I now come to the last question which I stated at the beginning of this discourse, "What is the amount of force exerted by radiation?" Well, I can calculate out the force in a certain way, from data supplied by this torsion apparatus (Fig. 4). Knowing the weight of the beam, the power of the torsion fibre of glass, its time of oscillation, and the size of the surface acted on, it is not difficult to calculate the amount of force required to deflect the beam through a given angle; but I want to get a more direct measure of the force. I throw a ray of light upon one of these instruments, and it gives a push; surely it is possible to measure the amount of this push in parts of a grain. This I have succeeded in doing in the instrument behind me; but before showing the experiment I want to illustrate the principle upon which it depends. Here is a very fine glass fibre suspended from a horizontal bar, and I wish to show you the strength of it. The fibre is only a few thousandths of an inch thick; it is about 3 feet long, and at the lower end is hanging a scale-pan, weighing 100 grains. So I start with a pull of 100 grains on it. I now add little lead weights, 50 grains each, till it breaks. It bears a pull of 750 grains, but gives way when additional weight is added. You see then the great strength of a fibre of glass, so fine as to be invisible to all who are not close to it, to resist a tensile strain.

Now I will illustrate another equally important property of a glass thread, viz. its power to resist torsion. Here is a still finer glass thread, stretched horizontally between two supports: and in order to show its position I have put little jockeys of paper on it. One end is cemented firmly to a wooden block, and the other end is attached to a little instrument called a counter—a little machine for registering the number of revolutions. I now turn this handle till the fibre breaks, and the counter will tell me how many twists I have given this fibre of glass. You see it breaks at twenty revolutions. This is rather a thicker fibre than usual. I have had them bear more than 200 turns without breaking, and some that I have worked with are so fine that if I hold one of them by the end it curls itself up and floats about the room like a piece of spider's thread.

Having now illustrated these properties of glass fibres, I will try to show a very delicate experiment. I want to ascertain the amount of pressure which radiation exerts on a blackened surface. I will put a ray of light on the pan of a balance, and give you its weight in grains; for I think in this Institution and before this audience I may be allowed a scientific use of the imagination, and may speak of weighing that which is not affected by gravitation.

The principle of the instrument is that of W. Ritchie's torsion balance, described by him in the 'Philosophical Transactions' for 1830. The construction is somewhat complicated, but it can be made out on reference to the diagram (Fig. 11). A light beam, A B, having 2 square inches of pith, C, at one end, is balanced on a very fine fibre of glass, D D′, stretched horizontally in a tube; one end of the fibre being connected with a torsion handle, E, passing through the tube, and indicating angular movements on a graduated circle. The beam is cemented to the torsion fibre, and the whole is enclosed in glass and connected with the mercury pump by a spiral tube, F, and exhausted as perfectly as possible. G is a spiral spring, to keep the fibre in a uniform state of tension. H is a piece of cocoon silk. I is a glass stopper, which is ground into the tube as perfectly as possible, and then highly polished and lubricated with melted indiarubber, which is the only substance I know that allows perfect lubrication and will still hold a vacuum. The pith, C, represents the scale-pan of the balance. The cross-beam, A B, which carries it, is cemented firmly to the thin glass fibre, D, and in the centre is a piece of mirror, K. Now the cross-beam A B and the fibre D being rigidly connected together, any twist which I give to the torsion handle E will throw the beam out of adjustment. If, on the other hand, I place a weight on the piece of pith C, that end of the beam will fall down, and I shall have to turn the handle, E, round and round a certain number of times, until I have put sufficient torsion on the fibre D to lift up the beam. Now, according to the law of torsion, the force with which a perfectly elastic body like glass tends to untwist itself is directly proportional to the number of degrees through which it has been twisted: therefore, knowing how many degrees of torsion I must put on the fibre to lift up the $\frac{1}{100}$ of a grain weight, I can tell

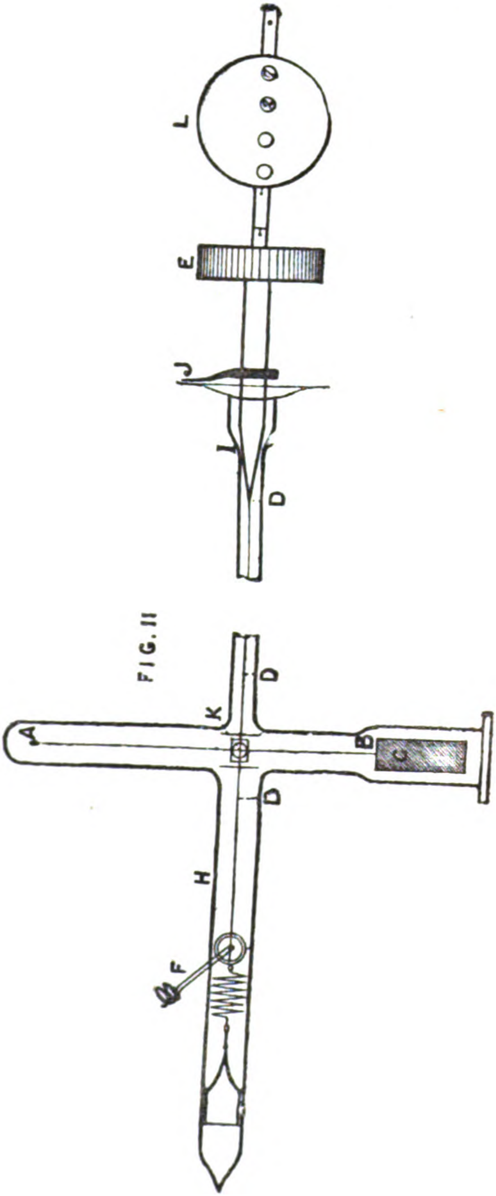

L
E
J
I
D
K
A
B
C
D
D
H
F

FIG. 11

how many degrees of torsion are required to lift up any other weight; and conversely, putting an unknown weight or pressure on the pith, I can find its equivalent in grains by seeing how much torsion it is equal to. Thus, if $\frac{1}{100}$ of a grain requires 10,000 degrees of torsion, $\frac{1}{50}$ of a grain would require 20,000 degrees; and conversely, a weight which required 5000 degrees torsion would weigh $\frac{1}{200}$ of a grain. Once knowing the torsion equivalent of $\frac{1}{100}$ of a grain, the ratio of the known to the unknown weights is given by the degrees of torsion.

Having thus explained the working of the torsion balance I will proceed to the actual experiment. On the central mirror I throw a ray from the electric light, and the beam reflected on a particular spot of the ceiling will represent zero. The graduated circle J of the instrument also stands at zero, and the counter which I fasten on at the end L stands at O. The position of the spot of light reflected from the little concave mirror being noted, the torsion balance enables me to estimate the pressure or weight of a beam of light to a surprising degree of exactness. I lift up my little iron weight by means of a magnet (for working in a vacuum I am restricted in the means of manipulating), and drop it in the centre of the pith: it knocks the scale-pan down, as if I had placed a pound weight upon an ordinary balance, and the index-ray of light has flown far from the zero-point on the ceiling. I now put torsion on the fibre to bring the beam again into equilibrium. The index-ray is moving slowly back again. At last it is at zero, and on looking at the circle and counter I see that I have had to make 27 complete revolutions and 301 degrees, or $27 \times 360^\circ + 301^\circ = 10{,}021^\circ$, before the force of torsion would balance the $\frac{1}{100}$ of a grain.

I now remove the weight from the pith-pan of my balance, and liberate the glass thread from torsion by twisting it back again. Now the spot of light on the ceiling is at zero, and the counter and index are again at O.

Having thus obtained the value of the $\frac{1}{100}$ of a grain in torsion degrees, I will get the same for the radiation from a candle. I place a lighted candle exactly 6 inches from the blackened surface, and on removing the screen the pith scale-pan falls down, and the index-ray again flies across the ceiling. I now turn the torsion handle, and in much less time than in the former case the ray is brought back to zero. On looking at the counter I find it registers four revolutions, and the index points to 188 degrees, making altogether $360^\circ \times 4 + 188 = 1628^\circ$, through which the torsion fibre has to be twisted to balance the light of the candle.

It is an easy calculation to convert this into parts of a grain weight; 10,021 torsion degrees representing 0·01 grain, 1628 torsion degrees represent 0·001624 grain.

$$10{,}021^\circ : 0{\cdot}01 \text{ grain} :: 1628^\circ : 0{\cdot}001624 \text{ grain}.$$

The radiation of a candle 6 inches off, therefore, weighs or presses the 2 square inches of blackened pith with a weight of 0·001624 grain.

In my own laboratory, working with this torsion balance, I found that a candle 6 inches off gave a pressure of 0·001772 grain. The difference is only 0·000148 grain, and is fairly within the allowable limits of a discourse experiment. But this balance is capable of weighing to far greater accuracy than that. You have seen that a torsion of 10,021° balanced the hundredth of a grain. If I give the fibre 1 degree more twist the weight is over-balanced, as shown by the movement of the index-ray on the ceiling. Now 1 degree of torsion is about the $\frac{1}{10000}$ part of the whole torsion required by the $\frac{1}{100}$ grain. It represents therefore the $\frac{1}{10000}$ part of the $\frac{1}{100}$, or the millionth part of a grain.

Divide a grain weight into a million parts, place one of them on the pan of the balance, and the beam will be instantly depressed.

Weighed in this balance the mechanical force of a candle 12 inches off was found to be 0·000444 grain; of a candle 6 inches off, 0·001772 grain. At half the distance the weight of radiation should be four times, or 0·001776 grain; the difference between theory and experiment being only four-millionths of a grain is a sufficient proof that the indications of this instrument, like those of the apparatus previously described, follow the law of inverse squares. An examination of the differences between the separate observations and the mean shows that my estimate of the sensitiveness of this balance is not excessive, and that in practice it will safely indicate the millionth of a grain.

I have only had one opportunity of getting an observation of the weight of sunlight: it was taken on December 13th, but the sun was so obscured by thin clouds and haze that it was only equal to 10·2 candles 6 inches off. Calculating from this datum, it is seen that the pressure of sunshine is 2·3 tons per square mile.

But however fair an equivalent ten candles may be for a London sun in December, a midsummer sun in a cloudless sky has a very different value. Authorities differ as to its exact equivalent, but I underestimate it at 1000 candles 12 inches off.

Let us see what pressure this will give:—A candle 12 inches off, acting on 2 square inches of surface, was found equal to 0·000444 grain; the sun, equalling 1000 candles, therefore gives a pressure of 0·444000 grain; that is, equal to about 32 grains per square foot, to 2 cwt. per acre, 57 tons per square mile, or nearly three thousand million tons on the exposed surface of the globe—sufficient to knock the earth out of its orbit if it came upon it suddenly.

It may be said that a force like this must alter our ordinary ideas of gravitation; but it must be remembered that we only know the force of gravity as between bodies such as they actually exist, and we do not know what this force would be if the temperatures of the gravitating masses were to undergo a change. If the sun is gradually cooling, possibly its attractive force is increasing, but the rate will be so slow that it will probably not be detected by our present means of research.

Whilst showing this experiment I wish to have it distinctly understood that I do not attach the least importance to the actual numerical results. I simply wish to show you the marvellous sensitiveness of the apparatus with which I am accustomed to work. I may, indeed, say that I know these rough estimates to be incorrect. It must be remembered that our earth is not a lampblacked body enclosed in a glass case, nor is its shape such as to give the maximum of surface with the minimum of weight. The solar forces which perpetually pour on it are not simply absorbed and degraded into radiant heat, but are transformed into the various forms of motion we see around us, and into the countless forms of vegetable, animal, and human activity. The earth, it is true, is poised in vacuous space, but it is surrounded by a cushion of air; and, knowing how strongly a little air stops the movement of repulsion, it is easy to conceive that the sun's radiation through this atmospheric layer may not produce any important amount of repulsion. It is true the upper surface of our atmosphere must present a very cold front, and this might suffer repulsion by the sun; but I have said enough to show how utterly in the dark we are as to the cosmical bearings of this action of radiation, and further speculation would be but waste of time.

It may be of interest to compare these experimental results with a calculation made in 1873, before any knowledge of these facts had been made public.

Professor Clerk Maxwell, in his 'Electricity and Magnetism,' vol. ii. p. 391, writes as follows: "The mean energy in one cubic foot of sunlight is about 0·0000000882 of a foot-pound, and the mean pressure on a square foot is 0·0000000882 of a pound weight. A flat body exposed to sunlight would experience this pressure on its illuminated side only, and would therefore be repelled from the side on which the light falls."

Calculated out, this gives the pressure of sunlight equal to about 2½ lb. per square mile. Between the 2½ lb. deduced from calculation and the 57 tons obtained from experiment the difference is great; but not greater than is often the case between theory and experiment.

In conclusion, I beg to call especial attention to one not unimportant lesson which may be gathered from this discovery. It will be at once seen that the whole springs from the investigation of an anomaly. Such a result is by no means singular. Anomalies may be regarded as the finger-posts along the high road of research, pointing to the bye-ways which lead to further discoveries. As scientific men are well aware, our way of accounting for any given phenomenon is not always perfect. Some point is perhaps taken for granted, some peculiar circumstance is overlooked. Or else our explanation agrees with the facts not perfectly, but merely in an approximate manner, leaving a something still to be accounted for. Now these residual phenomena, these very anomalies, may become the guides to new and important revelations.

In the course of my research anomalies have sprung up in every direction. I have felt like a traveller navigating some mighty river in an unexplored continent. I have seen to the right and the left other channels opening out, all claiming investigation, and promising rich rewards of discovery for the explorer who shall trace them to their source. Time has not allowed me to undertake the whole of a task so vast and so manifold. I have felt compelled to follow out, as far as lay in my power, my original idea, passing over reluctantly the collateral questions springing up on either hand. To these I must now invite the attention of my fellow-workers in Science. There is ample room for many inquirers.

Nor must we forget that the more rigidly we scrutinize our received theories, our routine explanations and interpretations of nature, and the more frankly we admit their shortcomings, the greater will be our ultimate reward. In the practical world, fortunes have been realized from the careful examination of what has been ignorantly thrown aside as refuse; no less, in the sphere of Science, are reputations to be made by the patient investigation of anomalies.

[W. C.]

WEEKLY EVENING MEETING,

Friday, February 18, 1876.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

C. William Siemens, Esq. D.C.L. F.R.S. M.R.I.

The Action of Light on Selenium.

When, upon former occasions, I have ventured upon this arena, it has been for the purpose of placing before you the results of inquiries of my own into special subjects, which circumstance gave me some title to your indulgence.

This evening I cannot claim the same advantage, because the subject matter which I am about to bring before you is almost entirely the result of the investigations of others, and especially of my brother, Dr. Werner Siemens, who has not the opportunity of addressing you himself.

It is, however, a matter of undoubted interest, and in mentioning some time ago the result of my brother's investigations to my friend Dr. Tyndall, I did so in the hope that he might feel disposed to deal with this subject in his own masterly fashion, and that I should thus procure for the members of the Royal Institution an evening both pleasant and instructive. I did not succeed, however, in obtaining for them such a treat, and it has become my duty to grapple personally with this subject, for which I possess no other qualification than a somewhat intimate acquaintance with a *kindred* subject, that of the influence of heat upon metallic conductors, on which I had the honour of addressing you a few years ago.

Amongst the powers of nature, light seems to be the one which enters least into the composition of matter. The beam of light falling upon the landscape, or upon a work of art, reveals instantly its form to our minds, but with the disappearance of the light its effects seem to vanish entirely: the landscape and the work of art still remain the same, and may be brought back again to our ocular perception, accompanied by all the beautifying effects of light and shade and colour, and yet there seems to be no permanent effect produced in the material condition of the objects before us. Shall we wonder, then, that the true nature of light has remained a mystery more profound than that of the other forces in nature, and that Newton himself exclaimed, in desponding mood, the memorable words, "Nil luce obscurius."

How well does this modest exclamation sit upon the brow of him who has done more to explain the mysteries of light than all other philosophers both before and after his time; and how strangely does it contrast with the self-assurance of his antagonists and critics, amongst whom I cannot refrain from citing Goethe, who, himself a moral philosopher, poet, and reader of the human soul of prodigious power, had taken up a branch of science as a pastime, and was evidently prouder of his misconceptions regarding the nature of light than he was of his 'Faust' or 'Wilhelm Meister.' In his 'Farbenlehre' occurs the following allusion to Newton's exclamation before quoted:

"Es sprach ein grosser Physicus
Mit seinen Schulverwandten,
'Nil luce obscurius!'
Ja wohl für Obscuranten."

While Newton laid down incontrovertible principles regarding the nature of light, it has been reserved for physicists of recent times to prove the effects of light upon solids. One of the most beautiful illustrations of the permanent effects of light upon matter is furnished us through photography; here the ray of light causes the decomposition of compounds of silver in a degree beautifully varying with its intensity.

Another effect of light upon solids is rendered visible by phosphorescent salts, which when acted upon by light continue to glow in various colours for a length of time when taken into a dark room, and I am enabled by the kindness of Mr. Warren De la Rue to show a beautiful series of tubes illustrating this effect.

If anyone required proof that light was a moving force, I would refer him to the discourse delivered in this very place a week ago, when Mr. Crookes gave motion to his radiometer by means of rays of light. But I would go a step-farther, and say that light is perhaps the most potential force in nature, because it covers the earth with trees and vegetation of all kinds. It is true that the mushroom thrives in what appears to us utter darkness; and within the last few days Dr. Higgs has called my attention to a fungus which grows in the deep recesses of the Derbyshire eaves, where it lives without the help of light; but an analysis of this fungus shows that it contains no woody fibre or solid carbon, and so helps to favour the hypothesis *that it is not heat but the ray of light which breaks up carbonic acid in the leaves of plants* in order to separate the carbon. Carbonic acid can indeed be broken up by heat; but it has been shown by Bunsen and De Ville that a temperature of 2500° C. is necessary for its accomplishment—a degree of heat which would at once destroy all vegetable organization.

Different from these effects of light upon solids is that which forms the subject matter of my discourse, viz. the effect of light upon selenium.

Selenium is an elementary body, which was discovered by Berzelius in 1817 in the residues resulting from the distillation of iron pyrites.

It is fusible, combustible, and similar in many other respects to sulphur, phosphorus, and tellurium. It is in fact one of those substances which are placed by chemists upon the border between metals and metalloids, and, like a true borderer, selenium refuses to be amenable to the laws governing either of these natural groups. If melted (at 217°) and cooled rapidly, it presents a brown amorphous mass of conchoidal fracture, which like sulphur and phosphorus is a non-conductor of electricity. But if a stick of this amorphous selenium is exposed for some time to the heat of boiling water, a structural change becomes observable: it assumes a crystalline fracture, and when again inserted in the galvanic circuit it is found to be a conductor of electricity. Professor Adams has shown recently that its conductivity is different when the current passes through in one direction or the other, and it may here also be observed that its conductivity in this form is still very slight, so that a powerful battery and a delicate galvanometer are necessary to show the effect. The same observer has also found, that, contrary to what takes place in metallic conductors, the conductivity of this substance increases with the power of the battery employed, a circumstance which makes it inapplicable as a substitute for resistance coils in the Wheatstone bridge arrangement.

On the 12th February, 1873, the Society of Telegraph Engineers received a communication from one of its members (Mr. Willoughby Smith [*]) to the effect that a stick of crystalline selenium, such as had been used for some time in telegraphy where high electrical resistances were required, offered considerably less resistance to a battery current when exposed to light than when kept in the dark. The statement of this observation, which had first been made by Mr. May, Mr. Willoughby Smith's assistant, stationed at Valentia, was received naturally with some incredulity. Could it be possible that the mere superficial action of light upon a solid substance could so change instantaneously its internal condition as to open among its particles flood-gates for the passage of the electric current, to close again upon the removal of the light? Yet the fact announced by Mr. Willoughby Smith was soon corroborated, first, by the Earl of Rosse, who proved clearly that the action was due solely to light, and afterwards by Lieut. Sale, R.N., whose further researches on this subject are described in the 'Proceedings of the Royal Society,' vol. xxi. p. 283, and in Poggendorff's 'Annalen,' bd. 150, s. 333.

Here the matter rested, when within the last twelve months, it was taken up by two independent inquirers; one in this country and the other in Germany; the one being my friend Professor Adams, of King's College, who has recently communicated the result of his researches to the Royal Society, and the other my brother Dr. Werner Siemens, who has made communications of his results to the Academy of Sciences of Berlin. It is interesting to observe the difference of methods by which each of these two inquirers arrived at results agreeing on many points of fact, while they differ in the deductions drawn from them, and in their application upon other branches of inquiry. I may here mention that when I expressed my willingness to bring this subject before the Royal Institution, I was not aware that Professor Adams was engaged upon it, and under these circumstances I may be excused if I dwell principally upon my brother's experimental researches, with which I am best acquainted, and regarding which he desires me to acknowledge the valuable assistance rendered him by Dr. Obach.

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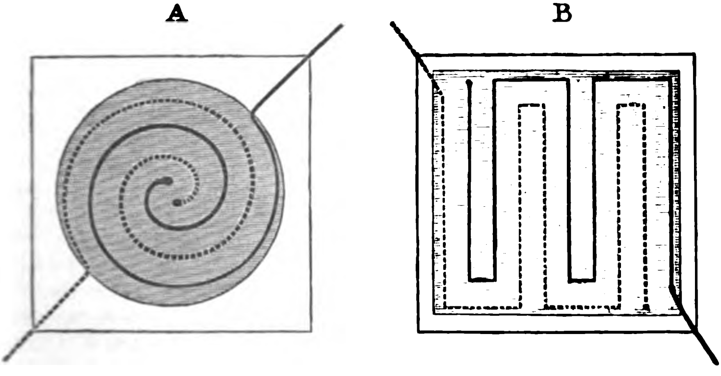
[*] 'Journal of Society of Telegraph Engineers,' vol. ii. p. 31.

One of my brother's achievements in his recent researches consisted in giving to the selenium under observation such a form that the surface action produced by the light attains its maximum effect, and that instead of large galvanic batteries and delicate galvanometers being required to obtain indications, one single Daniell cell and a galvanometer of ordinary construction suffice to produce decided results. His sensitive element is composed in the following manner: Two spirals of thin iron or platinum wire are laid upon a small plate of mica in such a manner that the two wires run parallel without touching each other. While in this position a drop of fluid selenium is made to fall upon the plate, filling the interstices between the wires, and before the selenium has had time to harden, another thin plate of mica is pressed down upon it so as to give firmness to the whole. Instead of spirals of wire, a double grating of wire so arranged that the zigzags of the one wire do not touch the interlacing zigzags of the other is sometimes used, and it will be observed that the size of the whole spirals or gratings hardly exceeds the size of a threepenny piece. These are shown in Diagram No. 1.

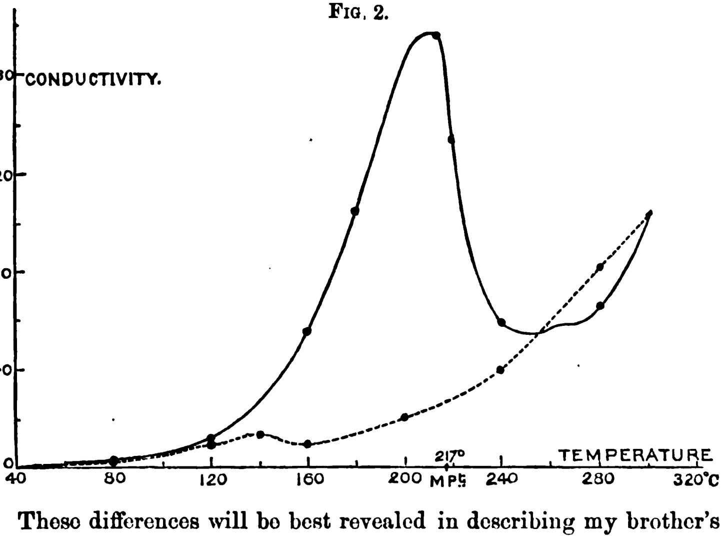
Fig. 1.



The two protruding ends of the two spirals or gratings serve to insert this selenium element in a galvanic circuit. I here hold an element so prepared of amorphous selenium, which I place in a dark box, and insert in a galvanic circuit comprising a Daniell's cell and a delicate galvanometer, the face of which will be thrown upon the screen through a mirror by means of the electric lamp. In closing the circuit it will be seen that no deflection of the needle ensues. We will now admit light upon the selenium disc, and close the circuit, when again no deflection will be observed, showing that the selenium in its present condition is a non-conductor both in the dark and under the influence of light. I will now submit a similar disc of selenium, which has been kept in boiling water for an hour and gradually cooled, to the same tests as before. In closing the circuit while the plate is in the dark, a certain deflection of the galvanometer will be discernible, but I will now open the lid of the box so as to admit light upon the disc, when on again closing the circuit a slight deflection of the galvanometer needle will be observed. In closing the box against the light, this deflection will subside, but will again be visible the moment the light is readmitted to the box. Here we have then the extraordinary effect of light upon selenium clearly illustrated.

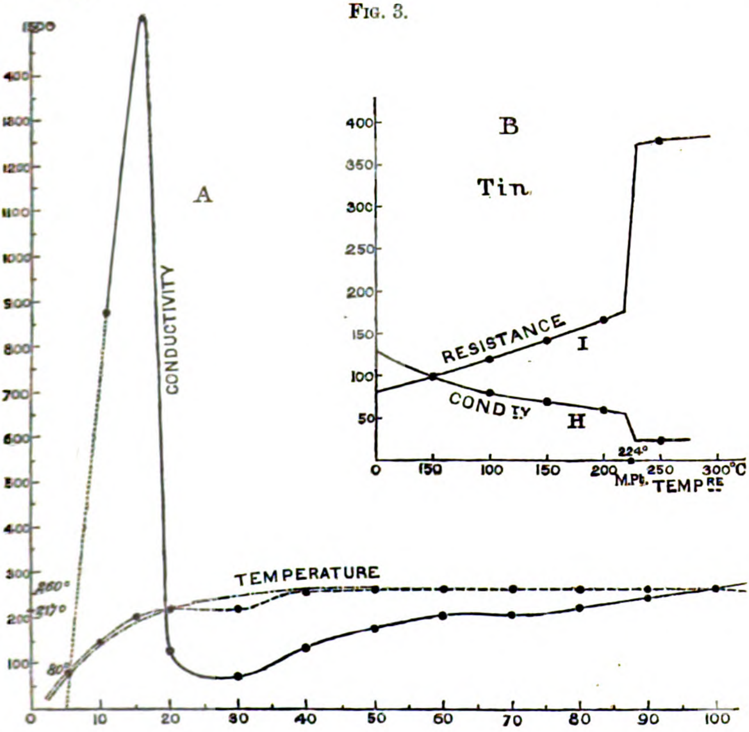
I will now insert into the same circuit another selenium plate which has been heated up to 210° C., and after having been kept at that temperature for several hours has been gradually cooled; it will be observed that this plate is affected to a greater extent than the former by the action of light, and other conditions to which I shall presently allude prove the selenium heated to a higher temperature to be in other respects dissimilar to the other two modifications of the same.

Fig. 2.



These differences will be best revealed in describing my brother's experiment. He placed one of his amorphous preparations of selenium in an air bath heated above the melting point of selenium (to 260° C.), while the connecting wires were inserted in a galvanic circuit consisting of only one Daniell's element and a delicate reflecting galvanometer, and every five minutes the temperature and conductivity of the selenium were noted. The results obtained are shown on the Diagram No. 2, in which the abscissæ represent temperatures, and the ordinates the conductivity of the selenium while in the dark. It will be observed that up to the temperature of 80° C. no current passed, that from this point onward the conductivity of the material rapidly increased until it attained its maximum at the temperature of 210° C., being nearly its melting point, after which an equally rapid diminution of conductivity commenced, reaching a minimum at the temperature of about 240° C., when the conductivity was only such as could be detected by a most delicate galvanometer. In continuing to increase the temperature of the fluid selenium very gradually but steadily, its conductivity increased again. The dotted line shows the conductivity on cooling.

FIG. 3.



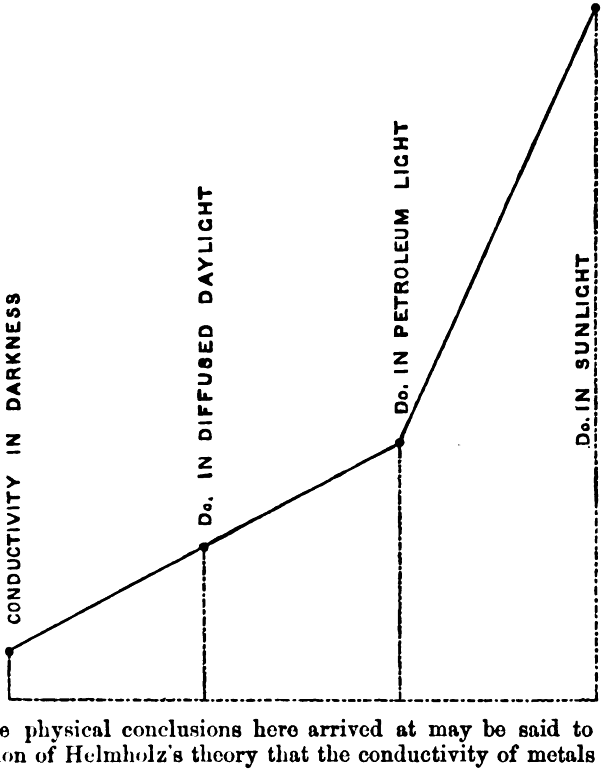
The curves on Diagram No. 3 give a comparison between the effects which actually take place in heating the selenium, and what would take place if the selenium did not melt nor undergo chemical change during heating. In this case the abscissæ represent periods in minutes from the commencement of the plunging of the selenium into the hot bath, while the ordinates represent conductivity and temperature respectively, on the curves, as marked. Up to the temperature of 88° C., the actual and theoretical curve accord exactly, but from this point onward the temperature of the selenium took the lead of that shown on the theoretical curve, firstly proving a spontaneous liberation of heat within the mass, and secondly an absorption of heat during melting, as indicated by the relative positions of the curves at the melting point 217° C. The beginning and end of the two curves correspond exactly. The theoretical curve is marked —.—.—.

The interpretation of these experiments, which are of too delicate and elaborate a nature to be repeated here, is as follows: Amorphous selenium retains a very large amount of specific heat, which renders it a non-conductor of electricity: when heated to 80°, this amorphous solid mass begins to change its amorphous condition for the crystalline form, in which form it possesses a greatly reduced amount of specific heat, giving rise to the increase of temperature beyond that of surrounding objects when the change of condition has once set in. If care is taken to limit the rise of temperature of the selenium to 100° C., and if it is very gradually cooled after having been maintained for an hour or two at that temperature, a mass is obtained which conducts electricity to some extent, and which shows increased conductivity under the influence of light. But in examining the conductivity of selenium so prepared at various temperatures below 80°, and without accession of light, it was found that its *conductivity increases with rise of temperature*, in which respect it resembles carbon, sulphide of metals, and generally the electrolytes. This my brother terms his first modification of selenium. But in extending the heating influence up to 210°, and in maintaining that temperature by means of a bath of paraffin for some hours before gradually reducing the same, he obtained a second modification of selenium in which its conductivity increases with fall of temperature, and in which modification it is therefore analogous to the metals. This second modification of selenium is a better conductor of electricity than the first, and its sensitiveness to light is so great that its conductivity in sunlight is fifteen times greater than it is in the dark, as will be seen from the following table, and in Diagram No. 4, in which is given the effects of different intensities of light on selenium (Modification II.) obtained at Woolwich on the 14th February, 1876:

Selenium in	Relative Conductivities.		Resistance in Ohms.
	Deflections.	Ratio.	
1. Dark	32	1·	10,070,000
2. Diffused daylight	110	3·4	2,930,000
3. Lamplight	180	5·6	1,790,000
4. Sunlight	470	14·7	680,000

Unfortunately, however, this second modification is not so stable as the first; when lowered in temperature parts of it change back into the first or metalloid modification by taking up specific heat, and in watching this effect a point is discovered at which ratio of increase of conductivity with fall of temperature changes sign, or where the electrolyte substance begins to predominate over the metallic selenium. If cooled down to – 15° C., the whole of the metallic selenium is gradually being converted back into the first variety.

FIG. 4.



The physical conclusions here arrived at may be said to be an extension of Helmholz's theory that the conductivity of metals varies inversely as the total heat contained in them. Helmholz had only the sensible heat or temperature (counting from the absolute zero point) in view, but it has already been shown by Hittorff and Werner Siemens that it applies in the case of tin and some other metals also to specific heat and to the latent heat of fusion. In selenium the specific heat is an extremely variable quantity, changing in the solid mass at certain temperatures, and, as is contended, under the influence of light,

Aided by these experimental researches, my brother arrives at the conclusion that the influence of light upon selenium may be explained by a *change of its molecular condition near the surface from the first or electrolyte into the second or metallic modification*, or in other words by a *liberation of specific heat upon the illuminated surface of crystalline selenium*, which liberated heat is reabsorbed when the liberating cause has ceased to act.

Although further proofs will be required before these views can be accepted as being more than a working hypothesis, yet it seems to be favoured by several collateral circumstances.

Foremost among these is the time necessary for the complete dispersion of the effects of light, as will be observed by the very gradual return of the needle to its zero position; and another is found in the circumstance that the effect of light is greatly diminished by long exposure to light, causing a readjustment of specific heat throughout the mass, notwithstanding a continuance of the disturbing cause. The fatigue of the sensitive plate will also be observed by the gradual return of the galvanometer needle toward its zero position when the influence of light is allowed to continue.

This fatigue wears off if the sensitive plate is allowed to rest in darkness for several hours, but even days of such rest is necessary to restore its full previous degree of sensibility.

These conclusions differ, however, from those given by Professor Adams, and which are as follows:

1. That the light falling on the selenium causes an electromotive force in it, in the same direction as the battery current passing through it, the effect being similar to the effect due to polarization in an electrolyte, but in the opposite direction.
2. That the light falling on the selenium causes a change on its surface akin to the change which it produces on the surface of a phosphorescent body, and that in consequence of this change the electric current is enabled to pass more readily over the surface of the selenium.

Time alone can tell which of these conclusions comes nearest to an absolute theory.

To these general results have to be added those regarding the relative influence upon sensitive selenium of different parts of the spectrum, and the experiment which I am about to make will show, if successful, that the actinic ray exercises no sensible effect, that the effect increases as we gradually approach the dark red, and that beyond that point the effect again decreases, reaching almost the zero in the heat rays.

The following table shows the influence of the different coloured rays of the spectrum of a paraffin lamp produced on selenium of the second modification by a prism of bisulphuret of carbon. The following are the different deflections of the galvanometer (conductivities):

Dark.	Ultra-violet.	Violet.	Blue.
139	139	148	158

Yellow.	Red.	Ultra-red.
178	188	180

In approaching the sensitive selenium plate with a dark hot poker, no sensible effect is produced, whereas the same hot poker when brought near a Crookes' radiometer causes it to revolve with great energy, showing that latter instrument is much more dependent upon heat rays than selenium.

Whenever a new discovery in science has been made, men, who delight in calling themselves practical, ask immediately, "and what will be the use of it?" They seem to be insensible to the fact that all increase of our stock of knowledge is of the greatest practical use, and that useful application will follow as a matter of course. The first application of the discovery under consideration which readily suggests itself to every mind is that of constructing a selenium photometer.

If the sensitive selenium was constant in its action under the effect of light, and if temperature did not exercise its influence upon it, the construction of a selenium photometer would present little difficulty. Two equal pieces of sensitive selenium would only have to be exposed, the one to a normal candle and the other to the source of light to be measured, and the distance of the one light or the other be so regulated that the two electrical resistances balanced each other in a Wheatstone bridge arrangement, when the relative distance of the two lights would signify their relative light intensity.

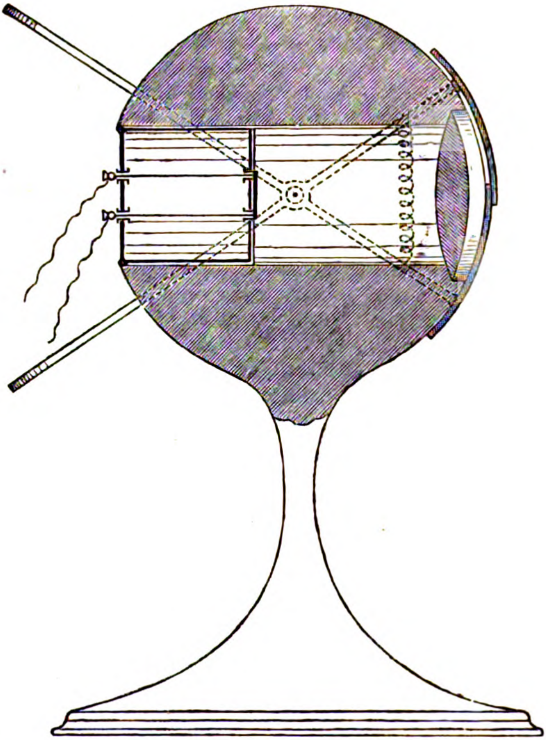
All observers agree that the effect of light upon the conductivity of selenium varies with the square root of the light intensity. Since light itself diminishes in the ratio of the square of the distance, it follows that the square of the respective distances determines (effects being equal) the relative intensity of the two lights.

My brother has constructed a selenium photometer which is not affected by the changes to which that substance is liable, and which consists of a single sensitive plate mounted upon a vertical axis, upon which it can be turned rapidly through a certain angular distance, limited by stops. When touching the one stop the selenium stands opposite the normal candle, and when touching the other opposite the light to be measured, the distance of the former being changed upon a scale until no effect upon the needle of a galvanometer is produced in turning the sensitive plate in rapid succession from the one stop to the other.

The result of this method is absolute for white light, but for coloured lights some allowance must be made for the fact that the dark-red rays exercise relatively a more powerful influence upon selenium than upon our retina; but the difference is not great, and might be compensated by the introduction of a transparent screen absorbing a portion of the red rays (a sheet of glass with a green tinge).

Before concluding, I wish to introduce to your notice a little apparatus which I have prepared to illustrate the extraordinary sensitiveness of my brother's selenium preparations, and an analogy between its action and that of the retina of our eye. It consists of a hollow ball with two openings opposite to each other, the one opening being furnished with a lens of 1½ inch diameter, and the other with an adjustable stopper carrying a sensitive selenium plate, which is connected by wires to a galvanometer and one Daniell cell. The lens is covered by two slides representing eyelids, the ball itself being the body of the eye, and the sensitive plate occupying the place of the retina, which it resembles also in size (see Fig. 5). I will now put a white screen in front of the artificial eye, and throw electric light upon it by means of a reflector. On opening the eyelids, a strong deflection of the galvanometer will be observed. I will now replace the white screen by black, when on opening the lids again hardly any movement of the galvanometer needle will be observed. A blue screen will cause some deflection, a yellow screen a greater one, whereas the greatest deflection, short of that produced by the white screen, will be effected by the red.

Fig. 5.



Here we have then an artificial eye which is sensible to light and to differences of colour, which shows the phenomenon of fatigue if intense light is allowed to act for a length of time, and from which it recovers again by repose in keeping the eyelids closed. It would not be difficult to arrange a contact and electromagnet in connection with the galvanometer in such a manner that a powerful action of light would cause the automatic closing of the eyelids, and thus imitate the spontaneous brain action of blinking the eyelids in consequence of a flash of light. To physiologists this analogy may be suggestive regarding the important natural functions of the human frame.

[C. W. S.]

WEEKLY EVENING MEETING,

Friday, February 25, 1876.

Warren De la Rue, Esq. D.C.L. F.R.S. Manager,
in the Chair.

The Rev. Stephen Joseph Perry, F.R.S.

The Transit of Venus.

The history of the transit of Venus leads us back only a few centuries. Whilst engaged in the study of planetary laws, Kepler discovered that both Mercury and Venus might be seen as black spots on the bright background of the sun in the year 1631, the former in November, and the latter in December. Gassendi witnessed this transit of Mercury, but that of Venus was not seen, as the sun rose in the west of Europe just after the egress of the planet.

For the transit of a planet it is only necessary that its latitude should be less than the sun's semidiameter when the planet is in inferior conjunction, or that the earth and planet should have the same longitude near a node of the planet's orbit. Now Venus and the earth perform their sidereal revolution in 224·701 and 365·256 days respectively; hence in 243 years Venus would go round the sun almost exactly 395 times, the two bodies returning to the same relative positions. A transit in 1631 must therefore be followed by another in 1631 + 243 = 1874, &c. But the time of thirteen revolutions of Venus amounts very nearly to eight years, and Jeremiah Horrocks found that Venus would only change her latitude by 22′ between the conjunctions eight years apart, and that, as the sun's diameter is more than 30′, Venus might be visible eight years after 1631. Horrocks and Crabtree were the only observers of this transit of 1639.

Both the above transits occurred near the ascending node, and 243 years later a similar pair must follow at the same node; but the orbits of the larger planets being almost circular, half that period should bring Venus and the earth into similar relative positions at the opposite, or descending node, and therefore 121½ years after December, 1639, viz. in June, 1761, another transit might be expected.

In 1677 Halley observed a transit of Mercury at St. Helena, and this observation suggested to his mind the possibility of employing the transit of Venus as a most accurate means for determining the exact distance of the sun. A few years later, in a paper read before the Royal Society, he proposed a method of observation, which still bears his name. This consists in recording the exact local time of the ingress and the egress of the planet at two stations, whose difference of latitude, and position with regard to the earth's axis of rotation, render the total durations of the transit as unequal as possible.

The inapplicability of Halley's method to transits that are too centrical, caused Delisle to propose another method, which is equally suited to all transits. His two observers are placed rather east and west, than north and south, and, as far as rotation and obliquity of orbit will permit, almost opposite the extremities of that solar diameter, which passes through the point of contact either at ingress or egress. The observations required at each station are a single contact, and a very accurate longitude determination, so as to obtain the absolute difference of time of contact at the two stations. This difference should of course be made as great as possible, so that any small error in observed time of contact, or in longitude, may bear the smallest possible proportion to the whole.

In June, 1761, the conditions were not favourable for Halley's method, and Delisle's failed from want of accurate longitudes.

The transit which followed eight years later was well observed, and the solar parallax and distance deduced. The observers were numerous, and the computed values differed considerably. Encke, in 1837, combined the results of various nations, and obtained 8″·55 as the solar parallax, which makes the sun's distance more than 95,000,000 miles.

This value was accepted universally as a fair approximation, until other methods of determining the solar parallax raised a most serious doubt as to the accuracy of Encke's result. Foucault's determination of the velocity of light by a revolving mirror, and that of Fizeau by a rotating wheel, combined with the observed difference in time of the phenomena of Jupiter's satellites, or with Bradley's constant of aberration, gave severally 8″·86 and 8″·88 as the solar parallax. Hansen, from the lunar inequalities found 8″·92, and Stone 8″·85. Le Verrier, from 106 years of meridional transits of Venus, from her observed latitude in 1761 and 1769, and from an occultation of a star by Mars, deduced respectively 8″·86, 8″·85, and 8″·87. And finally, the discussion of the parallactic displacements of Mars led Sir

Thomas Maclear, Mr. Stone, and Dr. Winnecke to the values 8″·90, 8″·94, and 8″·96. The close agreement of these numbers induced Mr. Stone to undertake the laborious task of a re-examination of Encke's value. The result of his labours, in which great attention was paid to the description given by each astronomer of what he observed as true contact, changed the solar parallax given by the transit of 1769 from 8″·55 to 8″·91, which is nearly the mean of the values found previously by methods differing so completely in their nature.

The sun's distance enters into the computation of the lunar tables, and this fact alone makes it a matter of the highest moment for a great naval power and a commercial people. Hence early attention was drawn to the preparations necessary for the accurate observation of the transit of 1874 by the astronomical representative of the Admiralty. There were two principal reasons that made it advisable to secure stations well suited for Delisle's method. 1. Only two observations of contact are required, and therefore, supposing the chances of good and of bad weather to be equal, the odds in favour of a complete Delislean observation would be as four to one, compared with Halley's method. 2. Our transit instruments and altazimuths are now so perfectly made that much greater reliance can be placed on the accuracy of our longitudes, even where the telegraph is wanting, than on our contact observations.

For ingress the British stations were the Sandwich Islands and Kerguelen, strengthened by Rodriguez; and for egress, New Zealand and Egypt. For Halley's method, Professor Struve combined with Sir G. Airy; the Russians occupying the north, and the British the south at the three Halleyan stations of Kerguelen, New Zealand, and Rodriguez.

Besides the indirect methods of observed contacts used in former transits, the solar parallax can now be found more directly by actual measures whilst Venus is on the sun, or photographs can be taken during the transit and measured afterwards. The heliometer, for actual measurement of greatest and least distance of limbs, seems to have been used only by the Germans and Russians, and by Lord Lindsay. The British expeditions were provided with double-image micrometers for measuring cusps, and diameters, and least distance of limbs near contact.

Our photographic preparations owe much to Dr. De la Rue and to Captain Abney. We adopted the equatorial mounting, with an image enlarged to four inches, and dry-plate photography, whilst the Americans preferred a fixed telescope and heliostat, without enlargement by eye-piece; and the French chose daguerreotype in preference to photography. An excellent arrangement was devised by Mr. Christie, for carrying out the idea of Dr. Janssen, of taking on the same sensitized plate a succession of photographs, with only second intervals, of the position of Venus near contact.

The only spectroscopes taken out for the observation of external contact at ingress were, as far as is known at present, those of Dr. Janssen, Tacchini, Lord Lindsay, and Captain Tupman, and an instrument constructed specially for our Kerguelen station by Mr. Browning.

The most advantageous moment for taking direct measures with the heliometer, or for obtaining photographs of Venus on the sun, is when the two stations to be compared together are situated on the same radius of the circular section, at the earth's distance, of the right cone enveloping the sun and Venus. Mr. Proctor has clearly pointed out the advantages of this in multiplying the good stations of observation, though many of the best mid-transit stations are equally good for Halley's method.

The active preparations for the transit of 1874 occupied several years. Large telescopes had to be collected, and new instruments devised, before the observatories were constructed, and the observers trained; but thanks to the untiring zeal and energy of Sir G. Airy, Captain Tupman, and the whole Greenwich staff, all was in readiness by the time appointed for departure. The observers for distant stations left England in the May and June that preceded the transit.

The Kerguelen expedition, to which I will now direct special attention for a short time, left in two detachments, which met at the Cape, where Commodore Hewett, V.C., and the Admiralty astronomer, Mr. Stone, did all in their power to assist us. H.M.S. 'Encounter' and 'Supply' had been appointed by the Admiralty to take the astronomers from the Cape to Kerguelen, but an accident had happened to the lifting piece of the screw of the 'Encounter' during the Ashantee war, and when this was known in England, H.M.S. 'Volage' was at once put into commission to replace the 'Encounter.' We left the Cape of Good Hope in the 'Volage' and 'Supply' on September 18th, passed the Crozets on October 2nd, and came almost within sight of Kerguelen on the 6th, when a fearful storm, that carried away a cutter, and drowned many of our live stock, made it impossible to advance for nine-and-forty hours. When the wind abated and the sky cleared, the maps of H.M.S. 'Challenger' enabled us to steer straight into Royal Sound without any difficulty. It was the end of spring, and yet the country was completely covered with snow. We found two sealing schooners in Royal Sound, waiting for a favourable wind to take them to Heard Island. Captain Bailey kindly offered to be our guide, and we soon found a site, which served excellently for our primary station. The anchorage for our vessels was perfect, the landing safe, the foundations for our instruments the solid rock, the supply of water good, all our huts could be placed on the same level, and the spot was protected from the N. and W. gales, without interfering with the horizon. When the heavy work of draining, landing stores, erecting huts, building piers, and fixing the instruments, was well advanced at our principal station, we selected a site for a second observatory, six or seven miles south of the first. Two observers were placed there with a transit and small altazimuth for finding local time and latitude, and with two four-inch telescopes for observing contacts.

The Germans and Americans, who at first intended to occupy Heard Island, were now stationed at Kerguelen; the latter in Royal Sound near the Prince of Wales' Foreland, some 8 miles N.E. of our first station, and the former at Betsy Cove, 10 miles N. of the Americans. The Crozets had been also abandoned, and thus a large cloud over the east end of Kerguelen might destroy at once all the best Delislean stations for retarded ingress, and the south stations for Halley's method. I determined therefore, if it was at all advisable, to attempt the occupation of Heard Island. Lieutenant Coke generously volunteered to take the observations, and Captain Fairfax prepared the 'Volage' for the trip, which was likely to be a stormy one. We had already made every possible inquiry from the sealers, and their reports were discouraging. Captain Fuller, who is best acquainted with those seas, we had not as yet met, but we expected to see him about two weeks before the transit. His account of the island left us finally no hope of a successful trip, as he told us we should probably have to wait many days before it would be possible to land at all, and even then our instruments, chronometers, &c., would have to be conveyed on shore in the sealing boats through a heavy surf. We were forced, therefore, however reluctant we might be, to give up Heard Island, and to occupy instead a third position on Kerguelen. This proved in the end to be particularly fortunate, as the whole of the transit was observed at the third station, and we learned afterwards from the sealers that the sun was not visible on December 8th at Whisky Bay, the only landing place on Heard Island.

The early morning of the day of the transit was fine at Kerguelen, but the sky gradually clouded, which interfered considerably with the observations. At Station I. Venus was observed until after bisection at ingress; a small cloud then covered the sun completely until after internal contact. Some thirteen photographs, and a few measures of least distance of limbs, and of diameters, were obtained, and internal and external contact, as well as bisection, were fairly observed at egress with the 6- and 4-inch equatorials. At Station II. the sun was well seen at ingress by both observers, but it was cloudy at egress. These results, with the success at Station III., were very satisfactory, considering the reputation for cloud, mist, and high wind enjoyed by this Land of Desolation.

The excellent state of the sky, the good definition of the telescope, and the care taken in the observation, make the time of internal contact at ingress, obtained by Lieutenant Corbet, especially reliable.

Our orders about longitude determinations were very stringent, and our stay on the island was consequently extended to five months, although this necessitated an arrangement about half rations. We obtained for our fundamental longitude nineteen transits of the moon, ninety double altitudes or azimuths, and one occultation. In the meantime the different stations in Kerguelen were connected together by aid of eight special chronometers, and satisfactory gunpowder signals were made for longitude connections on a central island in Royal Sound. These, with a run from the Cape to Kerguelen with all our chronometers, complete the work done to secure satisfactory longitudes.

During our stay the naturalist, the Rev. A. E. Eaton, was indefatigable in collecting the natural products of the island, and at our principal station we carried on, simultaneously with the astronomical work, a very complete series of observations of the elements of the earth's magnetic force, and readings were taken at all the even hours, day and night, of the chief meteorological instruments. The men of the Royal Engineers deserve great credit for the zeal they showed in willingly undertaking this laborious portion of our routine duties. On the advice of Captain Nares a number of rabbits and goats were taken from the Cape and left on the island to propagate. They were doing well when we left.

Two hours after the last observed passage of the moon across the meridian, our two vessels were already under steam; H.M.S. 'Supply' bound for the Cape of Good Hope, and H.M.S. 'Volage' for Ceylon and Aden, *en route* for England. Five months on the Island of Desolation was a sore trial to many, but the kindness and assistance we invariably met with from both Captain Fairfax and Captain Inglis, and from the officers and men, lightened our task very much. Before reaching the equator we passed within 22 miles of the centre of a cyclone. The 'Volage' rolled frequently more than 45°, and the sea poured freely over her hammock nettings, but fortunately we escaped after two days without loss of life. On our homeward journey we availed ourselves of the opportunities afforded of collecting data for the formation of magnetic charts of the declination, dip, and intensity.

With the sole exception of New Zealand, where fine weather was almost a certainty at that season of the year, the observers at the remaining British stations were favoured with excellent weather, and the harvest is proportionately abundant. India was equally successful; and the official astronomers at Melbourne and Sydney can show much that will lessen our disappointment at the failure in New Zealand. But England did not depend solely on her Government expeditions, or on those of her colonies for her share in the work of the late transit. Many private observers, as Mr. Tebbutt and Mr. Hennessy in Australia and India, Admiral Ommanney and Colonel Campbell in Egypt, and others, added their valuable results to those accumulated by official astronomers; but no expedition was more perfectly equipped, or more ably manned, than that which rounded the Cape in the yacht 'Venus,' and we have very great reason to regret that sickness, caused by this journey to Mauritius, has prevented Lord Lindsay from giving us himself a full account of the noble part he took in the transit of 1874.

Unfortunately the very limited time at our disposal will only allow us to do scanty justice to the triumphs of other nations, but we must at least cast a rapid glance at their successes. Russia, with her thirty-two stations, succeeded perfectly at five, partially at eight, and most unfortunately failed wholly at nineteen. France had admirable good fortune at S. Paul's, Nangasaki, and Pekin, but envious clouds interfered wholly at Campbell Island, and partially at Caledonia Island. America was less fortunate in the south than in the north; her best results are chiefly photographic. Germany obtained excellent observations with heliometer and photoheliograph; and Italy, Holland, and Austria added their quota to the total results.

Preparations conducted by such men as Airy and Struve, Puiseux, Auwers, and Newcombe, could not fail of securing the happiest results, when favoured by the fine weather which has fortunately prevailed. But it will be only when the longitudes have been computed, the photographs measured, and the contacts discussed, that we can with any confidence compare the results obtained by Halley and Delisle, by photograph and daguerreotype, by fixed and by movable telescope, by spectroscope and telescope, by indirect and by direct methods; or be able to say to what degree of approximation we have now attained.

But there are many things that we have already learnt from the late observations that are both interesting in themselves, and that will lead to more accurate results in the approaching transit of 1882. The aqueous vapour in the ring round Venus, discovered by Tacchini; the difference between photographic and eye contact, proved by Janssen; the possibility of seeing Venus on the chromosphere, before external contact, without the aid of the spectroscope, also verified by the same astronomer; the absence of ellipticity in the planet; are all useful and instructive additions to our previous knowledge. The difficulty about the ligament and the deformation of Venus, which so much troubled observers of the last century, has almost vanished, and we now find this messenger of darkness replaced by a messenger of light, a bright ring, which threatens to be nearly as troublesome as its predecessor. But difficulties are rarely invincible when we see clearly what we have to meet, and we may confidently expect that the observations of 1874 will not only furnish a close approximation to the solar parallax, but will also, by the lessons it affords, lead to a still closer approximation in 1882.

In this necessarily rapid sketch of what is already known of the observations made on the 8th of December, 1874, many names have doubtless been omitted which might have been mentioned, and full weight could scarcely have been given to many important results touched upon so cursorily. It is hoped, however, that no one will be led to conclude that this arises from any want of a due appreciation of the value of his labours in this field of science.

[S. J. P.]

WEEKLY EVENING MEETING,

Friday, March 3, 1876.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Odling, M.A. F.R.S. M.R.I.

The Paraffins and their Alcohols.

I. Hydrocarbons in General.

Olefiant gas, turpentine spirit, and the beautiful crystalline body naphthalene are familiar examples occurring in the gaseous, liquid, and solid states respectively, of a particular class of combustible substances; and although usually burning imperfectly, that is with a smoky flame, they are alike capable of complete combustion, that is of complete oxidation, by the oxygen of the air.

Instead, however, of burning these substances directly in the air, metallic copper may first be burnt or oxidized in the air, and the combustible substance be then heated with the resulting oxide of copper. In this way, the previously burnt copper becomes unburnt, and the combustible substance completely burnt by the oxygen which it acquires from the oxidized copper,—this oxygen having been originally acquired by the copper from the air in the process of its own oxidation or burning.

Pure hydrogen by being burnt in the air or with oxide of copper furnishes only water, and pure carbon by being completely burnt in the air or with oxide of copper furnishes only carbonic acid,—water and carbonic acid being the burnt or fully oxidized forms of hydrogen and of carbon respectively.

Now olefiant gas, turpentine spirit, and solid naphthalene are alike shown to be constituted of hydrogen and carbon by the circumstance of their alike yielding as products of their burning, water or burnt hydrogen and carbonic acid or burnt carbon.

That they have no other constituents than hydrogen and carbon is shown negatively, by the impossibility of recognizing in them the presence of any other constituent; and positively, by weighing the amounts of water and carbonic acid produced by their burning,—a process of great ease and exactitude when their burning is effected by means of oxide of copper.

For instance, from a gramme of any one of these substances, burnt with oxide of copper, there are produced a weight of water corresponding to a certain quantity of hydrogen and a weight of carbonic acid corresponding to a certain quantity of carbon,—these quantities of hydrogen and carbon taken together amounting to exactly one gramme.

Accordingly, olefiant gas, turpentine, and naphthalene, belong alike to the class of hydrocarbons,—that is, of compounds constituted solely of carbon and hydrogen.

Now while chemists are acquainted with only two compounds of hydrogen and oxygen with each other—namely, water and peroxide of hydrogen; and with only two compounds of carbon and oxygen with each other, namely, half burnt carbon or carbonous oxide, and fully burnt carbon or carbonic acid gas,—the known compounds of carbon and hydrogen with each other may be counted by hundreds.

These hundreds of hydrocarbons differ much from one another in many ways; and especially in their behaviour when submitted to active chemical agents. With the majority of hydrocarbons, exposed under suitable conditions to the action of chlorine for instance, chlorine is absorbed and a compound of the original hydrocarbon with chlorine obtained.

Thus in the special case of olefiant gas, the compound so obtained is an oily liquid, which may not only be shown by analysis to have the composition of olefiant gas plus chlorine, but which, when heated with metallic magnesium for instance, breaks up into its constituent olefiant gas and chlorine—the latter appearing in this case as chloride of magnesium, a compound producible directly by heating the metal in chlorine gas.

And the majority of known hydrocarbons agree with olefiant gas in this respect, namely, that they have the property of uniting directly with chlorine and its analogues; with the compounds formed by the union of chlorine and its analogues with hydrogen; and even, in many cases, under suitable conditions, with hydrogen itself.

The combination of these hydrocarbons with hydrogen is, however, usually effected indirectly. Olefiant gas, for instance, is first combined with chlorine Cl_2, or hydriodic acid HI, and the halogen of the resulting compound then simultaneously withdrawn and replaced by hydrogen, acting in the nascent state.

II. PARAFFINS IN GENERAL.

But certain hydrocarbons are distinguished from the generality by being altogether devoid of this property of entering into combination; and inasmuch as they are further very difficultly attackable in any way by chemical agents, they are called paraffins, or bodies deficient in chemical activity.

Hydrocarbons of this inactive chemical habit are some of them solid, some of them liquid, whether spirituous or oily, and some of them gaseous. The best known of the gaseous paraffins is marsh-gas,—the inflammable gas of coal-mines, and most abundant constituent of ordinary coal-gas. Of the liquid paraffins, the most dense varieties are used as lubricating oils; the intermediate varieties as burning oils, their use in this way being unattended with danger or even offensive smell; while the lightest and most volatile varieties constitute benzoline, a liquid of many uses in the arts, but exceedingly dangerous as a fuel for lamps. The solid paraffins including the curious mineral ozokerite are, as is well known, used largely in the manufacture of candles.

Now it is found by analysis that the proportion by weight of constituent hydrogen in the paraffins, solid, liquid, and gaseous, is larger than the proportion by weight of hydrogen met with in hydrocarbons of any other class; and that it varies from 14·5 per cent. in the most dense solid to 25·0 per cent. in the lightest gas, the proportion increasing regularly with the volatility of the paraffin.

Moreover hydrocarbons, not belonging to the paraffin class, become converted into paraffins by their direct or indirect fixation of additional hydrogen. Thus the compound formed by adding hydrogen to olefiant gas, in exchange for the iodine of the hydriodide of olefiant gas, constitutes the paraffin known as ethane.

The paraffins being then the most highly hydrogenized of hydrocarbons, and the halogens and halogen acids when got to combine with certain other hydrocarbons, occupying but the place of the excessive hydrogen of corresponding paraffins, the inability of the paraffins to enter into combination with hydrogen or its representatives would appear to depend upon their being already saturated with hydrogen; and accordingly they are often spoken of as saturated hydrocarbons.

A very simple relationship, in respect of composition, is found to subsist between the different paraffins one with another. In each and all of them, gaseous, liquid, or solid, the number of their proportions of hydrogen is found to exceed by two proportions, twice the number of their proportions of carbon; or they are each and all of them expressible by the general formula C_nH_{2n+2}.

Two liquid paraffins, for instance, met with in benzoline or paraffin-spirit, boiling the one at 99°, the other at 124°, have received the names of heptane and octane, and are expressed by the formulæ C_7H_{16}, and C_8H_{18} respectively; not, however, as a mere result of analysis, which indeed shows very little difference between them—the one consisting of 84 per cent. carbon and 16 per cent. hydrogen, the other of 84·2 per cent. carbon and 15·8 per cent. hydrogen.

The mode, however, in which the above and similar formulæ are arrived at is very simple. If chloroform or prussic acid, for example, be converted into vapour, it is found that the volume of vapour (reduced to standard temperature and pressure) which contains one gramme by weight of hydrogen, is 22·4 litres; and, further, that this same volume contains also twelve grammes by weight of carbon.

And taking the vapours of a variety of hydrogen compounds, whether or not carbonous also, it is found that 22·4 litres of such a vapour always contain, within the limits of experimental error, either one gramme or two, three, four, five, or six, &c., &c., grammes of hydrogen. Similarly, taking the vapours of a variety of carbon compounds, whether or not hydrogenous also, it is found, even more exactly, that 22·4 litres of such a vapour contain either twelve grammes, or twenty-four, thirty-six, forty-eight, &c., &c., grammes of carbon.

In the case of heptane C_7H_{16}, for instance, it is found that 22·4 litres of its vapour contain sixteen grammes of hydrogen, and eighty-four grammes of carbon; or 16 times as much hydrogen and 7 times as much carbon as are contained in 22·4 litres of the vapour of chloroform $CHCl_3$, or of prussic acid CHN. Whereas in the case of octane C_8H_{18}, 22·4 litres of its vapour are found to contain eighteen grammes of hydrogen and ninety-six grammes of carbon, or 18 times as much hydrogen and 8 times as much carbon as are found in 22·4 litres of the vapour of chloroform or prussic acid.

Moreover, in heptane, it is found possible to form definite new compounds by replacing its hydrogen in the proportion of one or more sixteenths of the whole, but not in the proportion of one or more eighteenths; whereas, in octane, it is found possible to replace its hydrogen in the proportion of one or more eighteenths of the whole, but not in the proportion of one or more sixteenths.

Again from certain similar derivatives of heptane and octane respectively, it is possible to remove by one and the same process exactly one-seventh of the entire carbon in the case of heptane C_7H_{16}, and exactly one-eighth of the entire carbon in the case of octane C_8H_{18}; showing the carbon of the two paraffins to be divisible into sevenths and into eighths respectively.

Other evidence leading to the same conclusion may be adduced; so that altogether the synoptic formulæ, as they are called, of the different paraffins, are the expressions of well-ascertained facts with regard to them.

III. ISOMERIC PARAFFINS.

But there is something of yet further interest to be made out with respect to the paraffins. Thus while the formulæ CH_4, C_2H_6, and C_3H_8, apply each of them, so far as is known, to a single paraffin only, the formula C_4H_{10} applies to two distinct compounds, the formula C_5H_{12} to three distinct compounds, the formula C_6H_{14} to five distinct compounds, the formula C_7H_{16} to eight distinct compounds, and so on.

Considering the case of the fourth paraffin more particularly, there are two perfectly distinct gases, or highly volatile liquids known, of composition expressible by the formula C_4H_{10}; the least volatile of them, or normal butane, boiling at 1° above the freezing point, and the most volatile, known as isobutane, boiling at 15° below the freezing point. Yet not only do equal weights of the two bodies contain exactly the same weights of hydrogen and of carbon respectively, but equal volumes of the vapours of the two bodies also contain the same weights of hydrogen and of carbon respectively. Moreover, the hydrogen of both of them is replaceable in tenths, and the carbon of both of them in fourths.

And the similar derivatives, obtainable from the two butanes by similar processes, present corresponding differences in their properties. Thus the butyric acid obtainable from normal butane is of sp. gr. 981, boils at 163°, and yields a calcium salt which, crystallizing with but one proportion of water, is singular in being more soluble in cold than in hot water; while the butyric acid obtainable from isobutane by exactly the same process, having the same composition and expressed by the same formula $C_4H_8O_2$, has a sp. gr. 959, boils at 154°, is more readily oxidizable than, though into the same products as the other acid, and yields a calcium salt crystallizing with five proportions of water, less soluble than the other salt and observing the usual rule of solubility.

Similarly with regard to pentane, or rather to the pentanes, alike represented by the formula C_5H_{12}. One pentane boils at 38°, another, known as isopentane, at 30°, and the third, which may be called neopentane, as low as 9·5°. The one yields an oily acid boiling at 185°, the other a like acid boiling at 175°, and the third a like acid boiling at 161°, all three acids of composition expressible by the same formula $C_5H_{10}O_2$.

Two or more bodies, having in this way the same composition and unit weight, are said to be isomeric; and this property of isomerism, manifested to such a degree by bodies of such a simple constitution as the paraffins, is certainly one of the most noteworthy phenomena as it is indeed one of the most suggestive problems of modern organic chemistry. And, passing from the paraffins to hydrocarbons in general, the number of bodies of this class with which the chemist has to deal is increased probably fourfold by the number of instances of isomerism met with among them.

IV. PARAFFINS AND ALCOHOLS.

Before considering further the nature of the paraffins, and of the isomerism manifested by the greater number of them, it is advisable to examine the relationship subsisting between the paraffins and a better known and far more active class of bodies, namely, the alcohols; and this relationship may well be considered first in the case of common or ethylic alcohol and its associated paraffin, ethane, —the paraffin producible by the hydrogenation of olefiant gas, and one of the few paraffins of which no isomers are known.

It has been said that the paraffins are not readily acted on by chemical agents; still they are attackable by chlorine, and with a proportionately less degree of difficulty in the case of the simpler members of the series. Accordingly when ethane C_2H_6, and chlorine Cl_2, are mixed together, although the paraffin does not combine directly with the chlorine, it nevertheless reacts with it, and so as to produce two new bodies, one of which is hydrochloric acid ClH, and the other chlorethane C_2H_5Cl,—a kind of ethane in which one proportion of hydrogen is replaced by one proportion of chlorine. Or the chlorine manifests its great tendency to unite with hydrogen, by taking some hydrogen away from the paraffin, subject to the condition of an equivalent quantity of chlorine filling the place of the hydrogen so taken away.

But while chlorine has the property, as illustrated above in the case of a paraffin, of replacing hydrogen directly, proportion for proportion, it can only replace oxygen by an exchange of two proportions of chlorine for one proportion of oxygen, this exchange being usually brought about by means of the very active chemical agent known as perchloride of phosphorus,—an agent which, by acting on a large number of oxidized organic compounds, takes away their oxygen, giving up to them an equivalent quantity of its own chlorine in exchange. Common camphor, for instance, $C_{10}H_{16}O''$, when acted on by perchloride of phosphorus, exchanges its one proportion of oxygen for two proportions of chlorine to form the compound $C_{10}H_{16}Cl_2$.

In the case of alcohol, however, C_2H_6O'', the action is somewhat different. Its one proportion of oxygen is indeed replaced by two proportions of chlorine, but with formation not of one di-chlorinated product, but instead, of two mono-chlorinated products, namely, chlorethane or chloride of ethyl C_2H_5Cl, and hydrochloric acid ClH, the identical products producible from ethane by the action of chlorine.

Now the substance known as chloride of ethyl or chlorethane, whether got from ethane or got from alcohol, is one and the same body, and convertible alike into either of the above two bodies from which it is obtainable. For its reconversion into the paraffin, it suffices to treat it with nascent hydrogen; for its reconversion into the alcohol, to heat it under pressure with dilute alkali. The paraffin and alcohol then, are mutually transformable through the intervention of the chloride producible from either of them.

The relationships of composition and mutual metamorphosis subsisting between the paraffin, chloro-paraffin, and alcohol, are precisely the relationships subsisting between hydrogen, hydrochloric acid, and water,—the action of chlorine on a unit of hydrogen H_2, leading to the production of two units of hydrogen chloride HCl... ClH, and its action on a unit of ethane C_2H_6, leading to the production of two units of ethyl chloride and hydrogen chloride C_2H_5Cl... ClH, jointly. Again, the exchange of chlorine for oxygen in a unit of water, H_2O or HOH, leading to the production of two units of hydrogen chloride, HCl... ClH, its exchange for oxygen in a unit of alcohol, C_2H_6O or C_2H_5OH leads to the production of two units of ethylchloride and hydrogen chloride, C_2H_5Cl... ClH, jointly.

The above kind of relationship subsisting between ethane and common alcohol is found to prevail generally,—every paraffin being similarly associated with its corresponding alcohol. Thus with propane C_3H_8, is associated propylic alcohol C_3H_8O, boiling at 97°; with butane C_4H_{10}, butylic alcohol $C_4H_{10}O$, boiling at 116°; and with isobutane $C_4H_{10}^{\sigma}$, isobutylic alcohol $C_4H_{10}O^{\sigma}$, boiling at 109°. With pentane C_5H_{12}, is associated pentyl alcohol $C_5H_{12}O$, boiling at 137°, and with isopentane $C_5H^{\sigma}_{12}$, isopentyl or amyl alcohol $C_5H_{12}O^{\sigma}$, boiling at 129°. The alcohol corresponding to neopentane $C_5H^{\nu}_{12}$ has not been examined.

V. ALCOHOLS IN GENERAL.

As regards the sources of the different alcohols, although the chief product of the alcoholic fermentation is common alcohol, there is always formed in addition a considerable proportion of isopentyl or amyl alcohol $C_5H_{12}O^{\sigma}$, which constitutes the chief constituent of what in English distilleries is called "faints," and in foreign distilleries "fousel oil." The propylic, isobutylic, and even isohexylic alcohols, C_3H_8O, $C_4H_{10}O^{\sigma}$, and $C_6H_{14}O^{\sigma}$ respectively, are also met with, as minor products of fermentation, and are capable of extraction from crude spirit of different kinds. But the greater number of the alcohols are derived by various processes from other and various sources, and are procured especially by the metamorphosis of their corresponding paraffins.

From the relationship subsisting between the alcohols and paraffins, it follows that the alcohols are as varied in their obvious properties as are the paraffins themselves. The simpler alcohols, typified by common alcohol and wood-spirit, are volatile or spirituous liquids, freely miscible with water and burning with a non-luminous flame; the higher alcohols are oily in appearance, immiscible with water, have high boiling points, and burn with a luminous and even smoky flame; while the yet higher alcohols, as those procurable from spermaceti, Chinese wax, and beeswax, are fusible, crystallizable, combustible solids.

Unlike the paraffins, however, the alcohols, whether spirituous, oily, or solid, are readily oxidizable; and in the case of certain alcohols, which constitute the class of alcohols proper or primary alcohols, the oxidation takes place in this fashion. The first stage of the oxidation results in the formation of an aldehyd—a body of very characteristic properties, differing in composition from the alcohol yielding it by a loss of two proportions of hydrogen. The next stage of the oxidation consists in the conversion of the aldehyd into a volatile acid, by the gain of one proportion of oxygen; while any further oxidation results in the breaking up of the substance into two or more carbon compounds.

Now it is the relationship of the paraffins to this particular variety of alcohols which has been already illustrated. And so far at least as the study of this relationship has gone, there would appear to be as many isomeric primary alcohols as there are isomeric paraffins; and the nature of the difference between the several isomeric members of the one class, whatever that may be, would appear to be also the nature of the difference between the several isomeric members of the other class,—the mutual convertibility of the paraffin and its alcohol not throwing any light upon the question.

VI. Secondary and Tertiary Alcohols.

But the majority of the paraffins are found to be associated each one of them not only with an alcohol of the above-described character, that is to say a primary alcohol, but also, and by a very similar mode of metamorphosis, with an isomeric alcohol of a different character, either of the class known as secondary or of the class known as tertiary alcohols; while in many instances, the paraffins are associated similarly not only with either a secondary or a tertiary alcohol, but with both a secondary and a tertiary alcohol, in addition to a primary alcohol. In other words, some paraffins are associated each with a primary alcohol only, others with a primary and secondary, others with a primary and tertiary, others with a primary, secondary, and tertiary alcohol.

Now the secondary and tertiary alcohols are distinguished from the primary alcohols, or alcohols proper, with which they are respectively isomeric, by a difference in boiling point, and more especially by a difference in the manner of their oxidation. The first stage of the oxidation of secondary alcohols, also called pseudo-alcohols or pseudols, as of the primary alcohols, consists in their deprivation of two proportions of hydrogen; but the products have no longer the characters of bodies belonging to the class of aldehyds, but of bodies belonging to a differently characterized class, that of the ketones. These ketones differ especially from the aldehyds in that they do not undergo any stage of oxidation corresponding to that by which the aldehyds are converted into acids, but undergo a further disruptive oxidation only. The tertiary alcohols, or carbinols proper, do not undergo even the stage of oxidation corresponding to that by which the primary and secondary alcohols are converted into aldehyds and ketones respectively, but suffer a disruptive oxidation only.

Now when ethane is acted upon by chlorine it yields only one monochlor-ethane of boiling point 12·5°, and corresponding to primary ethylic alcohol of boiling point 78·4°, which is indeed the only 2-carbon alcohol known. But propane, under different modes of treatment, is capable of yielding two distinct monochlorides C_3H_7Cl, the one boiling at 46·5°, and corresponding to primary propylic alcohol boiling at 98°, and the other boiling at 37°, and corresponding to pseudo-propylic alcohol boiling at 83°. Similarly butane and pentane each yield two monochlorides,—each a monochloride corresponding to primary butyl and primary pentyl alcohol, boiling at 116° and 137° respectively, and each a monochloride corresponding to pseudo-butyl alcohol and pseudo-pentyl alcohol, boiling at 99° and 119° respectively.

Corresponding, moreover, to isobutane, there is a primary alcohol —isobutyl alcohol, boiling at 109°, and yielding by oxidation isobutyraldehyd and isobutyric acid successively. There is not known, however, any isobutyl pseudo-alcohol yielding by oxidation a ketone, but instead a tertiary alcohol boiling at 82°, and susceptible of a disruptive oxidation only. While corresponding to isopentane is a primary alcohol, isopentyl or amyl alcohol, boiling at 129°, and yielding by oxidation valeraldehyd and valeric acid successively; also isopentyl pseudo-alcohol boiling at 107°, and yielding by oxidation isopentyl ketone; and lastly, isopentyl tertiary alcohol, boiling at 98°, and inoxidizable save into simpler carbon products. This series of relationships is illustrated in the following table.

TABLE I.

H_2	Hydrogen.	Water.		H_2O.
CH_4	Methane	66°	Methylic	CH_4O
C_2H_6	Ethane	78°	Ethylic	C_2H_6O
C_3H_8	Propane	97°	Propylic	C_3H_8O
		83°	Pseudo-propylic	,,
C_4H_{10}	Butane	116°	Butylic	$C_4H_{10}O$
		99°	Pseudo-butylic	,,
	Isobutane	109°	Isobutylic	,,
		82°	Katabutylic	,,
C_5H_{12}	Pentane	137°	Pentylic	$C_5H_{12}O$
		119°	Pseudo-pentylic	,,
	Isopentane	129°	Amylic	,,
		107°	Pseud-amylic	,,
		98°	Kat-amylic	,,
	Neopentane		?	
C_6H_{14}	Hexanes		Hexylic	$C_6H_{14}O$
C_7H_{16}	Heptanes		Heptylic	$C_7H_{16}O$
C_8H_{18}	Octanes		Octylic	$C_8H_{18}O$
$C_{16}H_{34}$	Cetane		Cetylic	$C_{16}H_{34}O$
$C_{27}H_{56}$	Cerane		Cerylic	$C_{27}H_{56}O$
$C_{30}H_{62}$	Melane		Melylic	$C_{30}H_{62}O$
C_nH_{2n+2}	Paraffin		Alcohol	$C_nH_{2n+2}O$

Of associated 2-carbon compounds then, there is only one paraffin and one alcohol known. Of the 3-carbon compounds, there is only one paraffin, but there are two alcohols; while of 4-carbon compounds there are two paraffins and four alcohols. Of 5-carbon compounds there are three paraffins known, corresponding alike to the formula C_5H_{12}; and there exist probably eight alcohols, four primary, three secondary, and one tertiary, corresponding alike to the formula $C_5H_{12}O$; but of these last, two primary, two secondary, and one tertiary alcohol are alone satisfactorily made out. While of 6-carbon compounds, there are probably five paraffins corresponding to the formula C_6H_{14}, and no fewer than sixteen alcohols corresponding alike to the formula $C_6H_{14}O$.

VII. PARAFFIN OXIDATION.

What then is the nature of the difference between primary, secondary, and tertiary paraffin alcohols? How comes it that, being three, there should be no more than three classes of these alcohols? How comes it that while every paraffin yields a primary alcohol, and some paraffins more than one, certain paraffins should yield a primary alcohol only, other paraffins should yield each a secondary alcohol in addition, yet others a tertiary alcohol, and yet others both secondary and tertiary alcohols? And what bearing, if any, has this property of yielding alcohols of different kinds upon the nature and origin of the isomerism manifested by the paraffins furnishing them respectively?

Some light may be thrown on these associated questions, by considering the course of oxidation in the case of the simplest paraffin, namely, marsh-gas. Methane, or marsh-gas, CH_4, like the other paraffins, is not readily susceptible of direct oxidation; but by acting on it with chlorine under suitable conditions, there is produced the chloro-derivative CH_3Cl, which, decomposed by caustic alkali KHO, yields wood-spirit or methyl alcohol CH_4O, together with potassium chloride KCl. The first stage of the oxidation being thus effected, the remaining stages are easy. By exposure of the alcohol to a current of heated air it is converted into formic aldehyd CH_2O; this aldehyd, by further absorption of oxygen, yields formic acid CH_2O_2; and this last is converted by even the feeblest oxygenants into carbon dioxide CO_2.

In the successive formation then of these several products, the first stage consists in an addition of oxygen O, the second in an abstraction of hydrogen H_2, the third in a further addition of oxygen O, and the fourth in a further abstraction of hydrogen H_2. There is not a continuously successive addition of oxygen, or removal of hydrogen, but a succession of one and the other action alternately. Now among paraffins in general the stages of oxidation, so far as they extend, are the same as in marsh-gas. Thus in the case of primary compounds there occur the three stages of plus oxygen, minus hydrogen, and then plus oxygen again; in the case of secondary compounds there occur the two stages of plus oxygen and then minus hydrogen; while in the case of tertiary compounds there is the one stage of plus oxygen only, as set forth in the upper part of the following table.

TABLE II.

		Methyl.	Ethyl.	Propyl.$^{\pi}$	Butyl.$^{\kappa}$
Chloride		CH_3Cl	C_2H_5Cl	C_3H_7Cl	C_4H_9Cl
Paraffin		CH_4	C_2H_6	C_3H_8	C_4H_{10}
Alcohol	$+ O$	CH_4O	C_2H_6O	C_3H_8O	$C_4H_{10}O$ Carbinol.
Aldehyd	$- H_2$	CH_2O	C_2H_4O	C_3H_6O Ketone.	
Acid	$+ O$	CH_2O_2	$C_2H_4O_2$ Acid.		
Carboxide	$- H_2$	CO_2			
Paraffin		CH_4	XCH_3	YCH_2	ZCH
Alcohol	$+ O$	$CH_3(HO)'$	$XCH_2(HO)'$	$YCH(HO)'$	$ZC(HO)'$
Aldehyd	$- H_2$	CH_2O''	$XCHO''$	YCO''	
Acid	$+ O$	$CH(HO)'O''$	$XC(HO)'O''$		
Carboxide	$- H_2$	CO_2''			

It is observable that in the case of marsh-gas, there are four proportions of hydrogen and four stages of oxidation to be dealt with; and this association of four stages of oxidation with four proportions of hydrogen becomes more suggestive when it is found, on further study, that the four stages of oxidation, though on the face of them alternate, are in reality successive, and consist, not indeed in four successive additions of oxygen, but in four successive substitutions of oxygen for the four proportions of hydrogen in marsh-gas,—in a replacement, that is to say, of the first proportion of hydrogen by a proportion of oxygen half saturated with hydrogen $(HO)'$, then in a replacement of two proportions of hydrogen by one proportion of unsaturated or diad oxygen O'', then in a replacement of three proportions of hydrogen by one proportion of half-saturated oxygen $(HO)'$ and one proportion of diad oxygen O'', jointly—and lastly in a replacement of four proportions of hydrogen by two proportions of diad oxygen $O'' + O''$, jointly.

Now what has been established with regard to the nature of the successive stages of marsh-gas oxidation has also been established with regard to the successive stages, so far as they proceed, of paraffin oxidation in general. In the indirect oxidation then of a paraffin into its alcohol, whether primary, secondary, or tertiary, there is an actual substitution effected for one proportion of hydrogen in the paraffin. In the further direct oxidation of the primary and secondary alcohol into aldehyd and ketone respectively, there is an actual substitution effected for two proportions of hydrogen in the paraffin;

and in the further direct oxidation of the aldehyd into its acid, there is an actual substitution effected for three proportions of hydrogen in the paraffin. But why, in the case of marsh-gas, there should be four successive substitutions of its four proportions of hydrogen, while in the case of other paraffins with six, eight, ten, twelve, and more proportions of hydrogen, the series of substitutions should be limited to three, two, and even one proportion of hydrogen, still remains to be accounted for.

Let it be imagined, however, that in some particular paraffin there exists the grouping CH as a distinct constituent of the paraffin, and further that, by the action of chlorine, it is the hydrogen of this particular grouping that is replaced so as to form the derived grouping CCl. Let it be imagined that, in another case, there exists the grouping CH_2, and that, by the action of chlorine, it is the hydrogen of this grouping that is replaced, so as to form the derived grouping CHCl; and lastly, let it be imagined that, in another case, there exists the grouping CH_3, and that, in the action of chlorine, it is the hydrogen of this grouping that is replaced, so as to form the derived grouping CH_2Cl,—on the substitution, in these several derived groupings, of their chlorine by half-saturated oxygen, there will result the oxidized groupings C(HO), CH(HO), and $CH_2(HO)$; and since, in the paraffins, direct oxidation does not occur save where indirect oxidation has initiated, it is intelligible that in the case of the grouping CH, the oxidation or substitution should cease at the indirect or alcoholic stage C(HO); while in the case of the group CH_2, there should be first the indirect or alcoholic stage CH(HO), and then the direct or ketonic stage CO''; while in the case of the group CH_3, there should be first the indirect or alcoholic stage $CH_2(HO)'$, and then the direct aldehydic and acid stages, CHO'' and $C(HO)'O''$, in succession—as set forth in the lower part of the preceding table.

VIII. SYNTHESIS OF PARAFFINS.

It remains to be considered whether or not the several differently oxidizable paraffins are actually constituted of the groupings CH_1, CH_2, and CH_3. This is a question to be decided more particularly by a study of their modes of formation—not indeed as conducted on a large scale in the process of destructive distillation, but as effected by definite synthetic reactions taken part in, as will appear, by certain halogen derivatives of the paraffins. Now it is well known that hydrogen, though in most of its chemical relations a somewhat inactive element, is not inactive in respect to chlorine and the halogens. Accordingly, when a paraffin such as ethane C_2H_6, is acted upon by chlorine, the chlorine takes away hydrogen from the paraffin to produce hydrochloric acid; and conversely, when nascent hydrogen acts on a chloro-paraffin such as chlorethane C_2H_5Cl, the hydrogen takes away chlorine from the chloro-paraffin to form hydrochloric acid — the two abstractions of hydrogen and chlorine respectively being alike attended by a substitution of the other element for the one taken away, as already illustrated.

Unlike hydrogen, however, chlorine and the halogens are possessed of a very general activity; and accordingly, when substituted directly or indirectly for the hydrogen of a paraffin, are capable of being removed, not only by hydrogen but by many of the metals. But while under certain conditions, so far at least as the final result is concerned, the metal behaves like hydrogen, abstracting the halogen and taking its place, under other conditions it effects a mere removal of the halogen, this removal being attended however by a phenomenon of a very remarkable kind—namely, the formation of a new and more complex paraffin, by the combination of two hydrocarbon residues with each other. Thus when iodethane C_2H_5I is acted on by mercury under the influence of sunlight, there is produced not the residue or radical C_2H_5, but instead the paraffin, butane, C_4H_{10}. Similarly when iodomethane CH_3I is acted on by sodium, there is produced not the radical CH_3, but instead the paraffin, ethane, C_2H_6; while when a mixture of iodethane C_2H_5I, and iodomethane CH_3I, is acted upon by sodium, there is produced not a mixture of the two radicals C_2H_5 and CH_3, but instead the paraffin, propane, C_3H_8, and so in other cases.

Arguing then from its above mode of formation, ethane is $H_3C.CH_3$; iodethane is consequently $IH_2C.CH_3$, or $H_3C.CH_2I$; and propane, resulting as it does from the reaction between iodethane $IH_2C.CH_3$ and iodomethane CH_3I, is $H_2C.CH_3,CH_3$. Or, so far as regards their above mode of formation, both ethane and propane are really constituted in part by a grouping CH_3; and, through the intervention of a chloro-derivative of this grouping, CH_2Cl, should yield each of them the three successive oxidation products, namely, alcohol, aldehyd, and acid,—with the formation of which last product the stages of oxidation should (save by an initiatory substitution of some further hydrogen by halogen) come to an end, in each instance, as is actually the fact.

But while ethane is constituted solely of the group CH_3 or H_3C, twice repeated, propane is constituted in part of a grouping H_2C or CH_2; and through the intervention of a chloro-derivative of this grouping, $ClHC$, should yield the series of only two successive oxidation products, namely, pseudo-alcohol and ketone. What the fact is, has been already stated, namely, that while ethane furnishes only one monochloro-derivative—this monochloride, howsoever produced, corresponding to an alcohol, oxidizable into an aldehyd and acid successively—propane is capable of furnishing two distinct monochlorides, C_4H_7Cl and $C_4H_7{}^{*}Cl$, distinguishable from one another by the difference in their boiling points; and of these two chloropropanes, the one of highest boiling corresponds to an alcohol oxidizable into an aldehyd and acid successively, while the one of lowest boiling point corresponds to a pseudo-alcohol oxidizable into a ketone only.

But not from the above considerations only may the chloride of higher boiling point be inferred to contain the grouping CH_2Cl, and that of lower boiling point, the grouping $CHCl$ or $ClHC$; for the chloride of higher boiling point is actually producible by the introduction and subsequent metamorphosis of the group CN''', while the chloride of lower boiling point is similarly producible by the introduction and metamorphosis of the group CO'', the single proportion of nitrogen being representative of three, and the single proportion of oxygen being representative of two proportions of halogen or hydrogen, as is well known. These relationships are illustrated in the following formulæ:

$H_2C.CH_3,CH_3$	Propane.	$H_2C.CH_3,CH_3$	Propane.
$ClHC.CH_3,CH_3$	Chloropropane.	$H_2C.CH_2Cl,CH_3$	Chloropropane.
$(HO)HC.CH_3,CH_3$	Propyl-pseudol.	$H_2C.CH_2(OH)',CH_3$	Propyl-alcohol.
$O''C.CH_3,CH_3$	Acetone.	$H_2C.CN''',CH_3$	Propionitrile.

The reaction between iodomethane and iodethane, with abstraction of their iodine by metal, resulting in the formation of propane $H_2C.CH_3,CH_3$, the similar reaction between iodomethane CH_3I and each of the two iodopropanes, $H_2C.CH_2I,CH_3$ and $IHC.CH_3,CH_3$, should furnish the two butanes $H_2C.CH_2.CH_3,CH_3$ and $HC.CH_3,CH_3.CH_3$; each of them constituted in part by a grouping CH_3, and capable of furnishing a distinct primary alcohol further oxidizable into aldehyd and acid; the former of them also constituted in part of the grouping H_2C, and capable of furnishing also a pseudo-alcohol oxidizable into a ketone only; the latter of them not constituted at all of the grouping H_2C, or capable of furnishing a pseudo-alcohol oxidizable into a ketone, but constituted in part of the grouping HC, and capable of furnishing a tertiary alcohol oxidizable only into simpler carbon products,—results which have in each case been satisfactorily established by experiment.

As with propane, so with butane, save that not merely two but even four isomeric iodobutanes are known, expressible by the following formulæ:

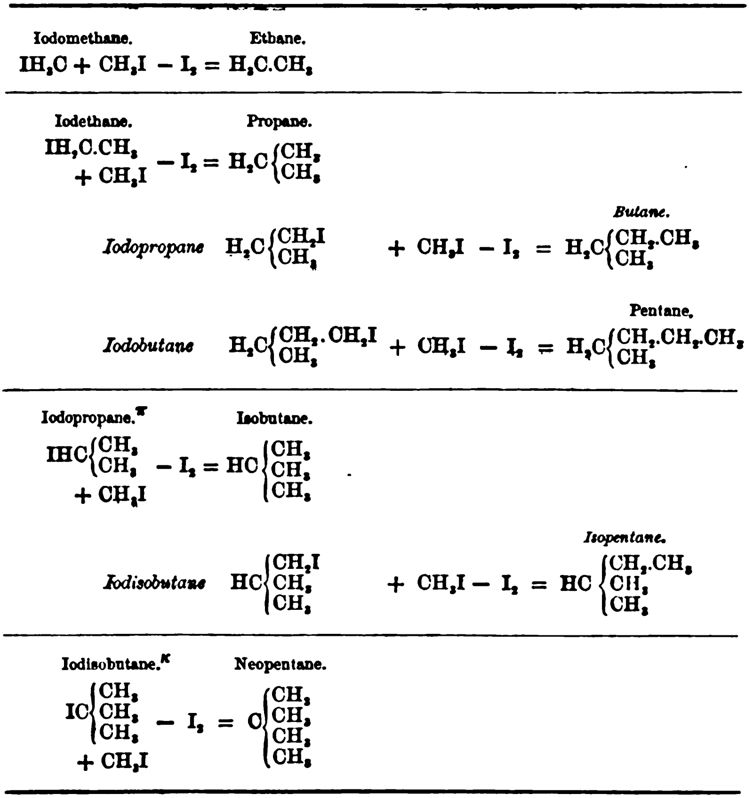
$$\left.\begin{array}{ll} 1^\circ & H_2C.CH_2.CH_2I,CH_3 \\ 2^\circ & IHC.CH_2.CH_3,CH_3 \end{array}\right\} \text{ from } \overset{\text{Butane.}}{H_2C.CH_2.CH_3,CH_3}.$$

$$\left.\begin{array}{ll} 3^\circ & HC.CH_2I,CH_3,CH_3 \\ 4^\circ & IC.CH_3,CH_3,CH_3 \end{array}\right\} \text{ from } \overset{\text{Isobutane.}}{HC.CH_3,CH_3,CH_3}.$$

Accordingly, from the reaction between the first of these and iodomethane CH_3I, there should result normal pentane $H_2C.CH_2.CH_2.CH_3,CH_3$; from the reaction between either the second or third iodobutane and iodomethane should result one and the same isopentane $HC.CH_2.CH_3,CH_3,CH_3$; while from the reaction between the fourth iodobutane and iodomethane should result neopentane $C.CH_3,CH_3,$

CH_3,CH_3; all these, or equivalent reactions having been actually realized with the above results. And isopentane being thus constituted in part of the group HC, in part of the group H_2C, and in part of the group H_3C or CH_3, is accordingly found to yield a tertiary alcohol inoxidizable save with disruption, a pseudo-alcohol oxidizable into a ketone, and a primary alcohol oxidizable into an aldehyd and acid successively. The scheme of the synthetic formation of ethane, propane, the two butanes, and three pentanes, is shown on the concluding table; wherein the combinations with one another of the different groupings taking part in the successive reactions are indicated by the lateral contiguity of their respective symbols.

TABLE III.



Iodomethane. Ethane.

$$IH_2C + CH_2I - I_2 = H_3C.CH_3$$

Iodethane. Propane.

$$\begin{matrix}IH_2C.CH_3\\+\ CH_3I\end{matrix} - I_2 = H_2C\left\{\begin{matrix}CH_3\\CH_3\end{matrix}\right.$$

Butane.

$$\textit{Iodopropane}\quad H_2C\left\{\begin{matrix}CH_2I\\CH_3\end{matrix}\right. + CH_3I - I_2 = H_2C\left\{\begin{matrix}CH_2.CH_3\\CH_3\end{matrix}\right.$$

Pentane.

$$\textit{Iodobutane}\quad H_2C\left\{\begin{matrix}CH_2.CH_2I\\CH_3\end{matrix}\right. + CH_3I - I_2 = H_2C\left\{\begin{matrix}CH_2.CH_2.CH_3\\CH_3\end{matrix}\right.$$

Iodopropane.$^{\pi}$ Isobutane.

$$\begin{matrix}IHC\left\{\begin{matrix}CH_3\\CH_3\end{matrix}\right.\\+\ CH_3I\end{matrix} - I_2 = HC\left\{\begin{matrix}CH_3\\CH_3\\CH_3\end{matrix}\right.$$

Isopentane.

$$\textit{Iodisobutane}\quad HC\left\{\begin{matrix}CH_2I\\CH_3\\CH_3\end{matrix}\right. + CH_3I - I_2 = HC\left\{\begin{matrix}CH_2.CH_3\\CH_3\\CH_3\end{matrix}\right.$$

Iodisobutane.$^{\kappa}$ Neopentane.

$$\begin{matrix}IC\left\{\begin{matrix}CH_3\\CH_3\\CH_3\end{matrix}\right.\\+\ CH_3I\end{matrix} - I_2 = C\left\{\begin{matrix}CH_3\\CH_3\\CH_3\\CH_3\end{matrix}\right.$$

[W. O.]

GENERAL MONTHLY MEETING,

Monday, March 6, 1876.

WILLIAM POLE, Esq. F.R.S. Vice-President, in the Chair.

Sir Thomas Neville Abdy, Bart.
Andrew Clark, M.D.
Miss Annie Frances Cole,
Theodore Duka, M.D.
Mrs. Frankland,
Sir John Hawkshaw, F.R.S. F.G.S.
William Huggins, Esq. D.C.L. F.R.S.
Edward Ind, Esq.
Francis Henry Laking, M.D.
Henry Maudsley, M.D.
Charles Packe, Esq. F.L.S.
Miss Sarah Catherine Prothero,
Evan Morgan Protheroe, Esq.
George Thomas Robinson, Esq.
Miss Sarah Frances Spedding,
Henry Yates Thompson, Esq.
Samuel King Wilson, Esq.
Arthur Bethune Woodd, Esq.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same, viz.:

FROM

American Academy of Arts and Sciences—Complete Works of Count Rumford, Vol. IV. 8vo. 1875.
Arnold, Thos. J. Esq. (the Editor)—The Conspiracy and Protection of Property Act, 1875; and the Employers and Workmen Act, 1875; with the Rules, &c. 8vo. 1876.
Bishop, P. J. Esq. (the Author)—The Channel Railway: Tube or Tunnel. (K 101) 8vo. 1876.
Bombay Branch of Royal Asiatic Society—Journal, Vol. XI. 8vo. 1875.
Cernuschi, H. Esq. (the Author)—Mécanique de l'Echange. 8vo. Paris, 1865.
Monnaie Bi-métallique. 8vo. Paris, 1876.
Bimetallic Money. 8vo. 1876.
L'Extrême Orient. [M. Cernuschi's Collection of Japanese Art.] 8vo. 1874.
Clinical Society—Transactions, Vols. VII. and VIII. 8vo. 1874–5.
Comitato Geologico d'Italia—Bollettini, 1875, Nos. 9, 10. 8vo.
Dublin Society, Royal—Journal, No. 44. 8vo. 1875.
Duret, M. Théodore (the Author)—Voyage en Asie. 12mo. Paris, 1874.

Editors—American Journal of Science for Feb., 1876. 8vo.
Athenæum for Feb., 1876. 4to.
Chemical News for Feb., 1876. 4to.
Electrical News for Feb., 1876.
Engineer for Feb., 1876. fol.
Journal for Applied Science for Feb., 1876. fol.
Nature for Feb., 1876. 4to.
Pharmaceutical Journal for Feb., 1876. 8vo.
Telegraph Journal for Feb., 1876. 8vo.
Franklin Institute—Journal, Nos. 601, 602. 8vo. 1875–6.
Geographical Society, Royal—Proceedings, Vol. XX. No. 2. 8vo. 1876.
Geological Society—Quarterly Journal, No. 125. 8vo. 1871–3.
Hamilton, Edward, Esq. M.D. F.L.S. M.R.I. (the Author)—Catalogue Raisonné of the Engraved Works of Sir Joshua Reynolds, 1755–1820. 8vo. 1874.
Hayden, F. V. Esq. U. S. Geologist in Charge—Report of the United States Geological Survey of the Territories, Vol. II. Cretaceous Vertebrata: by E. D. Cope. 4to. 1875.
Jablonowski'sche Gesellschaft, Leipzig—Preisschrift, XVIII. 4to. 1875.
Kempe, H. R. Esq. M.R.I. (the Author)—Handbook of Electrical Testing. 16to. 1876.
Linnean Society—Journal: Zoology, Nos. 60–62. 8vo. 1876.
Macmillan, Messrs.—J. J. Monteiro; Angola and the River Congo, 2 vols. 12mo. 1875.
Mailly, M. E. (the Author)—Frédéric Argelander. (O 16) 16to. 1875.
Meteorological Society—Quarterly Journal, New Series, No. 17. 8vo. 1876.
Perry, The Rev. S. J. F.R.S. (the Author)—Notes of a Voyage to Kerguelen Island to observe the Transit of Venus. (L 16) 8vo. 1876.
Photographic Society—Journal, No. 264. 8vo. 1876.
Preussische Akademie der Wissenschaften—Monatsberichte: Nov., 1875. 8vo.
Royal Society of London—Proceedings, No. 166. 8vo. 1875–6.
Sandys, R. H. Esq. M.A. (the Author)—In the Beginning. Part 2. 8vo. 1876.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, Feb., 1876. 8vo.

WEEKLY EVENING MEETING,

Friday, March 10, 1876.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

Professor William Henry Flower, F.R.S.

The Extinct Animals of North America.

Few branches of knowledge have received greater accessions during the last few years than that of the past history of the living creatures which have peopled the earth.

I propose this evening to call your attention to some of the results that have been achieved, mostly within the last four or five years, by a small but energetic band of explorers upon a limited part of the earth's surface; results the greatness of which already is only equalled by the promise they give of future still more important extensions of knowledge.

It is mainly through the agency of the admirably conducted geological and geographical surveys of the Western Territories, made by the United States Government, under the direction of Dr. F. V. Hayden, that the subjects to which I shall refer have been brought to light; surveys which are giving to the world, in an excellent series of publications, rich funds of information upon the physical geography, mineralogy, geology, palæontology, zoology, and botany of that hitherto little known but most remarkable region of the earth embracing and bordering the great range of the Rocky Mountains. For the special knowledge which we in England possess of the vertebrate fossils which have been discovered by these surveys, we are greatly indebted to the excellent descriptions of Professor Joseph Leidy of Philadelphia, who in two large and beautifully illustrated volumes * has given the results of his investigations upon them. More recently two other naturalists, Professor E. D. Cope of Philadelphia, and Professor O. C. Marsh of Yale College, have taken the subject in hand, both as explorers and describers.†

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* "Extinct Mammalian Fauna of Dakota and Nebraska, with a Synopsis of the Mammalian Remains of North America," 'Journ. Acad. Nat. Science,' Philadelphia, 1869; and "Contributions to the Extinct Vertebrate Fauna of the Western Territories," 'Report of the U. S. Geological Survey of the Territories,' Washington, 1873.

† I am glad to take the opportunity of thanking Professors Hayden, Leidy, Marsh, and Cope, for their kindness in sending me copies of all their numerous memoirs bearing upon the subject of this discourse.

It must be premised that the material has come to hand so rapidly during the last three or four years that most of the information which has hitherto been given to the world, especially by the two last-named palæontologists, is in a very provisional and fragmentary state, and that until the flood of new discovery begins to ebb, and the few labourers in this plentiful harvest-field have leisure to prepare careful, elaborate, and, above all, well-illustrated descriptions of the specimens, we shall remain in much uncertainty about the real nature and relations of many of the animals of that strange old fauna, which at present are to us little more than names.

I must presume that you are all familiar with the main epochs of time into which geologists have divided the earth's history. For the present purpose we shall only have to refer to the latest of these, the Tertiary, representing how many millions of years we cannot say, and which, for convenience, is generally subdivided into four sub-epochs, the Eocene, the Miocene, the Pliocene, and the Pleistocene, the end of which brings us to the period in which we now dwell. Of course it is not implied by this division that there was any sudden break or interruption of the steady course of the world's progress between these periods. They are merely artificial and arbitrary, though convenient stages, and pass insensibly one into the other; but without the use of some such terms we could not fix the epoch of any particular event or set of events. In geology we know nothing of centuries. We have no kings' reigns, as in political history, to mark the course of time, so we speak of "Miocene" much in the same vague kind of sense in which we speak of the "Middle Ages" in our chronology of the historical events in Europe.

The first evidence of mammalian remains in strata of Miocene age in Western America was that made known in 1846 by Dr. Hiram A. Prout, of teeth then supposed to belong to a gigantic species of *Palæotherium*,* and subsequently described by Leidy under the name of *Titanotherium*. This was the commencement of that interesting series of discoveries, which have now made the "Mauvaises Terres," or "Bad Lands" of the White River of Dacota, classical ground to the palæontologist. But it was not until 1869 that the older beds on the western side of the Rocky Mountains were explored, and the more ancient Eocene land fauna of North America brought to light. In that year commenced the explorations in the vicinity of Fort Bridger, a military post situated in the south-west corner of Wyoming Territory, which have yielded such an abundant harvest, and the locality of which is thus graphically described by Professor Leidy.†

"Fort Bridger occupies a situation in the midst of a wide plain, at the base of the Uintah Mountains, and at an altitude of nearly seven thousand feet above the ocean level. The neighbouring country, extending from the Uintah and Wahsatch Mountains on the south and west to the Wind River Range on the north-east, at the close of the Cretaceous epoch, appears to have been occupied by a vast fresh-water lake. Abundance of evidence is found to prove that the region was then inhabited by animals as numerous and varied as those of any other fauna, recent or extinct, in other parts of the world. Then, too, a rich tropical vegetation covered the country, in strange contrast to its present almost lifeless and desert condition.

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* 'Am. Journ. of Science and Arts,' 1847, p. 248.

† 'Extinct Vertebrate Fauna of the Bridger Tertiary Formation of Wyoming Territory,' 1873.

"The country appears to have undergone slow and gradual elevation; and the great Uintah lake, as we may designate it, was emptied, apparently in successive portions, and after long intervals, until finally it was drained to the bottom. The ancient lake-deposits now form the basis of the country, and appear as extensive plains, which have been subjected to a great amount of erosion, resulting in the production of deep valleys and wide basins, traversed by Green River and its tributaries, which have their sources in the mountain boundaries. From the valley of the Green River the flat-topped hills rise in succession, as a series of broad table-lands or terraces, extending to the flanks of the surrounding mountains."

"The fossils which form the subjects of our communication for the most part were derived from the more superficial deposits of the great Uintah basin, which Professor Hayden has distinguished as the Bridger group of beds. These compose the terraces or table-lands in the neighbourhood of Fort Bridger, and consist of nearly horizontal strata of variously coloured indurated clays and sandstones. As the beds wear, through atmospheric agencies, on the naked declivities of the flat-topped hills, the fossils become exposed to view, and tumble down to the base of the hills among the crumbling *débris* of the beds."

The immense length of time that this ancient lake existed may be inferred from the fact that the mud or sand deposited in it has accumulated to more than a mile of vertical thickness.

It is from this and from neighbouring localities systematically explored only during the last four or five years, both by the Government surveys and by expeditions organized for the purpose from Yale College, that most of the remarkable animals attributed to the Eocene epoch have been obtained; although still more recently fossiliferous beds of the same age have been discovered both in Colorado and New Mexico, so rich as to give hopes that we are still only on the threshold of our knowledge of the wonderful fauna of the ancient American continent. Besides the extensive and older known Miocene and Pliocene beds between the Rocky Mountains and the Missouri, others of corresponding age have been discovered west of the Blue Mountains in Eastern Oregon.

I must now pass in successive review some of the principal groups into which animals have been divided by naturalists, and show what is known of their past history on the great North American continent. I am aware that the summary I am about to give will be exceedingly imperfect, partly on account of the limited time allowed in one discourse, and partly on account of the difficulty of extracting a connected account of these discoveries from the exceedingly numerous notices in which they are described—often fragmentary and disconnected, and even contradictory, and scattered through a variety of periodicals and reports. As most of these descriptions are put forth by their authors as "preliminary," to be superseded by more elaborate and detailed work hereafter, so must this notice of them be regarded. It will at least serve the purpose of calling attention to the importance and interest of this comparatively new field of research.

The first group to which I will direct your attention, as it is that of the ancient history of which we have more complete knowledge than of any other, is the large order of *Ungulata*, or hoofed animals; and first among them I will consider those characterized, among many other distinctive peculiarities, by the uneven or *perissodactyle* structure of the foot,* represented in the actual fauna of the world only by the three families of Horses, Tapirs, and Rhinoceroses—animals differing very considerably from each other in general outward appearance, and yet having many important common characteristics.

It is well known that in the Old World, species of this group, very intermediate in characters to those now existing, flourished in the Eocene age. Cuvier's grand researches in the Paris gypsum beds, which laid the foundation of the study of mammalian palæontology, reconstructed the form, now almost as well known as that of the existing tapir, of the *Palæotherium*, and numerous allied species have since been found, not only in France, Switzerland, and Germany, but in the corresponding beds in our own country. But in America, before 1869, not a single Eocene Perissodactyle had been discovered. In fact, as just mentioned, no Eocene beds containing the remains of terrestrial animals had been explored. Since that date, however, it has been ascertained that the region in which the stupendous mountain ranges of western North America have since been elevated were tenanted by animals of similar form and characters, and quite as varied in species and as numerous in individuals as those which at a corresponding period of time ranged through the marshes and forests of the Paris and London basins.

None of these appear to be identical specifically with the European forms, and even the generic indications, being often founded on very limited portions of the organization only, as a few teeth, must be regarded as provisional. Many were undoubtedly quite distinct from any which we know from the Eastern world. It would be useless here to give a catalogue of the generic and specific names which have been given to the animals of this group already discovered. A brief mention of the most important and interesting will suffice. The two best known genera are those named by Leidy *Hyrachyus* and *Palæosyops*;

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* The parts of the foot arranged symmetrically on each side of a line drawn through the centre of the middle or third toe.

the former is allied to the Lophiodons and Tapirs,[*] the latter to the Palæotheriums. They both embrace animals in size varying from that of a small rhinoceros, down to a peccary. The numerous modifications and combinations of characters found in forms apparently allied to them, which are little known to us at present except by the names given to them by their discoverers, will doubtless afford for a long time to come materials for the minutest scrutiny. Some appear to be allied to the European *Lophiodon* and *Hyracotherium*, one of which, *Orohippus*, Marsh, seems to connect these forms through *Miohippus* and *Mesohippus* with the horse-like *Anchitherium*, and thus fill a link wanting in European formations in the pedigree of the Equine family. This animal, like so many other of the Eocene Perissodactyles, resembles the modern tapirs in retaining the fifth digit on the fore foot, though, as in all known members of the group, the first was wanting, and both first and fifth were wanting to the hind foot. Several species are described, but none larger than a common fox. One form only, *Diceratherium*, Marsh, is rhinocerotic. It is found in the uppermost Eocene strata of Utah, and gives the earliest indication of this group yet known. It seems, according to Marsh, to be connected with the lower Eocene *Hyrachyus* on the one hand, and the Miocene *Hyracodon* on the other.

In the Miocene period in North America the Perissodactyles attained a great development of form, variety, and size, the groups became more distinctly separated from each other, and some of them possessed remarkably specialized characters. True Tapirs have not yet been met with in this period, which is rather remarkable, taken in conjunction with the present geographical distribution of that group. The Palæotheroid and Lophiodont forms had nearly, if not quite, died out, but the more horse-like *Mesohippus*, *Miohippus*, and *Anchitherium* were abundant, and appear to continue the line from the Eocene *Orohippus* to the true horses of the Pliocene period.

Rhinocerotic forms now became ascendent, being represented by *Diceratherium*, Marsh, differing from all existing animals of the group, by having a pair of horns placed side by side on the nasal bones;[†] and a very interesting genus, *Hyracodon*, Leidy, an animal with molar teeth and many other of the characters of rhinoceros, but having no nasal horn, and having a complete set of incisor and canine teeth, as in all the older Perissodactyles, which are lost in the more modern Rhinoceros. It is therefore quite a connecting link between the Palæotheroid animals of the Eocene, and the true rhinoceros of the Pliocene, and occupies exactly the right geological horizon that such a form ought to do if the one has been genetically derived from the other.

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[*] The teeth resemble those of *Lophiodon* and *Hyracotherium*, but the last lower molar has a bilobate crown, as in Tapirs. The skeleton is very like that of the Tapirs. See Cope, "On the Osteology of the Extinct Tapiroid *Hyrachyus*," 'Proc. Amer. Philos. Soc.,' April 18, 1873.

[†] A small European Miocene Rhinoceros (*R. minutus*, Cuv., *R. pleuroceros*, Duvernoy) has a pair of prominences placed laterally on the nasal bones, which may have supported horns.

Hyracodon therefore has a high place of interest among many of similar nature which have been revealed by our newly acquired knowledge of the ancient American fauna. If, however, as is stated, the fifth digit of the fore foot is only rudimentary, it could scarcely have been, as remarked by Marsh, on the direct line of descent from the four-toed Eocene to the equally four-toed Miocene rhinoceros, though certainly in such a case we know not what ought to be allowed for reversion.

The same period (generally speaking) also produced several species of a more perfect rhinoceros, still hornless however, and resembling the contemporaneous European *Aceratherium*.

But the most remarkable of the Miocene Perissodactyles, and in some respects the most remarkable of all the animals which the recent explorations have brought to light, are a number of species of gigantic size, to the first known of which Leidy gave the name of *Titanotherium*, and of which other forms have been named by Marsh *Brontotherium*, and by Cope *Symborodon*.*

They must, by their great size and strength, grotesque appearance, and general mode of life, have taken the place in the Miocene times of the then extinct *Uintatherium* of the Eocene, and were in their turn replaced by the Mastodons and Elephants of later ages. The rhinoceros of the present day will serve to give the best general idea of the appearance of these creatures, but some of them (for they seem to have been numerous both in species and individuals) approached nearer to the elephant in size and length of limb. The numbers of their teeth are thus expressed: incisors, $\frac{2}{2}$; canines, $\frac{1}{1}$; premolars, $\frac{4}{3}$; molars, $\frac{3}{3}$; total, 38. The incisors were very small, and sometimes deciduous in the lower jaw; the canines of moderate size, the premolars and molars more like those of *Palæotherium* than rhinoceros. The skull in its general characters was quite rhinocerotic, but the nasal bones supported a pair of large, laterally divergent, rugged prominences, apparently for the attachment of horns. The limbs were intermediate in proportions between those of the elephant and rhinoceros, but, as in the latter, the femur has a third trochanter, and a deep pit for the round ligament. The feet were short and stout, but in essential characters agree with the true Perissodactyles, and have four toes in front and three behind.

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* The first indication of the existence of these animals is that mentioned above of the supposed *Palæotherium* teeth from the Mauvaises Terres of Dakota, described by Dr. Prout in the 'American Journal of Science and Arts' for 1847, p. 248, and afterwards by Leidy, as *Titanotherium Prouti* ('Ancient Fauna of Nebraska,' 1853). More complete remains, including the whole upper series of molars, were described by the same zoologist in 'The Extinct Mammalian Fauna of Dakota and Nebraska,' 1869. Cope's genus, *Symborodon* (1873), is differentiated from *Titanotherium* by the absence of lower incisors, a character which Marsh says is "evidently due either to age or imperfect specimens." Marsh, in his description of *Brontotherium*, says, "The only other genus of the group known with certainty is *Titanotherium* of Leidy (*Menodus*, Pomel), which, according to the descriptions of that author, differed essentially, in having four lower premolars, and in the absence of a third trochanter on the femur. Less important differences are seen in the composition of the teeth and in the diastema between the upper canine and first premolar." The last-mentioned character is certainly not one upon which it is necessary to found a generic distinction (and a generic distinction, unless necessary, is in my opinion something always to be avoided), and as to those considered essential, it seems very doubtful if they really exist, as the four inferior premolars of Leidy's specimen were not present but only "indicated" in an imperfect fragment ('Extinct Mammalia of Dakota and Nebraska,' p. 212), and there is no evidence that the portion of femur described without a third trochanter belonged to the same animal. It is surely safest to assume identity in such cases until the contrary can be proved, although the opposite rule seems to prevail with the eminent and industrious American palæontologists to whose labours we are indebted for all our knowledge of the subjects of this communication. Pomel's name, *Menodus* ('Bib. Univ. de Genève,' t. x., 1849), has the priority of *Titanotherium*, but it has never been adopted, being too closely identical with the still earlier *Menodon*, v. Meyer, to be admissible.

Numerous species have been described, both by Cope and Marsh, founded chiefly upon the form and direction of the horn cores on the nasal bones: they are all from the Miocene beds east of the Rocky Mountains, in Dakota, Nebraska, Wyoming, and Colorado; and there is no evidence of any of the *Titanotheridæ*, as the family should be called, after the first-characterized genus of the group, having survived to a later geological epoch.[*]

When we pass to the Pliocene and Pleistocene times, the Perissodactyles met with can all be referred to one or other of the three existing families; all the intermediate forms, and all those which have attained a different type of specialization, as those last named, have completely disappeared.

Remains of several species of *Rhinocerotidæ* were very abundant during the Pliocene period in western North America; they all appear to belong to the hornless type, and from causes unknown became entirely extinct before the Pleistocene age. No rhinoceros now exists on the American continent, nor is there evidence that it ever supported animals belonging to the minor groups of the family to which the existing Indian, Sumatran or African rhinoceros pertain.

During this period an immense development took place in the various forms of three-toed horses, *Anchippus*, *Protohippus*, *Parahippus*, *Hipparion*, &c., which replaced the *Anchitherium* of the Miocene. These in their turn, through many well-marked gradational forms (a full knowledge of which is among the many interesting results of these explorations), gave way to the true horses, of which remains of several species have been found in Pleistocene deposits, scattered throughout almost every region of the continent from Escholtz Bay in the north to Patagonia in the south. These also became entirely extinct before

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[*] See O. C. Marsh, "On the Structure and Affinities of the *Brontotheridæ*," 'American Journal of Science and Arts,' vol. vii., January 1874; E. D. Cope, in "Annual Report of the United States Geological and Geographical Survey of the Territories, embracing Colorado, for the year 1873," p. 480 *et seq.* *Megacerops coloradensis* (Leidy, 'Pr. Ac. Nat. Sc.,' Phil., Jan. 1870) was founded on detached nasal bones of a member of this family.

Fig. 1.

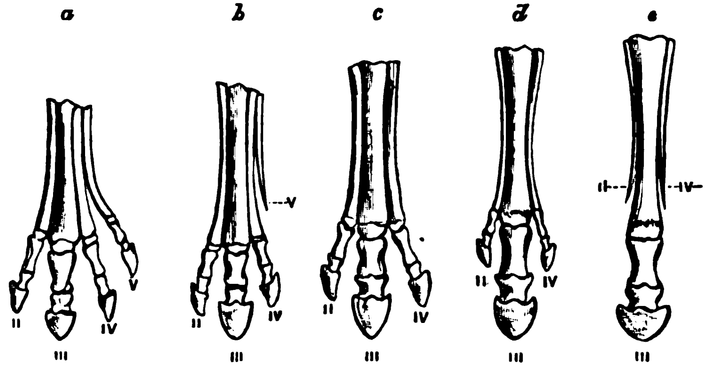

Diagram of several stages of modification of the feet of extinct forms of American horse-like animals (chiefly from Marsh), showing gradual reduction of the outer, and enlargement of the middle toe (III).

a, *Orohippus* (Eocene); *b*, *Mesohippus* (Miocene); *c*, *Miohippus* (Miocene); *d*, *Hipparion* or *Protohippus* (Pliocene); *e*, *Equus* (Pleistocene).

the discovery of America by the Spaniards—a most remarkable circumstance, considering the fitness of the country for their maintenance, as proved by the facility with which the descendants of horses introduced by the European invaders have multiplied in a feral state.*

On the other hand, of tapirs, fossil remains have been found most sparingly, though sufficient to show that they had a much wider geographical range northwards in the Pleistocene period than now, and yet several species of this genus still linger in the highlands of Central and South America, the sole direct representatives of the vast and varied Perissodactyle fauna of the continent in ages long gone by.

We may now pass to the remaining great group of hoofed animals, the even-toed or Artiodactyles,† represented at present by the Pigs,

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* For an outline of the history of the Horse family in America, see O. C. Marsh, "Notice of New Equine Mammals from the Tertiary Formation," 'Am. Journ. of Science and Arts,' vol. vii., March 1874; and "Fossil Horses in America," 'American Naturalist,' vol. viii., May 1874. Also, E. D. Cope, "Osteology of *Protohippus*," in "Report on the Stratigraphy and Pliocene Vertebrate Palæontology of Northern Colorado," 'Bull. U. S. Geol. Surv. of the Territories,' No. 1, 1874; and Leidy's memoirs quoted above.

† The parts of the feet are arranged symmetrically on each side of a line drawn between the third and fourth toes.

Hippopotami, Camels, Chevrotains, Deer, Antelopes, Sheep, and Oxen.

The remains of this group in the hitherto explored American Eocenes are very scanty and unsatisfactory as affording indications of its early history and development. Not a single specimen has yet been described which was found in a sufficiently perfect state of preservation to give a tolerably correct idea of its structure and affinities, and no forms corresponding to the well-established European *Anoplotherium*, *Dichodon*, *Xiphodon*, or *Cænotherium* have been found. Towards the close of the period only (if the age of the Tertiary beds of Utah are rightly determined) do we find indications of well-defined crescentic-toothed or Selenodont species (*Agriochœrus*, Leidy), and also of tubercular-toothed or Bunodont (*Elotherium* and *Platygonus*) forms. During the Miocene period, however, Artiodactyles of both these two main divisions abounded. It will be as well to take each group separately, and follow its history throughout, from the Miocene to the present day.

1. The Bunodonts, or those which inclined most to the pigs in dental structure. These were in North America, as in Europe, chiefly represented by animals of the genus *Elotherium*, huge swine-like creatures, some of which approached the hippopotamus in size, and also by an allied four-toed form, *Pelonax*, Cope, remarkable for its horn-like bony tubercles projecting out on each side from near the front end of the lower jaw. These became extinct, as in the Old World, before the close of the Miocene epoch.

Animals more like true pigs also existed, but all of the peccary type, the only one which now survives on the American continent. If the evidence of teeth alone can be trusted, this form, like the tapir (and the African *Hyomoschus*), is an unmodified remnant of the old Miocene fauna. But both at that period and in the Pleistocene, peccary-like animals existed in greater variety (as in the genus *Platygonus*), and in wider geographical distribution than at present. It is interesting to note that no remains of true *Sus* or any of its Old World modifications, as the wart hog (*Phacochœrus*), and babirussa, or of any species of hippopotamus, have hitherto been met with on the American continent. And thus the American bunodont Artiodactyles, instead of undergoing great and diverse modifications, as did the corresponding animals of the Old World, have been gradually dwindling and contracting to the two closely allied species of peccary (*Dicotyles tajacu* and *D. labiatus*), among the smallest and most insignificant of the whole group.

2. Of the Selenodont or crescentic-toothed Artiodactyles, the former existence in America of the long-extinct Old World genus *Hyopotamus* has been recognized by the discovery of a few teeth in the lowest Miocene of Dakota, and this is remarkable as the only recorded instance of an American form with three cusps on one of the lobes of the upper molars, a very common character among the European Miocene Artiodactyles.

Remains have also been found recently of various small ruminant-like animals, some not larger than a squirrel in size, to which the names *Leptomeryx*, *Hypisodus*, *Hypertragulus*, &c., have been applied. Whether these belonged to the family of Chevrotains or *Tragulidæ* (improperly called pigmy musk-deer), or whether, as appears more probable, they were not rather generalized or ancestral forms of the true Pecora or Ruminants, is difficult to determine from the present evidence.

Perhaps the most interesting of the American Miocene Artiodactyles, on account of their great abundance both in species and individuals, the full information which has been collected as to their structure, and their distinctness from any known forms from any other part of the world, is a family to which Prof. Leidy applied the term *Oreodontidæ*. They played the part in the North American Miocene fauna which the deer do now in the same country, the antelopes in Africa, and the sheep in Central Asia. They were in nearly all points of structure intermediate between the ruminants and pigs, and (with many other Old World forms, however) completely break down the line of demarcation which our knowledge when limited only to the existing fauna of the world caused zoologists to draw between those two groups.

They appear to have survived throughout the whole of the Miocene period, commencing in the genus called *Agriochœrus* in the uppermost Eocene, and ending in the *Merychyus* of the early Pliocene; and it is of great interest to know that a gradual modification can be traced in the characters of the animals of the group, corresponding with their chronological position, from the earlier more generalized to the latest comparatively specialized forms, thus affording one of the most complete pieces of evidence that is known in favour of a progressive alteration of form, not only specific, but even of generic importance, through advancing ages.

Another group of great interest made its appearance in the Miocene of North America, and which, if the evidence of fragments can be trusted, did not become extinct, like the last, but continuing to exist through the Pliocene and Pleistocene ages, is still represented on two distant parts of the earth by the three or four species of llama of South America, and the two species of camel of the Old World. The discovery of the early Miocene *Pöebrotherium* and of the Pliocene *Procamelus* and *Pliauchenia*, remains of which, and of Pleistocene *Aucheniæ* of great size, though not generically distinguishable from the modern llamas, are abundantly distributed over the North American continent, seem to show that that country was the original home of the singular family of *Camelidæ*, which was probably introduced by emigration in its perfect condition into the Old World, where none of the transitional forms from the more generalized ruminants, like those above mentioned, have been met with.*

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* See Cope "On the Phylogeny of the Camels," 'Proc. Acad. Nat. Sciences, Philadelphia,' Part II., 1875, p. 261.

On the other hand, of the gigantic four-horned *Sivatherium* of the Himalayas, the equally large but hornless *Helladotherium* of the Grecian Miocene, or of the giraffes, no representatives have hitherto been found in America. And very little light has been shed upon the origin and distribution of the true ruminants by these researches, except in a negative manner. No deer have been found in the Miocene (at which epoch they were abundant in Europe); in the Pliocene but a single and poorly developed species; while in the Pleistocene, with the exception of one large species, called *Cervus Americanus*, they scarcely differ from those of the actual fauna. Of the hollow-horned ruminants, several species of bison, of *Ovibos* or musk ox, and a single *Ovis*, have been described, all from the Pleistocene, but not a single species of antelope. From these facts it may safely be inferred that the few existing and Pleistocene hollow-horned ruminants are immigrants of recent date from other lands; and it is even possible that the deer have been similarly derived, though at a somewhat older period, which will account for their being more varied and wider spread in surface distribution, extending down almost to the southern extremity of the continent, while the hollow-horned ruminants are entirely confined to the north.

Scarcely any group to which the term "Order" is applied is so limited in the number of its existing species as that called, from one of the most striking external characteristics of the animals composing it, "*Proboscidea*." The two species of elephant, that of Asia and Africa, the largest and in some respects the strangest of all land animals, are its sole representatives. Between these two animals and all others now existing, there is a wide gap in numerous essential structural characters, so that really, in the world as we now see it, they have no near relatives.

But this was not always so. Leaving the existing condition of the earth's surface, and passing back to the last well-marked stage before our own, the Pleistocene period, we find abundant remains of elephants, imbedded in alluvial gravels, or secreted in the recesses of caves, into which they have been washed by streams or floods, or where, in many cases, they have been dragged in as food by hyænas or other predaceous inhabitants of these subterranean dens.

We find these remains of elephantine animals extensively distributed over regions of the earth where no such creatures have existed within the memory of man. We find, moreover, that the elephants of the Pleistocene period, judging from their bones, and especially their teeth, do not in most cases exactly correspond in form or size to either of the existing kinds. We certainly find remains undistinguishable from those of the existing African elephant, in the north of Africa and southern parts of Europe; but the majority of these remains not only differ among themselves, but differ more or less from either the African or Indian species, and hence have had many different appellations bestowed upon them, as belonging to what are considered to be different species.

But not only in the Pleistocene period did elephants abound. Animals which must come within any definition which will include both *Elephas Indicus* and *Elephas Africanus* are also found in the European Pliocenes; and even earlier in Asia, the deposits of the Sivalik Hills, belonging to a transition between the Pliocene and Miocene age, are rich with the remains of elephants of varied form, in some cases presenting a considerable departure from our better-known types. Further back in time, however, we search in vain for true elephants. In the Miocene period, it is true, many kinds of huge Proboscideans roamed over the surface of the earth, but these differ so much from what we now call "elephants," that it becomes necessary to distinguish them by another name; and that of *Mastodon*, first applied by Cuvier, is generally adopted.

Mastodons however were, after all, very like elephants, only being distinguished by some peculiarities of the teeth; and by means of intermediate species the two forms pass so gradually into one another, that it is difficult to say, in the case of some species, with which group they ought most properly to be classed. One other form of animal, which can be referred to the order *Proboscidea*, is known in the Old World—the *Dinotherium*, a huge beast, the nature of which for a long time was very doubtful, having been grouped by some naturalists with the Manatees, by others even with the Marsupials. Its remains have been found, though comparatively rarely, in Miocene deposits in Germany, France, Greece, Asia Minor, and India.

Here our knowledge of the history of the order *Proboscidea*, as derived from palæontological researches in the Old World, ends. The *Dinotherium* being in its teeth and some other respects slightly less specialized than the other forms, constitutes some kind of an approximation to the Ungulate animals, especially the tapirs; but the gap to be bridged over is still very wide; and no remains referable to animals of the order, or any intermediate forms between this and other orders, have been found in Old World Eocene deposits.

Let us now turn to America. Neither at the present time nor within the memory of man have any Proboscidean animals existed within the length or breadth of the whole continent. But at one time, and that, geologically speaking, a very recent one, both true elephants and true mastodons abounded in North America, and the last-named genus extended far into the southern portion of the continent. The elephant, the remains of which are most abundant throughout what are now the United States, differed but very slightly, if at all, from that which at the same period ranged throughout the northern portion of the Old World from the British Isles to Alaska. The commonest species of mastodon (*M. Americanus*, or *Ohioticus*, or *giganteus*) seems to have survived to a much later period than any of its European congeners, and even to have been the last extinct of all the American Proboscideans. Remains of other elephants and mastodons, though not differing in any remarkable degree from well-known European forms, have been found in Pleistocene (and at all events with respect to the last-named genus) in Pliocene deposits; but, as far as the evidence is at present before us, nowhere earlier.

So far, then, we find that elephants and mastodons, of types quite resembling those found in the Old World, but in less specific variety, appeared on the American continent at a later period than in the Old World (none having been found of undoubted Miocene age), and ultimately became completely extinct before the historic period. No animal corresponding to the *Dinotherium* has been found. We shall hardly, then, be prepared to look for primitive types of the race in earlier American formations.

Among the most remarkable discoveries of the Eocene formations of Wyoming, has been that of a group of animals of huge size, approaching, if not equalling, that of the largest existing elephants, and presenting a combination of characters quite unlike those known among either recent or extinct creatures, and of which there were evidently several species living contemporaneously.* Bones of some of these animals were discovered by Professor Marsh and Lieutenant Wann of the Yale College exploring party near Sage Creek, Western Wyoming, in September 1870, and described by the former in the following year,† though provisionally referred to the genus *Titanotherium*. Other remains were discovered and described by Leidy in 1872‡ under the generic name of *Uintatherium* (from the Uintah Mountains, near which they were found). Very shortly afterwards other portions of bones and teeth of either the same or closely allied forms were described by Marsh as *Dinoceras*, and by Cope as *Loxolophodon* and *Eobasileus*. Whether these names will ultimately be retained for separate generic modifications, or whether they will have to be merged into the first, it would be premature to attempt to decide upon the evidence before us. Until satisfactory grounds have been shown for considering them to be distinct, it will be best to speak of them all under the name which has the priority.

To form some idea of the general appearance of one of these animals, we must imagine a creature very elephant-like in its general proportions, elevated on massive pillar-like limbs, not quite so long certainly as those of elephants, but with the femur vertically placed, without third trochanter, and without pit for the round ligament as in those animals, the radius and ulna complete, and crossing, and the feet short, broad, massive, and with five toes on each. At first sight the skeletons of these feet (as figured by Marsh) show an extraordinary resemblance to those of the elephant, and to no other animal, especially in the form of the astragalus; but on closer inspection it is seen that in the mode of articulation between the different bones of the carpus and tarsus, they really come nearer to the rhinoceros and other Perissodactyles. For example, the upper end of the third metacarpal, instead of joining almost alone to the magnum, as in elephants, is united by two nearly equal facets to the magnum and unciform, and the astragalus articulates largely with the cuboid, which it does not in the elephants. The presence, however, of five complete and distinct toes to the fore foot, and probably also to the hind foot, is a definite distinguishing character from any known Perissodactyle.

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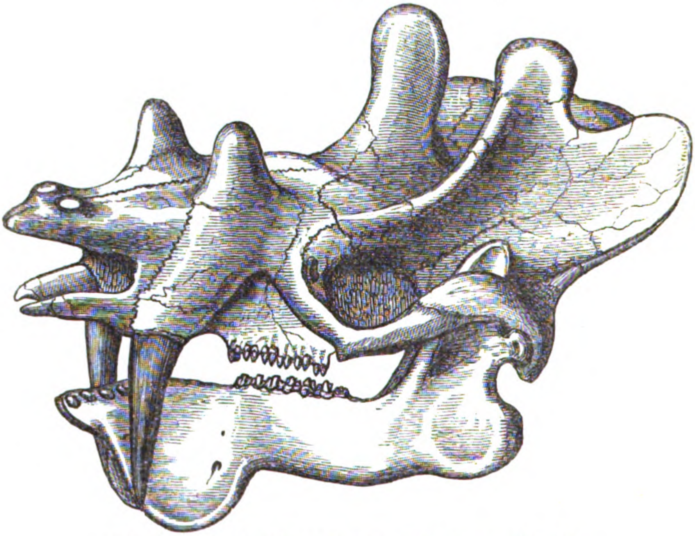
* See Leidy, 'Extinct Vertebrate Fauna of the Western Territories,' 1873: O. C. Marsh, "Principal Characters of the Dinocerata," 'American Journal of Science and Arts,' vol. xi. February 1876: E. D. Cope, 'Sixth Annual Report of the United States Geological Survey of the Territories,' 1873, p. 563: *Idem*, 'Systematic Catalogue of the Vertebrata of the Eocene of New Mexico, collected in 1874'; Washington, April 17, 1873.

† 'Am. Journ. Science and Art,' July 1871, p. 351.

‡ 'Proc. Acad. Nat. Sciences,' Philadelphia, July 30, 1872. In the same communication the great canine tooth, which was found apart from the skull, was described as that of a carnivore, under the name of *Uintamastix atrox*.

The vertebræ, in their main characters, resemble those of the Proboscideans; though the neck was somewhat longer in proportion. The tail was long and slender.

FIG. 2.



Restored skull of Uintatherium (*Dinoceras*, Marsh).

The head was long and narrow, and in its essential features more resembling that of the rhinoceros than the elephant. It was elevated behind into a great occipital crest, as in the former, but, unlike that of any other known animal, it had developed from its upper surface three pairs of conspicuous laterally diverging protuberances, one pair (the largest) from the parietal region, one on the maxillaries in front of the orbit, and one, much smaller than the others, near the fore part of the elongated nasal bones. Whether these were merely covered by bosses of callous skin, as the rounded form and ruggedness of their extremities would seem to indicate, or whether they formed the basis of attachment for horns of still greater extent, either like those of the rhinoceros or the buffalo, must still be a matter of conjecture. Whichever may have been the case, they would have given a very strange aspect to the creature which possessed them, and have been formidable weapons in encounters either with animals of its own kind, or with the carnivorous beasts whose remains have been found associated with it.

The teeth were no less remarkable than the general formation of the skull. The dental formula was $i\,\frac{0}{3},\ c\,\frac{1}{1},\ p\,\frac{3}{3},\ m\,\frac{3}{3} = 34$. The front teeth, or incisors were, as in modern ruminants, absent above, and in the lower jaw rather small, directed forwards, and forming a continuous series with the still smaller canine. A large, trenchant, enamel-covered tusk, not unlike that of the musk-deer, or Chinese water-deer (*Hydopotes*), descended from each side of the upper jaw, and lay against a singular flattened expansion of the lower border of the ramus of the lower jaw, which has been conjectured to be for the purpose of protecting them from injury, though no such processes are found necessary in the animals above mentioned with similar tusks; and they recall a similar conformation of the jaw of the *Megatherium*, which can have no such function. They must have effectually prevented any stabbing or penetrating action of these weapons. There is some evidence that the tusks were smaller in the females. The molar teeth were six on each side, above and below, placed in continuous series, and separated from the canines by a considerable interval. They were small for the size of the animal, and of simple structure, each having two more or less transverse crests, though those of the upper jaw diverge externally, and meet at the inner border of the tooth in a V-shaped manner.

The brain (as indicated by the size and form of the cerebral cavity, of which a cast has been made and figured by Professor Marsh) was proportionately smaller than in any other known mammal, recent or fossil, and was almost reptilian in its character. It was actually so diminutive (in Marsh's *Dinoceras mirabile*) that the entire brain could apparently have been drawn through the neural canal of all the presacral vertebræ, certainly through the cervicals and lumbars. It was therefore exceedingly unlike that of modern Proboscideans.

These animals, taking the totality of their organization into consideration, appear to belong to the great Ungulated group, and to hold a position somewhat intermediate to the Perissodactyles and the Proboscidea, though nearer to the former than was at first supposed. This affinity is still further shown by the discovery of other forms, constituting the genera *Bathmodon* and *Metalophodon* of Cope, from an earlier geological horizon, which with the general structure of the *Uintatheridæ*, retain, in an extremely interesting manner, many primitive characters, common to all early Ungulates, especially the complete number of incisor and premolar teeth. These are forms for fuller information upon which we anxiously wait.*

It should be mentioned that Professor Marsh has made of *Uintatherium* and its immediate allies, a peculiar order of mammals, to which he has given the name of *Dinocerata*, while Cope, who formerly included them in the Proboscidea, and placed *Bathmodon* with the Perissodactyla, has now ('Syst. Cat. of Vertebrata of the Eocene of New Mexico,' 1875) formed an order called *Amblypoda*, of which the *Dinocerata*, containing the genera *Uintatherium* and *Loxolophodon*, is one suborder, and the *Pantodonta*, containing *Bathmodon* and *Metalophodon*, the other. Both, however, admit that they hold a position somewhat intermediate between the modern orders of Proboscidea and Perissodactyle Ungulates, and so stand out, as it were, as broken piers of the bridge, by which the gulf which now so completely divides these orders might have been passed over.

The negative evidence (which of course must be received with the greatest caution in palæontology) of the absence of the remains of any of these animals in the Miocene or Pliocene deposits of North America, indicates that the race became extinct, at least in that land, though it possibly may have migrated elsewhere, and perhaps in Asia may have laid the foundation of that family, which first appears in the Old World under the more familiar form of the typical Proboscideans.

While, however, it would be the rashest possible assertion to say that these were derived directly from the Eocene Bathmodons and Uintatheriums, it is not too much to look upon the latter as affording us some indications of the steps by which the process might have taken place, and as such their discovery is one of the most interesting that has been revealed by modern palæontological research.

The history of the North American *Carnivora* may next engage our attention. In the actual condition of affairs, this order is tolerably well represented on that continent. The *Procyonidæ*, or raccoon-like animals, are almost peculiar to it; the bears, and their allies the otters, martens and skunks, are numerous. The dogs also are widely distributed and variously modified. The Felidæ, though tolerably abundant, do not attain the same size and strength as in the Old World, and the *Hyænidæ*, *Protelidæ*, *Cryptoproctidæ*, and the great family of *Viverridæ*, the civets and genettes, are entirely wanting.

As the modern tapirs and peccaries which pursue their peaceful existence in the deep shades of the tropical American forests, frequently become the victims of the ferocious jaguars and pumas, which prowl in search of prey through the rank vegetation of the river banks, or lie in wait concealed amid the luxuriant foliage of the branches overhead, it is only natural to suppose that the countless herds of tapir and swine-like herbivorous animals which lived in a similar manner amid the ancient Eocene swamps and forests of Wyoming and Colorado, were also destined to furnish subsistence to a tribe of rapacious beasts of form and fashion long passed away. Palæontological research amply shows that this was the case. Side by side with the remains of *Hyrachyus*, *Palæosyops*, and the rest, are found bones and teeth of animals of varied size and structure, but of undoubted carnivorous habits. Unfortunately at present most of these are known only by fragments, and not a few of the numerous genera recently described are founded on the evidence of a single tooth!

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* A figure of the skull of *Bathmodon elephantopus*, and much additional information upon the geology and palæontology of New Mexico, has been published by Professor Cope, in Lieutenant Wheeler's 'Annual Report upon the Geographical Explorations and Surveys West of the One Hundredth Meridian,' &c., Washington, 1875, which reached the writer since the above was in type.

There are some, however, about which our knowledge has, within the last two or three years, been greatly extended, and which have proved to be of very special interest.*

Among these are two genera, called by their describer, Professor Cope, *Synoplotherium* and *Mesonyx*, each represented by a single species, *S. lanius* and *M. obtusidens;* the latter the size of a large wolf, the former somewhat larger, both from the Eocene of Wyoming. These, like so many of the animals of the same period of the world's history, present such a combination of characters, that it is impossible to place them in either of the existing families of the order to which they belong, being in some respects bear-like, in others dog-like, and in some being more generalized than are any existing members of the order. For instance, their claws had not that narrow, compressed, curved, and sharp-pointed form seen more or less in all modern carnivores, and in the highest degree in the most typical or specialized members of the group, the cats; but they were nearly flat, straight, and blunt (from whence it has been conjectured that they were adapted for an aquatic life), and two bones of the carpus, the scaphoid and lunar, which in all existing carnivora (even including the seals), are united to form a single bone, were distinct from each other, as in the majority of mammalia.† The lower canine teeth were placed very close to the fore part of the jaw, which appears to Professor Cope "a special modification for peculiar habits, which," he says, "I suspect to have been the devouring of the turtles, which so abounded on the land and in the waters of the same period. The slender symphysis could most readily be introduced into the shell, while the lateral pressure of the upper canines with the lower would be well adapted for breaking the bony covering of those reptiles."

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* Cope, "On the Flat-clawed Carnivora of the Eocene of Wyoming," 'Proc. Am. Phil. Soc.,' vol. xiii. No. 90, 1873: *Idem*, 'Systematic Catalogue of the Vertebrata of the Eocene of New Mexico,' Washington, 1875.

† "The scaphoid and lunar bones have not yet been found united in any Eocene mammal."—Marsh, 'Am. Journ. of Science and Arts,' March 1876.

In the character of the molar teeth, of which there were a considerable number resembling one another in form, these animals, and many others less perfectly known, resemble the well-known *Hyænodon* of Europe, a lost type of carnivorous animal first found in the Upper Eocenes of Europe, but abundant also in America at an apparently later age. The members of this group of carnivores are all characterized by long and somewhat slender jaws, containing a series of teeth one behind the other, each being in its form a repetition of the one before it, as in many of the existing predaceous marsupial animals. The greater differentiation of the characters of the teeth and the shortening of the jaws, with corresponding increase of the force with which they can be closed, seen in the highest forms of modern carnivores, is one among many examples of progressive adaptations conducing to more complete efficiency in performing the functions of life. These Eocene carnivores also (according to Cope) showed a primitive character in the tibio-astragalar articulation, or "ankle-joint." "The astragalus is flat, and the applied surfaces are nearly a plane, and without the pulley-shaped character seen in existing carnivora, as dogs, cats, and, in a less degree in the bears and in other mammalia with specialized extremities, as *Perissodactyla*, *Artiodactyla*, &c. The simplicity of structure resembles, on the other hand, that found in the opossum and various *Insectivora*, *Rodentia*, and *Quadrumana*, and in the *Proboscidia*, most of which have the generalized type of feet. The structure indicates that the carnivorous genera named were plantigrade—a conclusion which is in conformity with the belief already expressed, that the mammalia of the Eocene exhibit much less marked ordinal distinction than do those of the Miocene or the recent periods. It is, indeed, questionable whether some of the genera here included in the carnivora are not gigantic *Insectivora*, since the tibio-tarsal articulation in many, the separation of the scaphoid and lunar bones in *Synoplotherium*, the form of the molars, and the absence of incisor teeth in some, are all characteristic of the latter rather than the former order." *

The Miocene carnivorous animals found associated with the herbivorous Oreodons of Dakota, are more perfectly known, many of them having been well worked out and figured some years ago by Leidy. The most remarkable are several species of *Hyænodon*, a genus already mentioned as found in the Upper Eocenes and Lower Miocenes of France, and also of the south of England; but one of the American species (*H. horridus*, Leidy) is larger than any of its European congeners, its skull (which, as Leidy remarks, is not like that of any existing carnivores, but something intermediate between that of a wolf and an opossum) fully equalling that of the largest individual of the black bear (*Ursus Americanus*); other species were not larger than a fox. These were the last survivors of a group notably different from any now existing.

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* This idea has been more fully developed in a paper by Professor Cope, "On the supposed Carnivora of the Eocene of the Rocky Mountains" ('Proc. Acad. Nat. Science,' Philadelphia, Nov. 30, 1875); and the group *Creodonta* as a sub-order of *Insectivora*, proposed for the reception of several genera previously referred to the *Carnivora*.

The remaining American carnivores of the Miocene and more recent ages, can be, as far as they are known, referred to one or other of the groups into which the order is now divided. The dog-like forms were abundant throughout the Miocene and Pliocene ages. But in the earliest period more generalized types were met with, assigned to the well-known European genus *Amphicyon*, which differs from the true dogs in the more tuberculated character of its molars, and the presence of the last upper tooth of this class, which is missing in the modern *Canidæ*, and also in the more bear-like structure of its limbs. Various modifications of *Felidæ* were also abundant, the most remarkable in the Miocene period belonging to that group (*Machærodus* or *Depranodon*), with immensely developed sabre-like upper canine teeth, which flourished throughout such an extensive period of time and in so many parts of the world: in the sub-Himalayan region; in Miocene and Pliocene epochs in various parts of Europe, and almost down to recent times in England, as we know by the teeth found in Kent's Hole; in South America, where remains of its largest and most powerful form (*M. neogæus*) have been found in the caves of Brazil and in the alluvial plains of Buenos Ayres; and again in the Miocene of the North American territories. Why this form, so highly specialized for its mode of life, once apparently the dominating type of the whole order throughout the world, should have entirely disappeared, and given way to the more modestly armed modern tigers and leopards, is not very easy to explain. We may, however, be allowed to conjecture that it may have been a case of over-specialization, in which the development of the carnivorous type of dentition, gradually accumulating in intensity, being, up to a certain degree at least, advantageous to its possessors, became at last by successive inheritance so exaggerated that its growth outran its usefulness of purpose, and the enormous teeth thus acquired became ultimately less manageable and less efficient than those of more moderate dimensions, and hence the animals possessing them were in the contest for existence gradually driven out and superseded by those which at present people the earth. Such appears to be constantly the fate of forms which become over-specialized, or in which the development of one part has run on in one particular direction out of due proportion to the rest of the organization. We know that it is quite possible, by artificial selection, to produce animals with one particular part developed even detrimentally to the entire economy of the creature, and it really seems as if something of the same kind not unfrequently occurs in nature.

From the time of the extinction of the sabre-toothed cats in North America, to the present period, other forms more like those now existing continued to prevail, none, however, equalling in size those of the Old World lion or tiger; but of the other families of the carnivora hitherto little has been found. *Ursidæ* and *Mustelidæ*, except in Pleistocene deposits, are very rare; and, what is more remarkable, remains that can with certainty be referred to the *Procyonidæ*, a group whose head-quarters are in America, have not been met with. The families which were previously mentioned as not now existent in that continent are equally unknown in its extinct fauna.

Perhaps the most conspicuous, both on account of their colossal size and their singular conformation and habits, of the animals inhabiting the American continent in the period immediately preceding the one in which we now live, were the great ground sloths, known to us familiarly by the names of *Megatherium*, *Mylodon*, *Megalonyx*, &c. As these animals are peculiarly American, it might have been expected that when the earlier formations of the continent on which they flourished were explored, the remains of similar or at least allied forms would have been brought to light. But hitherto this has not been the case.

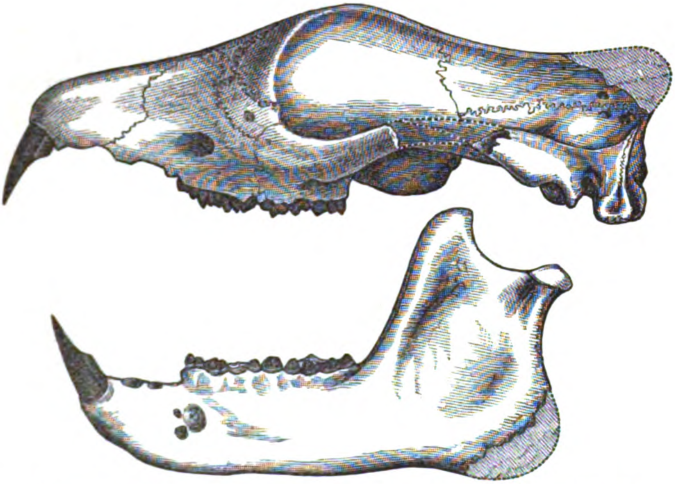
Two species of a genus (*Morotherium*, Marsh) allied to *Megalonyx* and *Mylodon*, from Pliocene strata in Central California and Idaho, have been described; but it is a most remarkable fact that not a fragment attributed with certainty to an Edentate animal has been found in any Miocene or Eocene deposit on the North American continent, and therefore (if this negative evidence can be trusted) we shall have to look elsewhere (probably to the Southern American continent), for the region which gave birth to these mighty creatures, and to look upon them as but temporary excursionists into the Northern portion of the continent during the Pleistocene epoch.

On the other hand, numerous species of the orders *Rodentia*, *Insectivora*, and even *Chiroptera*, and some attributed to the *Marsupialia*, have been found in almost all the hitherto explored fossiliferous deposits down to the Eocenes. Of these, time will not suffice to give an account, and this is less important as it is difficult to draw any general conclusions from the fragmentary descriptions of them which we possess at present. I must, however, not omit to call attention to two recently announced discoveries, which, when fully worked out, promise results of considerable interest.

Professor Leidy, in 1868, described a single lower molar tooth from a Tertiary formation, supposed to be Miocene, of Shark River, Monmouth County, New Jersey, apparently of Ungulate affinities, and to which he gave the name of *Anchippodus riparius*. Subsequently a lower jaw of very anomalous character, from the Bridger Eocene, with large rodent-like perpetually growing incisors, no canines, and bilobed molars, something like those of Palæotherium, was described by the same author under the name of *Trogosus castoridens;* but comparison with the single molar from New Jersey showed so close a resemblance, that the latter name was withdrawn, and both specimens referred to the first described, or *Anchippodus*.

Other similar forms found in a more perfect condition have been described by Professor Marsh, who at a meeting of the Connecticut Academy, Feb. 17, 1875, suggested that as they could be included in no known order of mammals, they should be placed in a new one, for which he proposed the name *Tillodontia.**

FIG. 3.



Skull of Anchippodus (*Tillotherium fodiens*, Marsh), ¼, from Marsh, 'Am. Journ. Sci. and Art,' 1876, Plate VIII.

"These animals," Professor Marsh observes, "are among the most remarkable yet discovered in American strata, and seem to combine characters of several distinct groups, viz. Carnivores, Ungulates, and Rodents. In *Tillotherium*, Marsh, the type of the order, the skull has the same general form as in the bears; but in its structure resembles that of the Ungulates. The molar teeth are of the Ungulate type, the canines are small, and in each jaw there is a pair of large scalpriform incisors faced with enamel, and growing from persistent pulps, as in the Rodents. The adult dentition is as follows: Incisors, $\frac{2}{2}$; canines, $\frac{1}{1}$; premolars, $\frac{3}{2}$; molars, $\frac{3}{3}$. The articulation of the lower jaw with the skull corresponds to that in Ungulates. The posterior nares open behind the last upper molars. The brain was small, and somewhat convoluted. The skeleton most resembles that of carnivores, especially the *Ursidæ;* but the scaphoid and lunar bones are not united, and there is a third trochanter on the femur. The radius and ulna, and the tibia and fibula are distinct. The feet are plantigrade, and each had five digits, all terminated with long, compressed, and pointed ungual phalanges, somewhat similar to those of the bears. The other genera of this order are less known, but all apparently had the same general characters. There are two distinct families, *Tillotheridæ* (perhaps identical with *Anchippodentidæ*), in which the large incisors grew from persistent pulps, while the molars have roots; and the *Stylinodontidæ*, in which all the teeth are rootless. Some of the animals of this group were as large as a tapir. With *Hyrax* or the *Toxodontia* they appear to have no near affinities."

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* 'Am. Journal of Science and Arts,' vol. ix. March 1875; *Ibid.* March 1876, with figures. Professor Cope has since suggested that the *Tillodontia* should form a sub-order of *Insectivora*.

The second recently announced discovery to which I alluded is, that a considerable number of fragments of teeth, jaws, and bones from the American Eocenes, the nature of which for some time was an exceedingly difficult problem, really belong to a low form of the great and important order *Primates*, an order embracing the lemurs, various species of monkeys, and culminating in man himself, and which hitherto has not been known with any certainty (except at least by some equally recent discoveries in France) to have existed in the Eocene period. The evidence, however, on which this announcement, made almost simultaneously by Professors Marsh* and Cope,† rests, is not very fully before the world. Already more than fifteen genera have been named and described, which are assigned to this group, and their characters are said to be those of a low or generalized form of lemur; while some are compared with the true monkeys. Far more rigid comparisons and carefully balanced deductions are required before we can assign their various species to their correct position, and appreciate their bearings upon the genetic history of the Primates. In some of the descriptions at present before us lemur and monkey are used as convertible terms, and yet those who have studied these groups most closely are far from being able to pronounce upon the true relationship even of the existing species, and some even doubt whether they ought properly to be associated in the same order. But this is far too large a subject to discuss in all its bearings at the close of a discourse. I can only indicate it as one which may have much light thrown upon it by the researches of American palæontologists.

I can say nothing now of what is being done by the same persons, in the same regions of the world, with regard to other classes of animals than the one I have hitherto been speaking of. But the great and important discoveries of new forms and new links between old forms have not been confined to the mammalia alone. The knowledge of the past history of birds, reptiles, and of fishes has likewise been greatly enlarged. The very remarkable discovery of Odontornithes, or birds with true teeth and other reptilian characters, has been made. Numbers of new invertebrates, and a whole world of new fossil plants, have been brought to light.

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* 'Am. Journ. Sci. and Arts,' vol. v. p. 405, Nov. 1872.

† 'Proc. Amer. Philos. Soc.' 1872, p. 554. See also Cope, "On the Primitive Types of the Orders of the Mammalia Educabilia," 'Am. Philos. Soc.' April 18, 1873; and Marsh, 'Am. Journ. Sci. and Arts,' vol. ix. March 1875.

Apart from the special interest of the individual results, some few only of which I have been able to bring before your notice this evening, the contemplation of what has been done in American palæontology in the last few years teaches us,—First, that the living world around us at the present moment bears but an exceedingly small proportion to the whole series of animal and vegetable forms which have existed in past ages. Secondly, that, notwithstanding all that has been said, and most justly said, of the necessary imperfection of the geological record, we may hope that there is still so much preserved that the study of the course of events which have led up to the present condition of life on the globe, may have a great future before it.

[W. H. F.]

WEEKLY EVENING MEETING,

Friday, March 17, 1876.

Sir Frederick Pollock, Bart. M.A. in the Chair.

Sir Henry Sumner Maine, K.C.S.I.

The State of Feudal Property in England and France on the Eve of the first French Revolution.[*]

The speaker began with remarks on the neglect of the provincial "cahiers," or memorials, by French historical writers on the first revolution, adding that the examination of some of them by MM. Chassin, Douiol, and Taine, and the publication of some by M. Prudhomme, had already led to considerable results, especially by throwing more light on the causes of the hostility of the cultivating peasantry to the territorial nobility in all France, except in the western provinces, such as Brittany and Anjou. The complicity of the peasants with the reign of terror was shown to be connected with a wish to preserve great advantages obtained at the very period when France became a republic; and their object in setting fire to a château was to destroy the title-deeds of the seigneur of the fief.

The French nobles and gentry were everywhere engaged in unceasing litigation with the peasantry; and they had little or no analogy to a landed aristocracy. Few had great estates, and the largest part of them had little or no land let for rent to tenants at will, or lessees, but lived on the money produce of the small incidental services due from owners of land held, as we should say, on copyhold to the lord of the manor. They received fines, and had monopolies and various nondescript sources of income; and concerning the legal foundation of these privileges a strong controversy was proceeding during the half-century preceding the Revolution. The first or constituent assembly swept away the greatest part of the feudal dues, with compensation for part; the second, or legislative assembly, abolished the residue and withdrew the compensation; the third, the convention, was zealously supported by the peasantry, through a desire to retain what had been gained.

The speaker, before considering the question, whether the Revolution took place because a great part of the soil of France was held on copyhold tenure, demonstrated the fallacy of the popular notion that at the Norman conquest the land of England was divided among a number of feudal lords, who parcelled it among their followers; to some for homage and military service, the rest being cultivated by their serfs or villeins, a slavish class, from whose holdings our copyhold tenure has descended.

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[*] This discourse will be eventually published in full by the author.

After reference to the Teutonic irruption and its results, and to the dissolution of the Carlovingian empire, when the feudal world was at last constituted, it was shown that all feudal society was a reproduction of a single typical form. This unit consisted of a group of men settled on a definite space of land, forming in England a manor, in France a fief, having at its head respectively a lord with his court baron, or a seigneur with his seignorial court. Under these, first, were the free tenants, doing military service and giving their opinion on judicial and other matters when required; but the greatest part of the land was in the hands of the villeins, who owed to the lord all sorts of taxes, dues, and personal labour, but were never precisely slaves, but always in a sense landed proprietors. Their condition was a close counterpart to that of the Russian serfs, who were permitted by the present emperor, some years ago, to commute their personal services for money, and became owners of the largest part of the village lands. The king of France was, as it were, lord of an exalted manor, and his free tenants were the dukes of Normandy, Burgundy, and similar dignitaries. This system was reproduced in England, with a difference. The more powerful Anglo-Norman kings allowed no one to be absolutely interposed between themselves and their subjects; they exacted fealty and military service from all Englishmen.

Why, then, did the transformation of the fief in one country end in a revolution, in the other end in a somewhat inconvenient form of landed property? In regard to this question the speaker compared the state of feudal tenures in France just before the Revolution, described by Arthur Young, with the evidence laid before the Copyhold Committee of the House of Commons in 1851, commenting on their surprisingly close analogy. He showed, on the evidence of Roger North, that the grievances of copyholders were considerable in the seventeenth century; but he expressed his opinion that if all copyholds were nowadays enfranchised it would be principally because they are an obstacle to agricultural improvement. He then showed that one powerful cause of the different issue in the two countries was the distinction in their judicial organization. In both a considerable part of the popular law was administered by local courts, controlled by higher tribunals. Those in France were the "parliaments," having much independence of spirit, while the justice administered in English courts was, from very early times, more emphatically than in other countries, the "king's justice." The English bench was nominated by the king, and might be filled with his creatures, but the seats in a French parliament were either purchased or inherited. The parliaments were never the sovereign's servile instruments or pliant nominees. The principal difference appears to have been this, that the French superior courts always inclined towards extending, the English superior courts to restricting, the area of the land held on tenures directly traceable to ancient villeinage. By the middle of the seventeenth century, as Roger North says, "most manors in England were more than half lost." Just the contrary occurred in France. Again, in England the landowners became richer through the acquisition of waste lands as pasture, and the production of wool; while in France the noblesse, impoverished through a court life, seem never to have been able to buy up the holdings of their former villeins. It is a most vulgar error to suppose that small properties in France date from the Revolution; since Arthur Young immediately before it expresses himself as amazed at their multitude, which was still increasing. As an illustration of the exasperating system to which the holders were subject, reference was made to Caleb Balderstone's raids, described in 'The Bride of Lammermoor.' In England, on the other hand, the bulk of the class corresponding to the French peasantry consisted of agricultural labourers or tenant farmers, never politically dangerous. The English copyholders and French peasants, it was said, were not hirers, but owners of land; and in the sixteenth and seventeenth centuries English copyholders frequently impaired their legal condition by accepting leases. Another cause of difference was the transfer of land by purchase and sale through the unusual facilities more frequent in England than elsewhere. The sacredness of contract was one of the fundamental ideas of the French philosophical creed, and it strongly influenced the proceedings by which the manorial rights of the French nobles were taken away. At first it was intended that they should receive compensation for the loss of such of their rights as originated in agreement; but it was as much as they could do to save their lives. Though there is no reason for supposing that manorial rights originated in violence, yet there is equally little for supposing that they originated in agreement. In England, on the contrary, the titles of the lord of the manor and of the copyholder were more deeply rooted in agreement than in any other deeply feudalized country. The lord had often purchased his rights, the copyholder had constantly obtained his land, subject to manorial rights, by purchase from somebody else.

In concluding, the speaker said that he did not wish it to be understood that the contrast between the sum of feudal obligations and rights taken in England and France is wholly to be explained by the causes analyzed in his discourse, but this set of causes appeared to him to have been kept too much in the background.

WEEKLY EVENING MEETING,

Friday, March 24, 1876.

WILLIAM SPOTTISWOODE, Esq. LL.D. M.A. Treas. R.S.
Secretary and Vice-President, in the Chair.

T. McK. HUGHES, Esq. M.A.
WOODWARDIAN PROFESSOR OF GEOLOGY, CAMBRIDGE.

Geological Measures of Time.

THE speaker, in considering the methods which have been employed in attempting to estimate the absolute age of geological phenomena, grouped them under two principal heads,—I. Astronomical, and II. Geological.

I. Those in which an appeal was made to astronomical causes, such as,—A. Limitations (α) of the period during which, assuming many things unchanged, the sun can have continued to give its heat and light to the earth; (β) of the time it would take the earth to cool down to its present state.

With regard to the first (α), he remarked that, as astronomers allowed from three to five hundred millions of years, he had no reason to say that geology required more: at any rate, it did not seem to be called for by anything that would come under consideration that evening.

With regard to the second (β), he pointed out that it belonged to what might be called the pre-historic period of geology; for we cannot say that we have any record of a molten earth. Lakes of molten matter at no great depth, or zones of heated matter ready to become molten when some of the pressure is removed, will do for us.

Why and how heated also we have no evidence to show—chemical reactions, or the irregular cooling from original high temperature of a mass of unequal conductivity will equally well explain all we see—and the difference in this respect between a loose volcanic ash and the same ash consolidated and metamorphosed into a hard clinking felstone may well teach us caution in receiving speculations founded upon the rate of passage of heat through large and necessarily unexplored masses of the earth's crust.

Yet he felt that it is to the physicist we must look for the next great advance in geology. Only the physicist must leave us an earth that within the whole period of which we have any record in the rocks has been under conditions which, though locally changing, have been on the whole similar to those which now obtain—whose crust has been always capable of an outside crumpling sufficient to cause at least twenty miles continuous vertical displacement in one area.

B. The other method of measuring geological time by an appeal to astronomy, is by correlating the periods of known astronomical combinations with episodes in our earth's history. This seems a fair and promising field of inquiry, but it has been pushed too far beyond the region of facts into that of hypothesis to be of much value at present. For these speculations practically depend (*a*) upon the establishment as a fact that astronomical combinations, such as the coincidence of the extreme of excentricity of the earth's orbit with the extreme of obliquity of the earth's axis, would produce excessive cold or heat in the northern or southern hemisphere as the case might require, or would produce such extremes in the seasons as would result in glacial conditions owing to the summer's sun being unable to undo the winter's frost, or would produce much effect worth mentioning if unaided by favourable geographical arrangements of land and water, or would produce any effect at all that might not be neglected in such inquiries owing to the certainty that it might be entirely counteracted by such small geographical changes as we know are continually going on.

But astronomers and physicists tell us, with regard to the effect of such combinations, that the difference of mean temperature under any circumstances that we have to do with would be trifling compared with the difference of climate produced to-day by geographical causes, such as give us, in the southern hemisphere, the land of the humming-bird in South Georgia on the same latitude as the ice-bitten hills of Tierra del Fuego, and, in the northern hemisphere, the coasts of Norway and North Britain on the same latitude as Greenland and Labrador.

It is, however, allowed that such combinations might produce an appreciable effect in intensifying or mitigating extremes of temperature in the winter and summer.

So that now they leave us in this position, that if we can show any periodicity in the recurrence of certain geographical arrangements of land and water, then if the astronomical combinations which tended to produce the same effect coincided with this we should have a greater result. That is obvious. But there is no known law as to the proportion of land to water in any successive periods; and if, for instance, the astronomical causes which tended most to intensify winter's cold coincided with a period when we had half land within the Arctic circle, how could that period be distinguished simply by effects produced from a previous period, during which perhaps three-fourths of the regions within the Arctic circle was land, and the astronomical causes tended to produce quite an opposite result?

β. But there is a still more serious objection to this method of measuring time—one founded more exclusively upon geology. It of course assumes that we have fixed the exact position in the earth's history of the episode which we are trying to make fit in with astronomical cycles.

To give some examples of the kind of evidence upon which this depends. We know of, at any rate, two periods—the Carboniferous and Miocene—represented in the rocks of the Arctic regions, during which, if we may assume that the habits and requirements of plants and animals were the same as those of their nearest living allies appear to be, there must have been a warmer climate there than now prevails. Previous to late experience we should have said the same on finding the bones of the mammoth and woolly rhinoceros in far northern regions. It might have appeared difficult to explain the occurrence of evergreen plants where there is four months' night, did we not know that the Alpine roses, a small kind of rhododendron, were buried for at least four months a year under snow so deep that they must be in total darkness, and yet they come out green. Were the fauna and flora of Australia of to-day found only fossil, we should bracket the beds in which they occurred as closely with the Jurassic as with our own deposits of recent age. We must not push that kind of evidence, depending upon analogy of form, too far, or infer that southern forms could not, with very slight modification of structure, be adapted for a more northern clime, especially if it also is tempered by such geographical changes as we have a right to assume possible and probable from an examination of the earth's surface at the present day.

On the other hand, it is easy to prove that over a given area in southern latitudes a severe Arctic climate once prevailed. The grinding ice leaves much clearer evidence of its former presence than does the genial warmth of the summer's sun.

We are therefore quite justified in taking as proved that in England, and indeed over the whole of Northern Europe,—in fact, over the whole of the northern hemisphere,—there was in late geological times a climate like that of Greenland.

But even if astronomers and physicists allowed us to speculate on astronomical combinations, which would account for such changes of temperature at the same time over the whole of one hemisphere, we have no right whatever to assume that all the various phenomena of extreme glaciation which we observe here and there and everywhere over the northern hemisphere, were produced at the same time, or anything near the same time. As soon as there was land lifted above the waters anywhere, rain and rivers began to cut out valleys and form alluvium. In like manner, as soon as anywhere there was land raised above the snow line (no matter what may have determined the snow line for that particular area), snow fields and soon glacier ice must have been formed.

Of course, those examples which we can examine are generally the more recent, or, in the case of ice scratches, those that have been covered up; yet there is no more reason for speaking of a Glacial Period than there is for speaking of an Alluvial Period, the agency of glaciers or of rivers having come into operation wherever and whenever suitable conditions for either appeared.

Palæontology by itself does not furnish any exact measure of time, but we must refer to it for the interpretation of some other phenomena that are appealed to in such speculations.

Local divisions of the post-pliocene, and especially the glacial deposits, were founded chiefly upon the differences in the life of the period known to depend upon climate; but no classification of the whole has been made out, such as would enable us to say whether the fauna of a cold period here was or was not synchronous with that of a warm period elsewhere.

Not only through the later periods, among which that known as the glacial period occurs, but also through all the periods we have recorded in the rocks, we have no evidence of an entire extinction of old forms at any one time, but there is always an overlap, many forms, if not identical with those in the older rocks, at any rate analogous to them, appearing in the newer. Now this is hardly possible if the extreme view that the ice from north and south almost met in equatorial regions be true. The supposition that the northern fauna and flora retreated up to the tops of the hills, or lingered along the margin of the ice sheets, and that the space between the two was still warm enough to keep alive tropical forms, does not commend itself to our judgment. The occurrence of Alpine flora on the high mountains in southern regions can be explained just as easily by reference to local and limited ice sheets as by the hypothesis of a great ice sheet proceeding from the poles.

The great glacial ice, of which we have evidence remaining in the northern hemisphere, has probably been now here, now there, through all the periods from the Crag to our own time, when it is strongly developed in Greenland. And the reason why we are not so likely to find traces of it going back to remote ages is, that denudation must in a long period remove such thin local deposits where they have not been submerged and covered up, and of course what we see is more likely to belong to the period of later emergence than of subsidence.

As for any difficulty arising from the rapidity of movements of elevation and depression of the earth's surface involved in this view, we need only refer to the almost recent shell-beds of Uddevalla and other places in Scandinavia, and in our own country to the evidence afforded by the shells of Macclesfield and Moel Tryfaen for proofs of upheavals up to at least a quarter of a mile vertical since the glacial period of those places—and surely since the deposition of the Crag we may have had time enough for ten times that amount of displacement.

In the early days of science, when recent sea shells were found far inland, and fossil shells imbedded in the rocks of the mountain tops, the whole was often referred to one great deluge. The extreme glacialists are doing much the same now. Not having yet sufficient knowledge to discriminate between the periods when in each locality glacial conditions prevailed, they refer the whole to one great glacial epoch; while the fact that it was not continuous is so clear that they are obliged to invent interglacial periods to explain the phenomena.

II. The more exclusively terrestrial phenomena, which have been appealed to as evidence of the age of deposits, may be divided into,—A. Palæontological, and B. Geographical.

A. The belief in the greater or shorter lapse of time that has taken place between two periods of the world's history, which is founded on the amount of change that has taken place in the life of the period, is only a vague impression, of different weight in different minds, according as they have had brought more constantly before them the facility with which, by artificial selection, varieties may be fixed, or, on the other hand, how constant nature is to the old types when conditions remain the same; and no exact measure has been attempted from such data.

B. There are measures of time founded on observation of geographical changes. Such as those due

a. To accumulation of deposits,
β. To removal of matter.

a. Deposits are either (i) chemical, (ii) organic, or (iii) mechanical. Under (i) we have what is known as calcareous tuff, travertine, stalactite, or stalagmite. In the case of caves and petrifying springs, the acidulated water is always either enlarging or stopping up its channels, and so the deposition of travertine in the same cave and on the same part of the cave must, from the nature of the case, vary continually; and the conditions upon which the variation depends must, as far as we can see at present, remain undetermined and indeterminable.

There are cases in which a period of time may with greater approximation to accuracy be measured by the deposition of travertine, when, for instance, the water from a drainage area of known extent is all carried through one channel. There, as the quantity of water falling over the whole area is probably nearly constant within recent times, and a new opening here makes up for a choked channel there, the average is pretty well kept up.

For example, the grand Roman aqueduct seen at the Pont du Gard near Avignon is lined to a thickness of about 14 inches with travertine. This was probably done in about eight hundred years; but even in this case, as the lining of travertine increased, either less water must have flowed along the aqueduct, or it must have run with greater velocity—both circumstances affecting the rate of deposit. But when we are dealing with antiquity so vast as that to which we are probably carried back by the record of the caves, we have no right to assume even the constancy of climatal conditions or of land level.

(ii) Next considering the rate of accumulation of organic deposits, the speaker confined himself, as in all the other cases, to such as have been employed as an exact measure of time, of which use he gave examples in each case.

He explained the growth of peat, pointing out that there are two kinds of peat, that which is formed in water, as in mountain tarns or old river-courses, and the peat that grows all over the slopes of moorlands high and low. The first is partly formed from drifted vegetable matter in the deeper parts, and from the decay of plants that grow on the spot all round the margin, which therefore encroaches rapidly. Here at the outset we meet with a source of error. The rate is very different in these two cases, the quantity of vegetable matter that drifts far in being generally very small. On the hillsides the growth is to be referred almost entirely to two or three species of moss, and in a smaller degree to the heather and other plants. As the lower part of the mosses sphagnum and hypnum decay away and add to the layer of peat below, the upper part grows on, and so a thick layer of vegetable matter is at length accumulated. Workmen tell us that when they have dug a trench into a peat moss, if they leave it alone it fills up again, or as they would say, the peat grows again. This happens when the peat is apt during some seasons to be full of water, so as to become a kind of slush or ooze. It is perfectly clear that the apparent rate of accumulation where such filling in occurs must often be deceptive. A good example of a similar thing happening on a large scale in nature is the case of the Solway moss, and many other instances as recorded by Lyell.

So we see that while the peat is being formed it is subject to all kinds of variations, and when it has been formed it is liable to be soaked with water and run, destroying the value of all evidence to be derived from any observation on its rate of growth elsewhere.

(iii) It is not often that we can in the study of geology get the advantages of experiment to test theories by, and our observations of nature are often extended over too wide a range of time and space; but in the case of the accumulation of ordinary sediment, as in the case of the peat, we have artificial operations to appeal to, where results produced rapidly help us to understand the larger and slower operations of nature. In the north of England, principally along the Humber and its tributaries, there is a system of improving and restoring the land near the rivers by a process known as "warping," i. e. laying on the silt suspended in the tidal waters over large tracts of low-lying lands, which thus become raised in a few years many feet above their natural level.

He explained the great variations in thickness and character of deposit produced by these artificial means, and also by natural operations in that same district, and pointed out as a result of such observations how unsafe it is to measure time by the accumulation of sediment on the low lands near the mouth of a great river.

Similarly from examples quoted of the effects of thunderstorms and sudden floods observed by himself in various places, he pointed out the great irregularity of the accumulation of torrent débris and river gravels.

β. Another method of estimating the lapse of time is founded upon the supposed rate at which rivers scoop out their channels. Although no very exact estimates have been attempted, still the immense quantity of work that has been done as compared with the slow rate at which a river is now excavating that same part of the valley is often appealed to as a proof of a very great lapse of time.

The fact of such an enormous lapse of time is not questioned, but this part of the evidence is challenged.

The previous considerations of the rate of accumulation of silt on the low lands prepares us to inquire whether there is any waste at all along the alluvial plains. Several examples were given to show that the lowering of valleys was brought about by receding rapids and waterfalls; for instance, following up the Rhine, its terraces could often be traced back to where the waterfall was seen to produce at once almost all the difference of level between the river reaches above and below it. At Schaffhausen the river terrace below the hotel could be traced back and found to be continuous with the river margin above the fall. The wide plains occurring here and there, such as the Mayence basin, were due to the river being arrested by the hard rocks of the gorges below Bingen so long that it had time to wind from side to side through the soft rocks above the gorges. When waterfalls cut back to such basins or to lakes they would recede rapidly, tapping the waters of the lake, eating back the soft beds of the alluvial plains, and probably in both cases leaving terraces as evidence not of upheavals or of convulsions, but of the arrival of a waterfall which had been gradually travelling up the valley. So when the Rhone cuts back from the falls at Belgarde we shall have terraces where now is the shore of Geneva; so also when the falls of Schaffhausen and ages afterwards when the falls of Laufenburg have tapped the lake of Constance, there will be terraces marking its previous levels. And so we may explain the former greater extent of the lake of Zurich, which stood higher and spread wider by Utznach and Wetzikon before it was tapped by the arrival of waterfalls which cut back into it and let its waters run off until they fell to their present level.

A small upheaval near the mouth of a river would have a similar effect. The Thames below London, and the Somme below St. Acheul, can now only just hand on the mud brought down from higher ground; but suppose an elevation of a hundred feet over those parts of England and France (quite imperceptible if extended over 10,000, 1000, or even 100 years), and the rivers would tumble over soft mud and clay and chalk, and soon eat their way back from Sheppey to London, and from St. Valery to Amiens.

So when we want to estimate the age of the gravels on the top of the cliff at the Reculvers, or on the edge of the plateau at St. Acheul, we have to ask not how long would it take the rivers to cut down to their present level from the height of those gravels at the rate at which that part of their channel is being lowered now, but how long would it take the Somme or Thames, which once ran at the level of those gravels, to cut back from where its mouth or next waterfall was then to where it runs over rapids now. We ought to know what movements of upheaval and depression there have been; what long alluvial flats or lakes which may have checked floods, but also arrested the rock-protecting gravel; how much the wash of the estuarine waves have helped. In fact, it is clear that observations made on the action of the rivers at those points now have nothing to do with the calculation of the age of the terraces above, and that the circumstances upon which the rate of recession of the waterfalls and rapids depends are so numerous and changeable that it is at present unsafe to attempt any estimate of the time required to produce the results observed.

But although, when we examine critically the various measures of time that have been employed, it would seem that with our present knowledge there is no certainty to be arrived at with regard to the age of any geological phenomena of considerable antiquity, still we know that we have measured some good base lines, and if as yet we have got no sensible parallax for those far-off worlds, we may hope some day to measure longer base lines, and get a better result.

[T. McK. H.]

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COLL. OF SURGEONS OF ENGLAND

Royal Institution of Great Britain

WEEKLY EVENING MEETING,

Friday, March 31, 1876.

George Busk, Esq. F.R.S. Treasurer and Vice President, in the Chair.

Professor James Dewar, F.R.S.E. Part II.*

The Physiological Action of Light.

On a former occasion I communicated the results of a research on the "Physiological Action of Light" executed conjointly with Professor T. G. McKendrick. The principal facts then established may be thus summarized.

Comparative Physiology of the Action of Light.—The impact of light upon the eyes of members of the following groups of animals, viz. mammalia, birds, reptiles, amphibia, fishes, and crustaceans, produced a variation amounting to from three to ten per cent. of the normal current. At that time we found light caused a negative variation in the case of warm-blooded animals.

Transmission of Action to Brain.—The electrical variation may be traced into the brain. Instead of severing the eye from the brain and cutting the optic nerve, simply remove the head of the frog. Then suppose one of the electrodes in contact with the surface of the brain and the other in contact with the surface of the cornea, an effect is obtained from the action of light similar to that just described.

Action due to Change in Retina.—This action is really due to an alteration in the retina itself. This must be definitely proved, because it was a legitimate criticism that the change produced by the action of light may be due to contraction of the iris; the iris being a muscular structure contracting on the action of light by a well-known reflex mechanism in normal circumstances, and even after removal of the eye from the head. A contraction of the iris might produce a negative variation or diminution of the electrical current, but it is difficult to imagine that it could cause an increase or positive variation. In order, however, to get rid of this difficulty, cut off the front part of the eye altogether, and place one electrode so as to touch the surface of the vitreous humour while the other impinges on the transverse section of the optic nerve, a current is obtained—no doubt weaker, but still a current sufficiently strong for detecting any variation which light may produce. In these circumstances, light still produces the variation I have described. To make it more definite still, pick out the retina with a fine glass point, and leave only the sclerotic and perhaps fragments of the choroid. Even then an electrical current is obtained; but this current is not affected by light. It is, therefore, proved that the variation produced by the action of light is due to some change or other occurring in the retina when light impinges upon it.

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* See R. I. Proceedings, vol. vii. p. 360.

Rays of the Spectrum.—Which rays of the spectrum produce the greatest effect? We know, of course, that the rays which are the most luminous to our consciousness are the yellow rays. The colours of a very pure spectrum were obtained, the eye being brought into the various rays successively, and the result noted. To obtain comparative results, the operations were repeated as quickly as possible. It was found, in studying the results, that those rays which we regard as the most luminous produce the greatest variation. For instance, the low red rays at the end of the spectrum produce very little effect, and if you go below the red into the heat rays there is no action. But the effect increases till you reach the yellow, and if you go on to the violet it gradually becomes less and less until, beyond the violet, there is no action.

Relation of Electrical Variation to Luminous Intensity.—Experiments made have conclusively shown that a quantity of light, one hundred times in excess of another quantity, only modifies the electric variation to the extent of increasing it from three to five times its original amount. The effects observed vary in such a manner as to correspond closely with the relative variations that would result if the well-known psycho-physical law of Fechner was applicable to this class of phenomena.

The effect of Fatigue.—The retina, on the action of light, behaves in a similar manner as regards fatigue, to a muscle that has been exhausted by repeated stimulation. The muscle diminishes in its mechanical effect for the same stimulation and recovers during repose. The amount of electric variation in the case of the eye diminishes for the same amount of light stimulation, unless the organ has had sufficient time to recover its normal condition. In this case, the recovery takes place in the absence of light.

We have continued this investigation in various new directions, and have arrived at results which may be thus shortly detailed.

New Method of Experimenting.—One of the chief difficulties in arriving at the exact relation between the electrical variation and the different luminous and colour intensity of light, was the continually diminishing sensibility to the stimulus, owing to the abnormal conditions of the eye when removed from the head. You can easily understand how this occurs. When you begin the experiment, the eye is remarkably sensitive to light, and a large variation of current is obtained; but the amount of this current is gradually falling in consequence of the gradual change in the parts of the eye, owing to their loss of vitality and sensibility. In fact, the parts are dying—the blood is not circulating, and molecular and chemical changes are slowly occurring. In the case of the frog's eyes, however, it is a fact that the retina retains its sensibility from three to four hours, and sometimes longer. After a lapse of two hours or so, the frog's eye frequently remains in a tolerably stable condition, in which it does not lose rapidly. This condition may last for four or five hours. In order to get rid of the difficulty of gradual death of the parts, we tried various methods. In our earlier experiments, we attempted to get the eye removed as quickly as possible, and to make the observations rapidly. In the case of the warm-blooded animals, this did not lead to very good results, because the sensibility to light disappeared in a very few minutes. We also on several occasions exposed the posterior aspect of the eye in the living anæsthetized warm-blooded animal, and succeeded in bringing one electrode into contact with the severed optic nerve while the other touched the cornea. This method was troublesome and difficult.

We, however, did succeed in obtaining definite results. These experiments are now made in quite a different way. By placing a frog, rabbit, or pigeon under the influence of chinoline, the animal remains motionless. We then remove a small portion of the surface of the cranium, so as to expose a portion of the brain. One of the electrodes is brought into contact with the surface of the cornea, and the other with the surface of the brain. The blood is still circulating. A current is obtained; and all the effects I have just mentioned may be observed with ease. The animal remains in this condition, retaining its sensibility to the action of light, for as long a period, in the case of the frog, as forty-eight hours. These observations led to the discovery made recently, that there is no necessity for even exposing the surface of the brain. That is to say, the action of light can be traced, if needful, through the whole body. If, for example, we take a frog, place it in position, slightly abrade the skin on the surface of the head or back, or any part of the body, then adjust the electrodes, one in front of the cornea and the other upon the abraded skin, we obtain an electrical current which is affected by light in the usual way. But if the electrode in contact with the cornea be shifted to some other part of the body, a current may be obtained; but this current is not sensitive to light. In order to produce the specific action of light upon the eye, the retina must be included in the circuit. This discovery enabled us to perform many experiments without injuring the animal, except to the extent of abrading or removing a small portion of skin. It at once opened up the way for making observations upon warm-blooded animals (one of the chief difficulties in our earlier investigations). For example: give a rabbit or a guinea-pig a small dose of chinoline, and the animal remains prostrate and quiet. Then cut off a little of the hair from the surface of the head at the back of the neck, and abrade the skin so as to have a moist surface; bring the electrodes into position, placing one in contact with the abraded surface and the other in contact with the surface of the cornea, and you will at once obtain the effect.

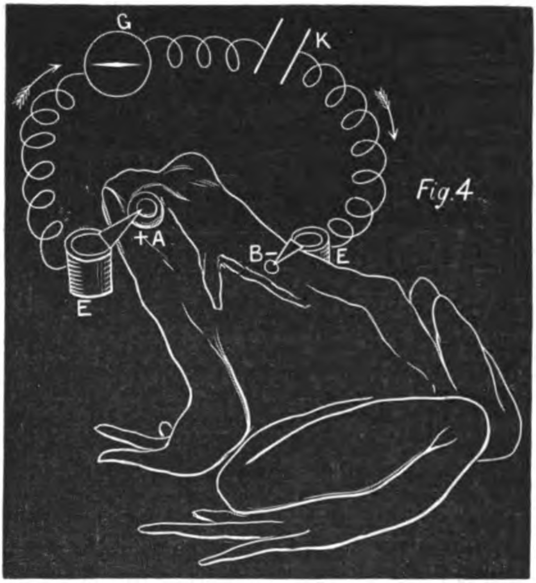

Diagram showing arrangement of apparatus in the experiment on eye of frog. A. Eye showing the electrode, E, in contact with it. B. Skin removed and subcutaneous tissue in contact with other electrode, E. K. Key. G. Galvanometer. Arrows indicate direction of current. Cornea, positive. Back, negative.

Action of Light in Warm-blooded same as in Cold-blooded Animals.—By the use of chinoline we were able to make experiments of the kind just described for a considerable time, without the necessity of maintaining artificial respiration. The result of those investigations upon warm-blooded animals has been to show that in these, as in the cold-blooded, light produces first an *increase* in the electric current on impact; continued light usually causes the electrical current to diminish; and on the removal of light, there is a second rise, as described in the case of the frog. In our earlier investigations, we always observed in the case of warm-blooded animals (when the eye had either been quite removed from the body or was receiving an inadequate supply of blood), that the action of light caused a negative variation, that is, a *diminution* in the electrical current. By improved methods, however, which have the effect of placing the eye in conditions more normal, we find that light causes a *positive* variation, that is, an increase; thus agreeing with what had hitherto been observed in the eye of the frog. This is a point worthy of notice. Du Bois-Reymond showed, even in the case of sensory nerves, that physiological action caused a *negative* variation. But it appears that in the case of the retina the action of the normal stimulus is to cause a positive not a negative variation.

Experiment with the Living Lobster.—The action of light can be readily shown in this animal. Fix it loosely in a cloth, and lay it on the table in a slightly oblique position. With a small trephine remove a circular portion of the carapace, about three millimètres in diameter, and expose the moist tegumentary surface. Bring one electrode into contact with this surface, while the other touches the cornea. The usual effects of light may then be noted; but in the case of the lobster, the variation caused by the impact is greater than what we have noticed in any other animal, often amounting to one-tenth of the total amount of current. Another interesting experiment, comparable with that of the two eyes just described, may be made on the lobster by placing an electrode in contact with each cornea. The result frequently is apparently no current, but in reality the currents neutralize each other. Light falling on the one eye causes the needle to move, say to the left, while if it fall on the other eye, the needle swerves to the right. When the eye of the lobster, removed from the body, was divided longitudinally into segments, each segment was found sensitive to light. The effect of light was then to increase the primary current, but no inductive action was observed on withdrawal.

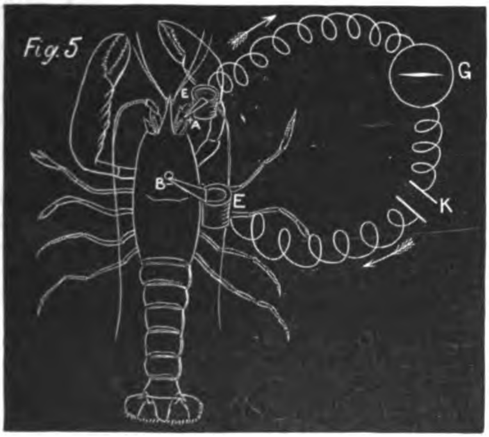
Fig. 5

Diagram showing arrangement of apparatus in experiment on living lobster. A. Corneal surface, having electrode, E, in contact with it. B. Portion of carapace removed so as to expose moist surface for electrode, E. K. Key. G. Galvanometer. Arrows indicate direction of current.

This observation is interesting as a confirmation of the views of physiologists regarding the mode of action of a compound eye.

Mode of Experiment on Eye of Fish.—Recently we were enabled to perform an experiment upon the eye of a fish in a very simple way, by a method adopted in Professor Stricker's laboratory in Vienna some months ago for another purpose. Take a fish and give it a very small dose of woorara. It soon becomes almost motionless, and sinks in some cases to the bottom of the vessel. The animal would soon die in consequence of paralysis of the movement of the gills necessary for respiration. But, if we take the animal out of the water, put it upon a glass plate, introduce a little bit of cork under each gill, and then by means of an indiarubber tube placed in the mouth allow a little water to flow over the gills, the fish will live out of water in that condition for many hours. By this method we were able to perform the experiment upon the eye of a fish with the same results.

Observation on Human Eye.—Having succeeded in detecting the action of light on the retina of the living warm-blooded animal without any operative procedures, it appeared possible to apply a similar method to the eye of man. For this purpose, a small trough of clay or paraffin was constructed round the margin of the orbit, so as to contain a quantity of dilute salt solution, when the body was placed horizontally and the head properly secured. Into this solution the terminal of a non-polarizable electrode was introduced, and in order to complete the circuit the other electrode was connected with a large guttapercha trough containing salt solution, into which one of the hands was inserted. By a laborious process of education, it is possible to diminish largely the electrical variation due to the involuntary movements of the eye-ball, and by fixing the eye on one point with concentrated attention, another observer, watching the galvanometer, and altering the intensity of the light, can detect an electrical variation similar to what is seen in other animals. This method, however, is too exhausting and uncertain to permit of quantitative observations being made.

Explanation of Variation in Direction of Current.—One phenomenon particularly attracted the attention of physiologists, and especially of those who first saw the experiments: viz. that sometimes, in the case of the eye of the frog, light produced an increase in the electrical current, and in other cases a diminution. This we could not at first account for. But we have been able to make out that the positive and negative variation, or the increase or diminution of the natural current on the action of light, depends upon the direction of the primary current, when the cornea and brain are in circuit. If the cornea be positive and the brain be negative, then light produces an *increase* of the electrical current. If, on the other hand, the cornea be negative and the brain positive, light then produces a *diminution* in the electrical current. It is thus conclusively shown that the current superadded, or if we may use the language, induced by the action of light, is always in the same direction; only in the one case it is added to, and in the other subtracted from, the primary current.

The Use of equal and opposite Currents.—We have performed many experiments in which equal and opposite currents were transmitted through the galvanometer at the same time, and observed the effect of light in these circumstances. By the use of resistance coils, it was not difficult to balance the current from the eye; but, owing to the inconstancy of even a Daniell's cell in such experiments as these, it was impossible to avoid fluctuations which might possibly have been mistaken for those due to the action of light. This difficulty was got over by what we formerly called *the double eye experiment*, in which two similar eyes are placed in reversed positions on the electrodes, so that the current from the one neutralizes that of the other. When this is accomplished, it is easy by means of a blackened box, having a shutter at each side, to allow light to fall on either the one eye or the other, and it is then shown that the galvanometer needle moves either to the right or left, according to the eye affected. Instead of removing the eyes from the head and balancing them as just described, it is a much better method to apply the two electrodes directly to the corneas in their natural position. By a little manipulation, it is possible to obtain two positions, that seemingly give no electrical current. In these circumstances, light, allowed to fall on the one eye or the other, produces the effects above detailed.

Action of Polarized Light and Colours of Spectrum.—The next point recently investigated is the action of polarized light and the various complementary colours. We arrived at the results of our earlier experiments with the colour-spectrum in various ways, such as by passing light through solutions having various absorptive powers, by the direct coloured rays of the spectrum, &c., but always with the same conclusion—namely, that the most luminous rays produce the greatest effect. For studying the action of polarized light, we have recently used the simple contrivance of a black box, having a hole on one side of it, placed over the eye. Opposite the hole we placed two cylindrical tubes of brass, each carrying a Nicol's prism, and between the two prisms a thin plate of quartz is introduced, producing the various colours of polarized light on rotating one of the prisms. The general results were exactly the same as when we used the colours of the spectrum. In all cases, the impact of the yellow rays produced the greatest effect. It has also been ascertained by this method that the effect of the *impact* of light is much more regular than the effect of its removal. The results of one series of observations are given in the two following tables:

Action on Frog's Eye of Colours of Polarized Light.

	Initial Effect.	Final Effect.
Purple	rise of 3	rise of 14
Light Blue	,, 5	,, 12
Red Violet	,, 5	,, 15
Blue	,, 7	,, 20
Red	,, 8·5	,, 15
Orange Red	,, 10	,, 22
Green Blue	,, 10	,, 24
Green	,, 13	,, 24
Yellow	,, 16	,, 24
Rose	,, 8	,, 19

Action on Frog's Eye of Spectrum of Oxyhydrogen Flame.

	Initial Effect.	Final Effect.
Yellow, near Orange	rise of 70	rise of 10
Green Yellow	,, 25	,, 5
Green—low	,, 15	,, 0
Green—high	,, 15	,, 0
Green—higher	,, 18	,, 8
Yellow Green	,, 85	,, 35
Yellow	,, 80	,, 40

Determination of Electro-motive Force.—Very soon after the first experiments were announced, certain physiologists said, that although we had obtained the results of the action of light which I have just described as indicated by the galvanometer, we had no right to say that there was a change in the electro-motive force as stated in the earlier communications. We had, however, satisfied ourselves that the effect was due to an alteration in the electro-motive force, but reserved details to the second part of our investigations. At first, in attempting this Sir William Thomson's electrometer was used, but the amount of electric potential to be measured was too small to get good results. Another plan of determining the electro-motive force was adopted. This was the method introduced by Mr. Latimer Clarke, the eminent electrician, and described in his work on 'Electrical Measurements.' The instrument devised for this purpose is called by him a Potentiometer, and measures electro-motive forces by a comparison of resistances. Practically we found the Daniell's cell far too strong a battery to use as a standard of comparison. A thermo-electric junction of bismuth and copper was substituted for it. One end of the junction was constantly heated by a current of steam passing over it, the other being immersed in melting ice. The electro-motive force of this thermo-electric junction, as estimated many years ago by Regnault, is extremely constant, and is about the $\frac{1}{175}$th part of a Daniell's cell. By means of this arrangement the following results were obtained:—The electro-motive force of the nerve-current dealt with in our experiments on the eye and the brain of a frog varies from the $\frac{1}{300}$th to the $\frac{1}{400}$th of a Daniell's cell. Light produced an alteration in the electro-motive force. This change was, in many instances, not more than the $\frac{1}{10000}$th of a Daniell's cell. But though small, it was quite distinct, and enabled us to say positively that light produced a variation in the amount of the electro-motive force. By the same arrangement, the gastrocnemius muscle of a well-fed frog gave $\frac{1}{35}$th of a Daniell; the same muscle from a lean frog which had been long kept, gave $\frac{1}{240}$th of a Daniell; and the sciatic nerve of the well-fed frog $\frac{1}{480}$th of a Daniell. Dr. Charles Bland Radcliffe states, in his 'Dynamics of Nerve and Muscle,' p. 16, that he obtained, by means of Sir William Thomson's quadrant electrometer, from a muscle, a positive charge equal to about the tenth of a Daniell's cell, a much greater amount than ascertained by the method I have just described.

The electro-motive force existing between cornea and posterior portion of the sclerotic in a frog amounts to $\frac{1}{130}$th part of a Daniell, and between the cornea and cross section of the brain is about four-fifths of the above.

Effect of Temperature on the Eye of the Frog.—From numerous experiments on the irritability of muscle induced by the excitation of nerve, it has been satisfactorily proved that a temperature of about 40° C. destroys the action of motor nerves in cold-blooded animals. Up to the present time, we are acquainted with no observations as to the temperature at which a terminal sense organ becomes incapable of performing its functions. Having satisfactorily proved that the retina is the structure in the eye producing the electrical variation we have observed, it becomes evident that as long as this phenomenon can be detected, the retina is still capable of discharging its normal functions. In order to investigate thoroughly the effect of an increasing temperature on the sensibility of the retina, a method of procedure was adopted of which the following may be taken as a general account: a frog was killed, the two eyes removed rapidly from the body, the one eye was placed on electrodes and maintained at the ordinary temperature of 16° C., while the other was placed on similar electrodes, contained in the interior of a water bath having a glass front, the sides of the air chamber being lined with black cotton wool saturated with water. Into this chamber a delicate thermometer was inserted, and the currents coming from the two eyes were alternately transmitted to the galvanometer every five minutes by means of a commutator, the temperature and the electrical variation produced by the same amount of light being noted in each case. The general results are shown in the following table:

Table showing Comparative Effect of Temperature on Sensibility of Frog's Eye.

Eye kept continuously at 16° C.		Eye at different Temperatures.		
Initial Effect.	Final Effect.	Temperature.	Initial Effect.	Final Effect.
55	28	16° C.	58	21
61	28	19° C.	55	16
53	27	24° C.	65	14
53	39	29° C.	97	5
53	45	29° C.	103	—4
60	45	37° C.	65	—3
60	50	38° C.	65	—4
53	41	43° C.	12	—5
60	40	43° C.	no effect.	no effect.

The initial amount of current was, however, increased on the whole by the action of the higher temperature, thus showing that the sensibility to light does not depend on the amount of current circulating through the galvanometer. It will be observed, on inspecting this table, that the eye maintained at the temperature of 16° C. remains tolerably constant in its initial action, although it gradually gets more sluggish, whereas the final effect steadily rises. On the other hand, in the case of the eye subjected to a higher temperature, the initial effect seems to have a maximum about 29° C., then gradually diminishes, and vanishes about 43° C., the final effect continuously falling and being actually reversed. To succeed in this experiment, it is necessary to heat the electrodes which are to be used in the water bath up to 40° C., in order to be certain that no changes are induced in the electrodes themselves that might be mistaken for those above mentioned. An eye that had been placed in dilute salt solution along with lumps of ice was found to have the usual sensibility to light.

Effect of Temperature on Eye of Pigeon.—Having succeeded in experimenting with a water bath, in the manner above described, it appeared interesting to ascertain if the eye of a warm-blooded animal would be benefited by being maintained at the normal temperature of body. The head of a pigeon was placed in the water bath, at a temperature of 40° C., the eyes were found sensitive to light, the action, however, being always a negative variation; but instead of vanishing quickly, as it does at the ordinary temperature, kept up its activity for at least an hour. For example, in one experiment, the electrodes, being placed on the two corneas, so that the currents were balanced, sensibility was active for an hour and a quarter, but half an hour later it had almost disappeared. In this experiment, the sensibility of the eye is shown by the large deflection produced by a single candle at different distances—thus:

Distance of Candle from Eye.	Divisions of Galvanometer Scale.
9 feet	100
6 feet	180
3 feet	230
1 foot	420

Sensibility of the Optic Nerve.—We have formerly shown that when the retina is entirely removed from the eye-ball, and the optic nerve is still adherent to the sclerotic, no effect of light can be detected; and it now appeared possible to examine this question by repeating Donders' experiment of focussing an image on the optic disc in the uninjured eye, when no electrical disturbance ought to occur. This was done in the eye of the pigeon, but an image free from irradiation on the optic disc could not be produced, and consequently there was always an electrical effect observed.

Exhaustion and Stimulation of the Retina.—When the same light from a fixed position is allowed to act on the eye for successive intervals of time, say two minutes of light and two minutes of darkness, it gradually falls off in electrical sensibility. Thus, a candle at 9 inches gives the following results when successively used as a stimulus:

	Initial Effect.	Final Effect.
1st experiment	259	254
2nd "	171	276
3rd "	140	282
4th "	122	274

These figures show a rapid fall of the initial effect. In these circumstances, it is evident that the image being always localized on the same minute portion of the retina, only a few of the rods and cones of that structure are really exhausted. If the eye be allowed repose in the dark for a period of from half an hour to an hour, it will regain as much as triple the exhausted sensibility. But another mode of proving that only a minute portion of the retina was affected was to show that an alteration of position of the image by a slight movement of the luminous body was followed by a new electric variation. In order to vary and extend the action of a retinal image, it is necessary to suspend a steady lamp by means of an indiarubber cord or spiral spring, so as to be able, by inducing vibrations in any direction, to stimulate in rapid succession different retinal areas. On oscillating a pendulum of this kind, we have observed an electrical variation whenever the amplitude of the vibrations is increased, and by inducing a combination of vibrations, the electrical variation observed corresponds to what would be found if the luminous intensity were sixteen times as great as that of the stationary light. Similar experiments may be made by throwing an image from a small silver mirror connected with a metronome. The rapid exhaustion of the eye may be most readily demonstrated by cutting off the anterior half of the eye, leaving the vitreous humour in contact with the retina, observing the effect of a candle, and then subjecting it to the action of a magnesium lamp. The sensibility will now be enormously diminished. The electrical variations resulting from the respective actions of a candle and a magnesium lamp placed at the same distance from the eye were as follows:

	Initial Effect.	Final Effect.
Candle	38	78
Magnesium lamp	120	135

This experiment proves that an increase of 200 per cent. in the illuminating power of a source of light only triples the electrical effect. Thus the eye becomes less sensitive as the illumination increases.

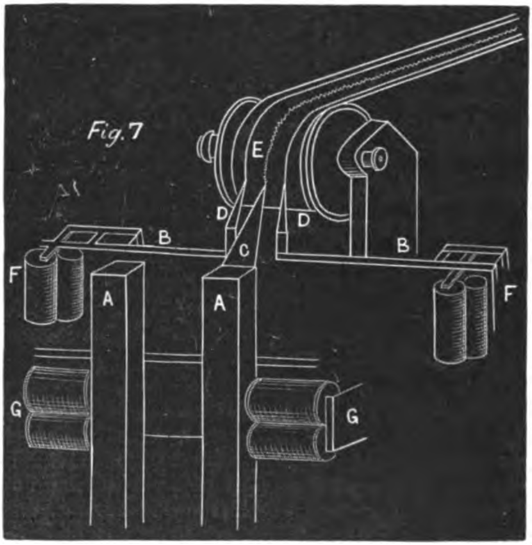

Diagram showing the recording portion of Regnault's Chronograph. A A. Limbs of recording fork, worked by electro-magnets, G G. C. Stilette on limb of recording tuning fork. B B. Levers in connection with armatures of electro-magnets, F F, and bearing markers D D, which, along with C, record on E, a strip of blackened paper passing over pulley.

Chronometrical Observations.—The last point I wish to bring under your notice, is what we have recently been doing in the way of measuring the time required from the initial impact of light before the electrical variation is produced. As the electrical variation has been shown to agree with our consciousness of luminous effects, it became an interesting point to ascertain whether the time occupied by the action of light upon the eye of the frog is similar to the time occupied in its action upon the eye of man. A good many years ago, Professor Donders and his pupil, Schelske, performed a number of experiments by which they determined that the time required by the human being to observe light and to signal back the impression occupied about $\frac{1}{10}$th of a second. That is to say, $\frac{1}{10}$th of a second is occupied by the action of light on the eye, the transmission of nerve-current to the brain, the change induced in the brain during perception and volition, the time for the transmission of the nerve-current to the muscles, on signalling the result, and the time occupied by muscular contraction. The true period of latent stimulation in the case of man must therefore be a very small fraction of a second. In order to attempt a solution of this problem we have used a chronograph made by Dr. König, of Paris. A diagram of the recording portion of the instrument is given above. The experimental arrangements were as follows: The galvanometer, the eye apparatus, and the chronograph being in separate rooms, one observer was stationed at the galvanometer for the purpose of signalling the moment the needle worked, which was recorded by one of the markers D in the diagram, the other marker being used to register the time of initial action.

The *first experiment* was to transmit at a known moment, through the eye circuit in the dark room, a quantity of current equal in amount to the electrical variation produced when the eye was stimulated by a flash of light from a vacuum tube, and to record the difference of time between the origin of the current and the observer's signal from the galvanometer.

The *second experiment* was to flash a vacuum tube at a known moment in a room where the eye was placed, and to record as before the instant the galvanometer was affected. From the first observation we ascertain the minimum amount of time necessary to overcome the inertia of the instrument, the observer's personal equation, and the signalling under the conditions of the experiment. If this result is subtracted from the record of the second observation, the difference will represent the latent period of light stimulation. From a large number of experiments made on the eye of the frog we have found the latent period amounts to less than $\frac{1}{10}$th of a second, but its absolute value must be ascertained by some method not liable to the variations that are inevitable to the process described. Altogether the problem is one of great difficulty, but we hope to continue the investigations.

[J. D.]

GENERAL MONTHLY MEETING,

Monday, April 3, 1876.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Mrs. Wm. Amhurst Tyssen Amhurst,
Professor Albert James Bernays, Ph.D.
Frederick Joseph Bramwell, Esq. F.R.S.
Lieut.-Col. Christopher Buckle,
Manuel Garcia, Esq. Hon. M.D.
The Lady Albert Leveson Gower,
Miss Wilhelmina Lydia Hall,
Miss Mary Hall McClean,
H. Saxon Snell, Esq.
Samuel Sandars, Esq. M.A. Cant.
William Woolley Turton, Esq.
George Whitaker Walter, Esq.
Allan V. White, Esq.
James Wilson Remington-Wilson, Esq.

were *elected* members of the Royal Institution.

The following Arrangements of the Lectures after Easter were announced:—

Professor P. M. Duncan, F.R.S. — Four Lectures on the Comparative Geology and former Physical Geographies of India, Australia, and South Africa; on Tuesdays, April 25 to May 16.

Professor Tyndall, D.C.L. LL.D. F.R.S. — Seven Lectures on Voltaic Electricity; on Thursdays, April 27 to June 8.

Henry Woodward, Esq. F.R.S. F.G.S.—Two Lectures on Crustacea; on Saturdays, April 29 and May 6.

Professor W. G. Adams, F.R.S.—Three Lectures on some of Wheatstone's Discoveries and Inventions; on Tuesdays, May 23 to June 6.

Frederick J. Furnivall, Esq.—Two Lectures on Chaucer; on Saturdays, May 13 and 20.

Professor Henry Morley, M.A.—Three Lectures on King Arthur's Place in English Literature; on Saturdays, May 27 to June 10.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, *viz.*:—

FROM

Agent-General for New South Wales—Financial Statement of Hon. J. Robertson. fol. 1875.

Agricultural Society of England, Royal—Journal, Second Series, Vol. XII. Part 1. 8vo. 1876.

American Philosophical Society—Proceedings, No. 95. 8vo. 1875.
Asiatic Society of Bengal—Journal, 1875, Part I. No. 3; Part II. No. 2. 8vo.
  Proceedings, 1875. No. 9. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVI. Nos. 3, 4. 8vo. 1876.
British Architects, Royal Institute of—Sessional Papers, 1875–6. Nos. 5–8. 4to.
Chemical Society—Journal for Feb. 1876. 8vo.
Editors—American Journal of Science for March, 1876. 8vo.
  Athenæum for March, 1876. 4to.
  Chemical News for March, 1876. 4to.
  Electrical News for March, 1876.
  Engineer for March, 1876. fol.
  Journal for Applied Science for March, 1876. fol.
  Nature for March, 1876. 4to.
  Nautical Magazine for March, 1876. 8vo.
  Pharmaceutical Journal for March, 1876. 8vo.
  Telegraph Journal for March 15, 1876. 8vo.
Franklin Institute—Journal, Nos. 602, 603. 8vo. 1876.
Iron and Steel Institute—Journal, 1875. No. 2. 8vo.
Linnean Society—Journal, Botany, No. 82. 8vo. 1876.
Meteorological Office—Quarterly Weather Reports, 1874. Part 2. 4to. 1876.
Manchester Geological Society—Transactions, Vol. XIV. Part 2. 8vo. 1876.
New Zealand Government—Statistics for 1874. fol. 1875.
Photographic Society—Journal, No. 265. 8vo. 1876.
Preussische Akademie der Wissenschaften—Monatsberichte, Dec. 1875. 8vo.
Royal Society of Edinburgh—Transactions, Vol. XXVII. Part 3. 4to. 1874–5.
  Proceedings, No. 90. 8vo. 1874–5.
Royal Society of London—Proceedings, No. 167. 8vo. 1876.
Royal Swedish Academy of Sciences—Handlingar (Memoirs), 1870, 1871, 1873. 3 vols. 4to.
  Bi-hang (Supplement): I. II. 8vo.
  Ofversigt (Bulletins). Nos. 28–31. 8vo.
  Lefnadsteckingar: I. 3. 8vo.
Scottish Society of Arts—Transactions, Vol. IX. Part 3. 8vo. 1875.
Smithsonian Institution—Smithsonian Report, 1874. 8vo. 1875.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, March, 1876. 8vo.
Tennant, Professor, F.G.S.—T. Sopwith, F.R.S. Description of Geological Models. 12mo. 1875.
Victoria Institute—Journal, No. 36. 8vo. 1876.
Whitburn, T. Esq. (the Editor)—Westward Hoe for Avalon in the New-found-land: as described by Captain R. Whitbourne, of Exmouth, Devon, 1622. 12mo. 1870.
Wild, M. H. (the Editor)—Repertorium für Meteorologie. Band IV. Heft 2. 4to. 1875.
Woodworth, Dr. J. M. U.S.—Report of U.S. Marine Service. 8vo. 1874.
  Cholera Epidemic in the United States, 1873. 8vo. 1875.
Yorkshire Archæological Society—Journal, Parts 13, 14. 8vo. 1875.

WEEKLY EVENING MEETING,

Friday, April 7, 1876.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Edward Burnett Tylor, Esq. F.R.S.

Ordeals and Oaths.

In primitive stages of society the clannish life of rude tribes may well have been more favourable to frank and truthful and faithful relations between man and man, than our wider and looser social intercourse can be. Yet one can see from the habits of modern savages that already in early savage times society was setting itself to take measures against men who broke faith, to save themselves from harm, or to gain some coveted good. At the stage of civilization where social order was becoming regular and settled, the wise men turned their minds to devise guarantees stronger than mere yes and no. Thus the ordeal and the oath were introduced that wrong-doing should not be concealed or denied, that unrighteous claims should not be backed by false witness, and that covenants made should not be broken.

The principles on which these ordeals and oaths were invented and developed may to this day be plainly made out. It is evident that the matter was referred to the two intellectual orders of early times—the magicians and the priests. Each advised after the manner of his profession. The magician said, With my symbols and charms I will try the accused, and bind the witness and the promiser. The priest said, I will call upon my spirits, and they shall find out the hidden thing, and punish the lie and the broken vow. Now magic and religion are separate in their nature and origin. *Magic* is based on a delusive tendency arising out of the association of ideas, namely, the tendency to believe that things which are ideally connected in our minds must therefore be really connected in the outer world. *Religion* is based on the doctrine of spiritual beings, souls, demons, or deities, who take cognizance of men and interpose in their affairs. It is needful to keep this absolute distinction clear in our minds, for on it depends our finding our mental way through a set of complicated proceedings, in which magical and religious elements have become mixed in the most intricate ways. Well they might, considering how commonly the professions of sorcerer and priest have overlapped so as even to be combined in one and the same person. But it seems from a general survey of the facts of ordeals and oaths, that on the whole the magical element in them is earliest and underlying, while the religious element is apt to come in later in history, often only taking up and consecrating some old magical process.

In the series of instances to be brought into view, this blending of the religious with the magical element will be repeatedly observable. It will be seen also that the ordeal and the oath are not only allied in their fundamental principles, but that they continually run into one another in their use. Oaths, we shall see, are made to act as ordeals, and ordeals are brought in as tests of oaths. While recognizing this close connection, it will be convenient to divide the two, and take them in order, according to their practical application, ordeals being proceedings for the discovery of wrong-doers, while oaths are of the nature of declarations or undertakings.

The association of ideas which serves as a magical basis for an ordeal is quite childish in its simplicity. Suppose it has to be decided which of two men have acted wrongfully, and appeal is had to the ordeal. There being no evidence on the real issue, a fanciful issue is taken instead, which can be settled, and the association of ideas does the rest. Thus in Borneo, when two Dayaks have to decide which is in the right, they have two equal lumps of salt given them to drop together into water, and the one whose lump is gone first is in the wrong. Or they put two live shell-fish on a plate, one for each disputant, and squeeze lime-juice over them, the verdict being given according to which man's champion mollusk moves first. This reasoning is such as any child can enter into. Among the Sandwich islanders, again, when a thief had to be detected, the priest would consecrate a dish of water, and the suspected persons, one by one, held their hands over it, till the approach of the guilty was known by the water trembling. Here the connection of ideas is plain. But we may see it somewhat more fully thought out in modern Europe, where the old notion is not forgotten that the executioner's sword will tremble when a thief draws near, and even utter a dull clang at the approach of a murderer.

Starting with the magical ordeal, we have next to notice how the religious element is imported into it. Take the ordeal of the balance, well known to Hindu law. A rude pair of scales is set up with its wooden scale-beam supported on posts, the accused is put in one scale, and stones and sand in the other to counterpoise him, then he is taken out, and the balance is called upon to show his guilt by letting him go down on re-weighing, or his innocence by raising him up. This is pure magic, the ideal weight of guilt being by mere absurd association of ideas transferred to material weight in a pair of scales. In this process no religious act is essential, but in practice it is introduced by prayers and sacrifices, and a sacred formula appealing to the great gods who know the walk of men, so that it is considered to be by their divine aid that the accused, when put back into the scale, rises or falls at once in material fact and moral metaphor. If he either goes fairly up or down, the case is clear. But a difficulty arises if the accused happens to weigh the same as he did five minutes before; so nearly, at least, as can be detected by a pair of heavy wooden scales which would hardly turn within an ounce or two. This embarrassing possibility has, in fact, perplexed the Hindu lawyers not a little. One learned pundit says—he is guilty unless he goes right up! A second suggests—weigh him again! A third distinguishes with subtlety—if he weighs the same he is guilty, but not so guilty as if he had gone right down! The one only interpretation that never occurs to any of them is, that sin may be an imponderable. We may smile at the Hindu way of striking a moral balance, but it should be remembered that a similar practice, probably a survival from the same original Aryan rite, was kept up in England up to the last century. In 1759, near Aylesbury, a woman who could not get her spinning-wheel to go round, and naturally concluded that it had been bewitched, charged one Susannah Haynokes with being the witch. At this Susannah's husband was indignant, and demanded that his wife should be allowed to clear herself by the customary ordeal of weighing. So they took her to the parish church, stripped her to her under-garments, and weighed her against the church Bible; she outweighed it, and went home in triumph. Here the metaphor of weighing is worked in the opposite way to that in India; but it is quite as intelligible, and not a whit the worse for practical purposes. For yet another case, how an old magical process may be afterwards transformed by bringing in the religious sanction, we may look at the ancient classic sieve and shears, the sieve being suspended by sticking the points of the open shears into the rim, and the handles of the shears balanced on the forefingers of the holders. To discover a thief, or a lover, all that was required was to call over all suspected names, till the instrument turned at the right one. In the course of history, this childish divining-ordeal came to be christianized into the key and Bible, the key of course to open the secret, the Bible to supply the test of truth. For a thief-ordeal, the proper mode is to tie in the key at the verse of the 50th Psalm: "When thou sawest a thief, then thou consentedst with him," and then when the names are called over, at the name of the guilty one the instrument makes its sign by swerving and falling from the holder's hands. This is interesting as being almost the only ordeal which survives in common use in England; it may be met with in many an out-of-the-way farmhouse. It is some years since English rustics have dared to "swim" a witch, that is, to put in practice the ancient water-ordeal, which our folklore remembers in its most archaic Aryan form. Its essential principle is as plainly magical as any; the water, being set to make the trial, shows its decision by rejecting the guilty, who accordingly comes up to the surface. Our ancestors, who did not seize the distinction between weight and specific gravity, used to wonder at the supernatural power with which the water would heave up a wicked fellow, even if he weighed sixteen stone.

Mediæval ordeals, by water or fire, by touch of the corpse, or by wager of battle, have fallen to mere curiosities of literature, and it is needless to dwell here on their well-known picturesque details, or to repeat the liturgies of prayer or malediction said or sung by the consecrating priests. It is not by such accompanying formulas, but by the intention of the act itself, that we must estimate the real position of the religious element in it. Nowhere is this so strong as in what may be called the ordeal by miracle, where the innocent, by divine help, walks over the nine red-hot ploughshares, or carries the red-hot iron bar seven paces, or drinks a dose of deadly poison, and is none the worse for it; or, on the other hand, where the draught of harmless water cursed or consecrated by the priests will bring within few days dire disease on him or her who, being guilty, has dared to drink of it.

Looking at the subject from the statesman's point of view, the survey of the ordeals of all nations and ages enables us to judge with some certainty what their practical effect has been for evil or good. Their basis being mere delusive imagination, when honestly administered their being right or wrong has been matter of mere accident.

It would, however, be a mistake to suppose that fair-play ever generally prevailed in the administration of ordeals. As is well known, they have always been engines of political power in the hands of unscrupulous priests and chiefs. Often it was unnecessary even to cheat, when the arbiter had it at his pleasure to administer either a harmless ordeal like drinking cursed water, or a deadly ordeal by a dose of aconite or physostigma. When it comes to sheer cheating, nothing can be more atrocious than this poison-ordeal. In West Africa, where the Calabar bean is used, the administerers can give the accused either a dose which will make him sick, and so prove his innocence, or they can give him enough to prove him guilty and murder him in the very act of proof. When we consider that over a great part of that great continent this and similar drugs usually determine the destiny of people inconvenient to the fetish-man and the chief, the constituted authorities of church and state, we see before us one efficient cause of the unprogressive character of African society. The famed ordeal by red-hot iron, also, has been a palpable swindle in the hands of the authorities. In India and Arabia it is a recognized test of guilt or innocence to lick a hot iron; now no doubt the initiated know that innocent or guilty alike can lick a white-hot iron with impunity, as any blacksmith will do, and as I have done myself, the layer of vapour in a spheroidal state preventing any chemical contact with the skin. As for the walking over red-hot ploughshares, or carrying a red-hot iron bar seven paces in the palm of the hand, their fraudulent nature fits with the fact that the judges who administered them took their precautions against close approach of spectators much more carefully than the jugglers do who handle the red-hot bars and walk over the ploughshares at the circus nowadays; and moreover, any list of cases will show how inevitably the friend of the church got off, while the man on the wrong side was sure to "lose his cause and burn his fingers." Remembering how Queen Emma with uplifted eyes walked over the ploughshares without knowing it, and asked when the trial was to begin, and how after this triumphant issue one-and-twenty manors were settled on the bishopric and church of Winchester, it may be inferred with some probability that the glowing ploughshares glowed with nothing more dangerous than daubs of red paint.

Almost the only effect of ordeals which can be looked upon as beneficial to society, is that the belief in their efficacy has done something to deter the credulous from crime, and still more often has led the guilty to betray himself by his own terrified imagination. Visitors to Rome know the great round marble mask called the Bocca della Verità. It is but the sink of an old drain, but many a frightened knave has shrunk from the test of putting his hand into its open mouth and taking oath of his innocence, lest it should really close on him, as tradition says it does on the forsworn. The ordeal by the mouthful of food is still popular in southern Asia for its practical effectiveness; the thief in the household, his mouth dry with nervous terror, fails to masticate or swallow fairly the grains of rice. So in old England the culprit could not swallow the consecrated cor-snæd or trial-slice of bread or cheese, but it stuck in his throat, as in Earl Godwin's in the story. To this day the formula, "May this mouthful choke me if I am not speaking truth!" keeps up the memory of the official ordeal. Not less effective is the ordeal by curse still used in Russia to detect a thief. The *babushka,* or local witch, stands with a vessel of water before her in the midst of the assembled household, and makes bread pills to drop in, saying to each in order: "Ivan Ivanoff, if you are guilty, as this ball falls to the bottom, so your soul will fall into hell." But this is more than any common Russian will face, and the rule is that the culprit confesses at sight. This is the best that can be said for ordeals. Under their most favourable aspect, they are useful delusions or pious frauds. At worst, they are those wickedest of human deeds, crimes disguised behind the mask of justice. Shall we wonder that the world, slowly trying its institutions by the experience of ages, has at last come to the stage of casting out the judicial ordeal? Or shall we rather wonder at the constitution of the human mind, which for so many ages has set up the creations of delusive fancy to hold sway over a world of facts.

From the Ordeal we now pass to the Oath. The oath, for purposes of classification, may be best defined as an asseveration made under superhuman penalty, such penalty being (as in the ordeal) either magical or religious in its nature, or both combined. Here, then, we distinguish the oath from the mere declaration or promise or covenant, however formal. For example, the covenant by grasping hands is not in itself an oath, nor is even that wide-spread ancient ceremony of entering into a bond of brotherhood by the two parties mixing drops of their blood, or tasting each other's. This latter rite, though often called an oath, can under this definition only be reckoned as a solemn compact. But when a Galla of Abyssinia sits down over a pit covered over with a hide, imprecating that he may fall into a pit if he breaks his word, or when in our police-courts we make a Chinaman swear by taking an earthen saucer and breaking it on the rail in front of the witness-box, signifying, as the interpreter then puts it in words, "If you do not tell the truth, your soul will be cracked like this saucer"—we have here two full oaths, of which the penalty, magical or religious, is shown in pantomime before us. (By the way, the English judges who authorized this somewhat sensational ceremony must have believed that they were calling on the Chinaman to take a judicial oath after the manner of his own country; but they acted under a mistake, for in fact the Chinese use no oaths at all in their law-courts.) Now we have to distinguish the real oaths from mere asseverations, such as are made in emphatic terms, or with descriptive gestures, merely for the purpose of showing the strength of resolve in the declarer's mind. Where, then, does the difference lie between the two? It is clearly to be found in the incurring of supernatural penalty. There would be no difficulty at all in clearing up the question, were it not that theologians have set up a distinction between oaths of imprecation and oaths of witness. Such subtleties, however, looked at from a practical point of view, are seen to be casuistic cobwebs, which a touch of the rough broom of common-sense will sweep away. The practical question is this, Does the swearer mean that by going through the ceremony he brings on himself, if he breaks faith, some special magic harm, or divine displeasure and punishment? If so, the oath is practically imprecatory; if not, it is futile, wanting the very sanction which gives it legal value. It does not matter whether the imprecation is stated or only implied. When a Beduin picks up a straw, and swears by Him who made it grow and wither, there is no need to accompany this with a homily on the fate of the perjured. This reticence is so usual in the world, that as often as not we have to go outside the actual formula and ceremony to learn what their full intention is. Let us now examine some typical forms of oath. The rude natives of New Guinea swear by the sun, or by a certain mountain, or by a weapon, that the sun may burn them, or the mountain crush them, or the weapon wound them, if they lie. The even ruder savages of the Brazilian forests, to confirm their words, raise the hand over the head or thrust it into their hair, or they will touch the points of their weapons. These two accounts of savage ceremony introduce us to customs well known to nations of higher culture. The raising of the hand toward the sky seems to mean here what it does elsewhere. It is in gesture calling on the Heaven-god to smite the perjurer with his thunderbolt. The touching of the head, again, carries its meaning among these Brazilians almost as plainly as in Africa, where we find men swearing by their heads and limbs, in the belief that they would wither if forsworn; or when among the Old Prussians a man would lay his right hand on his own neck, and his left on the holy oak, saying, "May Perkun (the Thunder-god) destroy me!"

As to swearing by weapons, another graphic instance of its original meaning comes from Aracan, where the witness swearing to speak the truth takes in his hand a musket, a sword, a spear, a tiger's tusk, a crocodile's tooth, and a thunderbolt (that is, of course, a stone celt). The oath by the weapon not only lasted on through classic ages, but remained so common in Christendom that it was expressly forbidden by a Synod; even in the seventeenth century, to swear on one's sword was still a legal oath in Holstein. As for the holding up of the hand to invoke the personal divine sky, the successor of this primitive gesture remains to this day among the chief acts in the solemn oaths of European nations.

It could scarcely be shown more clearly with what child-like imagination the savage conceives a symbolic action, such as touching his head or his spear, will somehow pass into a reality. In connection with this group of oaths, we can carry yet a step further the illustration of the way men's minds work in this primitive stage of association of ideas. One of the accounts from New Guinea is that the swearer, holding up an arrow, calls on Heaven to punish him if he lies; but by turning the arrow the other way, and using certain herbs, the oath can be neutralized. This is magic all over. What one symbol can do, the reverse symbol can undo. True to the laws of primitive magical reasoning, uncultured men elsewhere still carry on the symbolic reversal of their oaths. An Abyssinian chief who had sworn an oath he disliked has been seen to scrape it off his tongue, and spit it out. There are still places in Germany where the false witness reckons to escape the spiritual consequences of perjury by crooking one finger, to make it, I suppose, not a straight, but a crooked oath; or he puts his left hand to his side to neutralize what the right hand is doing. Here is the idea of our "over the left"; but so far as I know this has come down with us to mere schoolboy's shuffling.

It has just been noticed that the arsenal of deadly weapons by which the natives of Aracan swear includes a tiger's tusk and a crocodile's tooth. This leads us to a group of instructive rites belonging to Central and North Asia. A few years ago, probably to this day, there might be seen in law-courts in Siberia the oath on the bear's head. When an Ostyak is to be sworn, a bear's head is brought into court, and the man makes believe to bite at it, calling on the bear to devour him in like manner if he does not tell the truth. Now the meaning of this act goes beyond magic and into religion; for we are here in the region of bear-worship, among people who believe that this wise and divine beast knows what goes on, and will come and punish them. Nor need one wonder at this, for the idea that the bear will hear, and come, if called on, is familiar to German mythology. I was interested to find it still in survival in Switzerland a few years ago, when a peasant woman, whom a mischievous little English boy had irritated beyond endurance, pronounced the ancient awful imprecation on him, "The Bear take thee!" (Der Bär nimm dich!). Among the Hill tribes of India a tiger's skin is sworn on in the same sense as the bear's head among the Ostyaks. Rivers, again, which to the savage and barbarian are intelligent and personal divinities, are sworn by, in strong belief that their waters will punish him who takes their name in vain. We can understand why Homeric heroes swore by the rivers, when we hear still among Hindus how the sacred Ganges will take vengeance sure and terrible on the children of the perjurer. It is with the same personification, the same fear of impending chastisement from the outraged deity, that savage and barbaric men swore by sky or sun. Thus the Huron Indian would say in making solemn promise, "Heaven hears what we do this day!" and the Tunguz, brandishing a knife before the sun, would say, "If I lie may the sun plunge sickness into my entrails like this knife." We have but to rise one stage higher in religious ideas to reach the type of the famous Roman oaths by Jupiter, the Heaven-god. He who swore held in his hand a stone, praying that if he knowingly deceived, others might be safe in their countries and laws, their holy places and their tombs, but he alone might be cast out, as this stone now—and he flung it from him. Even more impressive was the great treaty-oath, where the pater patratus, holding the sacred flint that symbolized the thunderbolt, called on Jove that if by public counsel or wicked fraud the Romans should break the treaty first, "in that day, O Jove, smite thou the Roman people as I here to-day shall smite this swine, and smite the heavier as thou art stronger!" So saying, he slew the victim with the sacred stone.

These various examples may be taken as showing the nature and meaning of such oaths as belong to the lower stages of civilization. Their binding power is that of curses, that the perjurer may be visited by mishap, disease, death. But at a higher stage of culture, where the gods are ceasing to be divine natural objects like the Tiber or Ganges, or the sun or sky, but are passing into the glorified human or heroic stage, like Apollo or Venus, there comes into view a milder kind of oath, where the man enters into fealty with the god, whom he asks to favour or preserve him on condition of his keeping troth. Thus, while the proceeding is still an oath with a penalty, this penalty now lies in the perjurer's forfeiting the divine favour. To this milder form, which we may conveniently call the "oath of conditional favour," belong such classic phrases as, "So may the gods love me!" (Ita me Dii ament!), "As I wish the gods to be propitious to me!" (Ita mihi Deos velim propitios). I call attention to this class of oaths, of which we shall presently meet with a remarkable example nearer home. We have now to take into consideration a movement of far larger scope.

Returning to the great first-mentioned class of savage and barbaric oaths, sworn by gestures or weapons, or by invocations of divine beasts, or rivers, or greater nature-deities; the question now to be asked is, What is the nature of the penalties? They are that the perjurer may be withered by disease, wounded, drowned, smitten by the thunderbolt, and so forth; all these being temporal visible punishments. The state of belief to which the whole plan belongs is that explicitly described among the natives of the Tonga Islands, where oaths were received on the declared ground that the gods would punish the false swearer here on earth. A name is wanted to denote this class of oaths, belonging especially to the lower culture. Let us call them "mundane oaths." Now it is much above their original level in culture, that the thought comes in of the perjurer being punished in a world beyond the grave. This was a conception familiar to the Egyptians in their remotely ancient civilization. It was at home among the old Homeric Greeks, as when Agamemnon, swearing his mighty oaths, calls to witness not only Father Zeus, and the all-seeing sun, and the rivers and earth, but also the Erinnyes who down below chastise the souls of the dead, whosoever shall have been forsworn. Not less plainly is it written in the ancient Hindu Laws of Manu, "A man of understanding shall swear no false oath even in a trifling matter, for he who swears a false oath goes hereafter and here to destruction." To this stage of culture, then, belongs the introduction of the new "post-mundane" element into oaths. For ages afterwards, nations might still use either kind, or combine them by adding the penalty after death to that in life. But in the latter course of history, there comes plainly into view a tendency to subordinate the old mundane oath, and at last to suppress it altogether. How this came to pass is plain on the face of the matter. It was simply the result of accumulated experience. The continual comparison of opinion with facts could not but force observant minds to admit that a man might swear falsely on sword's edge or spear's point, and yet die with a whole skin, that bears and tigers are not to be depended on to choose perjurers for their victims, and that in fact the correspondence between the imprecation and the event was not real but only ideal. How judgment by real results thus shaped itself in men's minds, we may see by the way it came to public utterance in classic times, nowhere put more cogently than in the famous dialogue in the "Clouds" of Aristophanes. The old farmer Strepsiades asks, "Whence comes the blazing thunderbolt that Zeus hurls at the perjured?" "You fool," replies the Socrates of the play, "you smack of old Kronos' times—if Zeus smote perjurers, he would have been down on those awful fellows Simon and Kleonymos and Theoros. Why, what Zeus does with his bolt, is to smite his own temple, and the heights of Sunium, and the tall oaks! Do you mean to say that an oak-tree can commit perjury?" What is said here in chaff, full many a reasonable man in the old days must have said to himself in the soberest earnest, and once said or thought, but one result could come of it, the result which history shows us did come. The scene of the judicial oath was gradually changed, and the later kind, of which the penalty concerned the future life, remained practically in possession of the field.

As a point in the Science of Culture which has hitherto been scarcely, if at all, observed, I am anxious to call attention to the historical stratification of judicial oaths, from the lowest stratum of mundane oaths belonging to savage or barbaric times, to the highest stratum of post-mundane oaths such as obtain among modern civilized nations.

Roughly, the development in the course of ages may be expressed in the following two classifications:

Mundane	Oaths	Curse.
Mixed		Conditional favour.
Post-mundane		Judgment.

Though these two series only partly coincide in history, they so far fit that the judicial oaths of the lower culture belong to the class of mundane curse, while those of the higher culture in general belong to that of post-mundane judgment. Anthropologically, this is the most special new view I have here to bring forward. It forms part of a wider generalization, belonging at once to the Science of Morals and the Science of Religion. But rather than open out the subject into this too wide field, we may do well to fix it in our minds by tracing a curious historical point in the legal customs of our own country. Everyone knows that the modes of administering a judicial oath in Scotland and in England are not the same. In Scotland, where the witness holds up his hand toward heaven, and swears to tell the truth as he shall answer it at the Day of Judgment, we have before us the most explicit possible example of a post-mundane oath framed on Christian lines. In contrasting this with the English judicial oath, we first notice that our acted ceremony consists commonly in taking a New Testament in the hand and kissing it. Thus, unlike the Scotch oath, the English oath is sworn on a *halidome* (Anglo-Saxon *hâligdôm*, German *heiligthum*), a holy or sacred object. Many writers have fallen into confusion about this word, mystifying it into sacred judgment, or, "holy doom;" but it is a perfectly straightforward term for a sanctuary or relic, as "On tham haligdome swerian" = to swear by the relic. Now, this custom of swearing on a halidome belongs to far præ-Christian antiquity, one famous example being when Hannibal, then a lad of nine years old, was brought by his father to the altars and made to swear by touching the sacred things (tactis sacris) that when he grew up he would be the enemy of Rome. In classical antiquity the sacred objects were especially the images and altars of the gods, as it is put in a scene in Plautus, "Touch this altar of Venus!" The man answers, "I touch it," and then he is sworn. When this ancient rite came into use in early Christian England, the object touched might be the altar itself, or a relic-shrine like that which Harold is touching with his right forefinger in the famous scene in the Bayeux tapestry, or, it might be a Missal, or a book of the Gospels. In modern England a copy of the New Testament has become the recognized halidome on which oaths are taken, and the practice of kissing it has almost supplanted the older and more general custom of touching it with the hand. Next, our attention must be called to the remarkable formula in which (in England, not in Scotland) the invocation of the Deity is made, "So help me God!" or "So help you God!" Many a modern Englishman puzzles over this obscure form of words. When the question is asked what the meaning of the oath is, the official interpretation practically comes to saying that it means the same as the Scotch oath. But neither by act nor word does it convey this meaning. So obvious is the discrepancy between what is considered to be meant, and what is actually done and said, that Paley, remarking on the different forms of swearing in different countries, does not scruple to say that they are "in no country in the world, I believe, worse contrived either to convey the meaning, or impress the obligation of an oath, than in our own."

This remark of Paley's aptly illustrates a principle of the Science of Culture, which cannot be too strongly impressed on the minds of all who study the institutions of their own or any other age. People often talk of mystic formulas and mystic ceremonies. But the more we study civilization in its earlier stages, the more we shall find that formulas and ceremonies, both in law and in religion, are as purposeful and business-like as can be, if only we get at them anywhere near their origin. What happens afterwards is this, that while men's thoughts and wants gradually change, the old phrases and ceremonies are kept up by natural conservatism, so that they become less and less appropriate, and then as meaning falls away, its place is apt to be filled up with mystery. Applying this principle to the English oath-formula, we ask what and where it originally was. It was Teutonic-Scandinavian, for though corresponding formulas are known in Latin ("Ita me adjuvet Deus"), and in Old French ("Ce m'ait Diex," &c.), these are shown by their comparatively recent dates to be mere translations of the Germanic originals. Now although ancient English and German records fail to give the early history of the phrase, this want is fortunately supplied by a document preserved in Iceland. Some while after the settlement of the island by the Northmen, but long before their conversion to Christianity, the settlers felt the urgent need of a code of laws, and accordingly Ulfliot went to Norway for three years to Thorleif the Wise, who imparted to him his legal lore. Ulfliot went to Norway A.D. 925, so that the form of judicial oath he authorized, and which was at a later time put on record in the Icelandic Landnâmabôk, may be taken as good and old in Norse law. Its præ-Christian character is, indeed, obvious from its tenour. The halidome on which it was sworn was a metal arm-ring, which was kept by the godhi or priest, who reddened it with the blood of the ox sacrificed, and the swearer touching it said, in words that are still half English: "Name I to witness that I take oath by the ring, law-oath, so help me Frey, and Niördh, and almighty Thor (hialpi mer svâ Freyr, ok Niördhr, ok hinn almâttki Âss), as I shall this suit follow or defend, or witness bear or verdict or doom, as I wit rightest and soothest and most lawfully," &c. Here, then, we have the full and intelligible formula which must very nearly represent that of which we keep a mutilated fragment in our English oath. So close is the connection, that two of the gods referred to, Frey and Thor (who is described as the almighty god) are the old English gods whose names we commemorate in Friday and Thursday. The formula belongs, with the classic ones lately spoken of, to the class of oaths of conditional favour, "*so* help me *as* I shall do rightly," while Frey and Niördh are gods whom a Norse warrior would ask for earthly help, but who would scarcely concern themselves with his soul after death. It is likely that the swearer was not indeed unmindful of what the skalds sang of Nâströnd, the strand of corpses, that loathly house arched of the bodies of huge serpents, whose heads, turned inward, dripped venom on the perjurers and murderers within. But the primary formula is, as I have said, that of the oath of conditional favour, not of judgment. With the constituents of the modern English oath now fairly before us, we see that its incoherence, as usual in such cases, has a historical interpretation. What we have done is to transplant from archaic fetish-worship the ceremony of the halidome or consecrated object, and to combine with this one half of a præ-Christian formula of conditional favour, without the second half which made sense of it. Considering that to this combination is attached a theological interpretation which is neither implied in act nor word, we cannot wonder if in the popular mind a certain amount of obscurity, not to say mystery, surrounds the whole transaction. Nevertheless we may well deprecate any attempt to patch up into Scotch distinctness and consistency the old formula, which may well last untouched so long as judicial oaths shall remain in use in England.

Being in the midst of this subject, it may not be amiss to say a few words upon old and new ideas as to the administration of oaths to little children. The canon law expressly forbade the exacting of an oath from children under fourteen, "Pueri ante annos xiv. non cogantur jurare." This prohibition is derived from yet earlier law. The rough old Norsemen would not take oaths from children, as comes out so quaintly in the saga of Baldur, where the goddess makes all the beasts and birds and trees swear they would not harm him, but the little mistletoe only she craved no oath from, for she thought it was too young. Admitting the necessity of taking children's evidence somehow, the question is how best to do this. In England it must be done on oath, and for this end there has arisen in our courts a custom of putting the child through an inquisition as to the theological consequences of perjury, so as usually to extract from it a well-known definition, which the stiffest theologian will not stand to for a moment if put straight to him, but which is looked upon as a proper means for binding the conscience of a little child. Moreover, children in decent families learn to answer plain questions some years before they learn to swear, and material evidence is often lost by the child not having been taught beforehand the proper answers to make when questioned as to the nature of an oath. I heard of a case only lately, which was expected to lead to a committal on a charge of murder, and where an important point rested on the evidence of a young lad, who was to all appearance truthful, but who did not satisfy the bench that he understood the nature of an oath. Those in whom the ceremony of swearing a child arouses the feeling of physical repugnance that it does in myself, may learn with interest a fact as yet little known in England, and which sufficiently justifies my bringing forward the subject. Hearing that there was something to be learnt from Germany, I applied to the eminent jurist, Dr. Gneist, of Berlin, and hear from him that under the new German Rules of Procedure, which are expected shortly to come into force, the evidence of children under sixteen may be received without oath, at the discretion of the judge. In these days there is a simple rule, which an Englishman will do well to act up to, and that is, "Don't be beaten by a German!" Let us live in the heartiest fellowship with the Germans, and never let them get ahead of us if we can help it. In this matter of children's legal evidence they are fairly leaving us behind, by introducing a plan which is at once more humane and more effective than ours.

If now, looking at the subject as one of practical sociology, we consider what place the legal oath has filled in savage, barbaric, and civilized life, we must adjudge to it altogether higher value than to the ordeal. At certain stages of culture it has been one of the great forces of society. There was a time when Lycurgus the orator could tell the men of Athens that the oath is the very bond that holds the democracy together. There was a time when, as Montesquieu insists, an oath was so binding on the minds of the Romans that for its observance they would do more than even patriotism or love of glory could draw them to. In our own day its practical binding power is unmistakable over the consciences of a numerous intermediate class of witnesses, those who are neither truthful nor quite reckless, who are without the honesty which makes a good man's oath superfluous, who will indeed lie solemnly and circumstantially, but are somewhat restrained from perjury by the fear of being, as the old English saying has it, "once forsworn ever forlorn." Though the hold thus given is far weaker than is popularly fancied, it has from time to time led legislators to use oaths, not merely in special and solemn matters, but as means of securing honesty in the details of public business. When this has been done, the consequences to public morals have been disastrous. There is no need to hunt up ancient or foreign proofs of this, seeing how conspicuous an instance is the state of England early in the present century, while it was still, as a contemporary writer called it, "a land of oaths," and the professional perjurer plied a thriving trade. A single illustration will suffice, taken from the valuable treatise on oaths, published in 1834, by the Rev. Jas. Endell Tyler: "During the continuance of the former system of Custom House oaths, there were houses of resort where persons were always to be found ready at a moment's warning to take any oath required; the signal of the business for which they were needed was this inquiry: "Any damned soul here!" Nowadays the enormous excess of public oaths has been much cut down, and with the best results. Yet it must be evident to students of sociology that the world will not stop short at this point. The wider question is coming into view, What effect is produced on the every-day standard of truthfulness, by the doctrine that fraudulent lying is in itself a minor offence, but is converted into an awful crime by the addition of a ceremony and a formula? It is an easily-stated problem in moral arithmetic; on the credit side Government is able to tighten with an extra screw the consciences of a shaky class of witnesses and public officers; on the debit side the current value of a man's word is correspondingly depreciated through the whole range of public and private business. As a mere sober student of social causes and effects, dispassionately watching the tendencies of opinion, I cannot doubt for a moment how the public mind must act on this problem. I simply stand here and predict that where the judicial ordeal is already gone, there the judicial oath will sooner or later follow. Not only do symptoms of the coming change appear from year to year, but its greatest determining cause is unfolding itself day by day before observant eyes, a sight such as neither we nor our fathers ever saw before.

How has it come to pass that the sense of sanctity of intellectual truth, and the craving after its full and free possession, are so mastering the modern educated mind? This is not a mystery hard to unravel. Can any fail to see how in these latter years the methods of scientific thought have come forth from the laboratory and the museum, to claim their powers over the whole range of history and philosophy, of politics and morals? This very spot has been for years an active centre whence truth in thought has spread in wide waves through the outside world. Of intellectual truthfulness, truthfulness in word and act is the outward manifestation. In all modern philosophy, there is no principle more fertile than the doctrine so plainly set forth by Herbert Spencer, that truth means bringing our minds into accurate matching with the realities in and around us; so that both intellectual and moral truth are bound up together in that vast process of evolution whereby man is gradually brought into fuller harmony with the universe he inhabits. There need, then, be no fear that the falling away of such artificial crutches as those whose history I have traced to-night, should leave public truth maimed and halting. Upheld by the perfect fitting of the inner mind to the outer world, the progress of truth will be firmer and more majestic than in the ancient days. If, in time to come, the grand old disputation before

King Darius were to be re-enacted, to decide again the question, "What is the strongest of all things?" it would be said, as then, that "Truth abides and is strong for evermore, living and conquering from age to age." And the people, as of old, would say again with one voice, "Truth is great, and prevails!" *

[E. B. T.]

WEEKLY EVENING MEETING,

Friday, April 28, 1876.

George Busk, Esq. Treasurer and Vice-President, in the Chair.

George J. Romanes, Esq. M.A. F.L.S.

The Physiology of the Nervous System of Medusæ.†

For the sake of clearness in what follows, I shall begin by very briefly describing such features in the anatomy of the Medusæ as will afterwards be found more especially to concern us. The general form, then, of these animals varies in different species from that of a bell to that of a bowl, an umbrella, or a saucer. The external, or convex, surface is separated from the internal, or concave, surface by a thick mass of transparent and non-contractile jelly. The external surface itself consists of a thin layer, or sheet, of pavement epithelium cells, while the internal surface consists of a thin layer, or sheet, of muscular tissue. The structure thus far described constitutes the main bulk of the animal, and is called the swimming-bell. From the middle, or highest point, of the internal surface of the swimming-bell there always depends a contractile organ, which is called the manubrium or polypite. This structure, although it varies greatly in different species both as to size and form, is always the mouth and stomach of the animal. From the point at which the polypite is inserted into the swimming-bell there arise a system of nutrient tubes, which radiate quite down to the margin of the swimming-bell, where they all open into a continuous circular tube, which is called the marginal canal. In one large division of the Medusæ—the naked-eyed—the radial tubes are unbranched, and usually four in number, while in the other large division—the covered-eyed—the radial tubes are strongly branched. The margin of the swimming-bell, both in the naked- and covered-eyed Medusæ, supports a series of contractile tentacles, and also another series of bodies which are of great importance for us to-night. These are the so-called marginal bodies, or eye-specks, which vary in number, size, and structure in different species.

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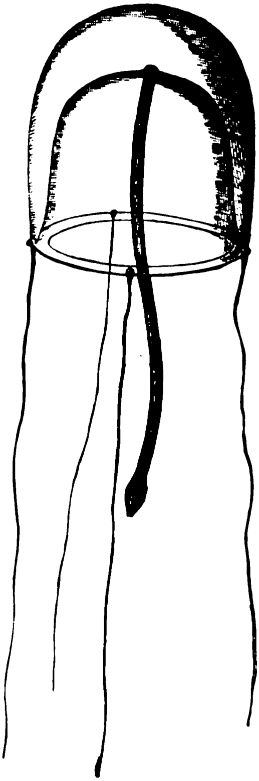
* Esdras i 4: *μεγάλη ἡ ἀλήθεια, καὶ ὑπερισχύει.* "*Magna est veritas, et prævalet.*"

† For a full account of the investigation see the Croonian Discourse before the Royal Society, to be published in the forthcoming volume of the 'Philosophical Transactions.'

In all the covered-eyed species, however, the eye-specks are in the form of little bags of crystals, which are protected by curiously-shaped "hoods" or "covers" of gelatinous tissue. In the naked-eyed species these hoods are always absent, and the crystals usually so. In nearly all cases the eye-specks contain more or less brightly coloured pigments.*

FIG. 1.



Sarsia × 3 times.

The question as to whether a nervous system is present in the Medusæ, is a question which has long occupied the more or less arduous labours of many naturalists. Hitherto, however, there has been so little agreement in their results, that Professor Huxley—himself one of the greatest authorities upon the group—thus defines the present standing of the question: "No nervous system has yet been discovered in any of these animals." † The cause of this uncertainty is to be found in the fact, that the transparent and deliquescent nature of the tissues of the Medusæ renders adequate microscopical observation in their case a matter of extreme difficulty; so much so, that, looking to the quantity and quality of the labour which has already been bestowed upon the question, I doubt whether the latter could ever have been satisfactorily settled by the histological methods alone. As, however, there is one very competent histologist who states that in the case of one particular genus of the Medusæ it is possible, with great care and peculiar treatment, to obtain microscopical evidence of the presence of nerves, I shall here give a brief epitome of his results. The observer is Prof. Haeckel, and he describes the nervous system of *Geryonia* as consisting of a continuous nerve-fibre running all round the margin of the swimming-bell, swelling out into a ganglion in the immediate vicinity of each eye-speck, and sending off a nerve-fibre to each of the four radial tubes. Thus, according to Haeckel, the course of the nervous system everywhere follows the course of the nutritive system, and forms a chain of ganglia all the way round the margin of the swimming-bell. It will immediately be seen in how striking a manner Prof. Haeckel's histological observations are confirmed by the physiological experiments which I will now proceed to describe.

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* It may not be superfluous to state that the Medusæ swim by means of an alternate contraction and relaxation of the entire bell. At each contraction water is ejected from the open mouth of the bell, and the consequent reaction propels the animal in the opposite direction.

† Classification of Animals, p. 22.

FUNDAMENTAL OBSERVATIONS.—In the case of all the naked-eyed Medusæ which I have been able to procure—viz. thirteen species belonging to six of the most divergent genera—it has been found by numberless experiments, that excision of the entire margin of a swimming-bell is invariably followed by immediate, total, and permanent paralysis of the entire organ. That is to say, if we take any naked-eyed Medusa which is swimming about in the astonishingly active and vigorous way that is characteristic of these animals, and if with a pair of scissors we cut off the extreme periphery of the swimming-bell, then all the swimming motions instantly cease, and are never again resumed. It would be impossible to imagine anything more decided than is this highly remarkable effect. Indeed, I do not know of any case in the whole animal kingdom where the removal of a centre of spontaneity is followed by so sudden and so complete a paralysis of the muscular system—even such gentle reflex twitchings as are observable in the muscles of vertebrated animals after removal of the brain not being here apparent.

So much, then, for the mutilated swimming-bell; it is completely and permanently paralyzed. But, on the other hand, the *margin* which has just been removed from the swimming-bell continues its rhythmical motions with a vigour and a pertinacity not in the least impaired by its severance from the main organism. For hours, and even for days, after the operation these motions persist; so that the contrast between the death-like quiescence of the mutilated swimming-bell, and the active contractions of the thread-like portion which has just been removed from its margin, is a contrast as striking as it is possible to conceive. Of course it does not signify how large or how small a part of the swimming-bell is left adhering to the severed margin; for whether this part be large or small, it continues to be animated by the margin. For instance, if in *Sarsia* (Fig. 1) the incision be carried through the equator of the animal, then all the upper half of the swimming-bell is paralyzed, while all the lower half continues its contractions as before; only, as the latter is now converted into an open tube, it is, of course, unable to progress.

Hence there can be no doubt that in the naked-eyed Medusæ all the spontaneity of the swimming-bell is lodged exclusively in the marginal rim. The question, however, arises, Is every part of the marginal rim equally endowed with the function of spontaneity, or is this function lodged only or chiefly in the marginal bodies? Now it is impossible to answer this question by direct experiment in the case of most of the species, from the fact that the marginal bodies are too numerous to admit of being cut out separately. In the case of *Sarsia*, however, the marginal bodies are only four in number; so that nothing can be easier than to try the differential experiment, of first cutting out all the four marginal bodies without injuring any of the intertentacular marginal tissue, and then, conversely, of cutting out all the intertentacular marginal tissue without injuring any of the marginal bodies. Well, I find by these experiments that in *Sarsia* (and so probably in all the naked-eyed Medusæ,) the *principal* ganglionic supply is seated in the marginal bodies; but that, nevertheless, it is not *exclusively* so. For whatever be the condition of the animal as to size, vigour, &c., before the operation, it endures removal of three of its eye-specks without suffering much apparent detriment; but immediately the fourth one is removed the animal falls to the bottom of the water perfectly motionless. If the specimen was not previously in a vigorous condition, it will perhaps never move again: it has been killed by nervous shock. But if the specimen was previously in a lively state, the period of quiescence will probably be but short—varying from a few seconds to half an hour, or more. Usually, however, the motions of the animal after the operation are conspicuously less vigorous than they were before it. Nevertheless, the fact that any motion at all takes place after removal of the marginal bodies alone, proves that some ganglia must be present in the intervening portions of the margin. Occasionally, indeed, it happens that the swimming-bell of *Sarsia* will continue its pulsations after all the margin has been removed, with the exception of a piece of the intertentacular tissue so small as to be invisible without the aid of a powerful lens.

So much, then, for the naked-eyed Medusæ. Turning now to the covered-eyed Medusæ, or those large sea-blubbers with which we are all acquainted, I find that removal of the margin is attended with results *analogous to*, but not *identical with*, the results which we have just seen to be so remarkable in the case of the naked-eyed Medusæ. For, in the case of the covered-eyed Medusæ, the paralysis, though usually complete for a time, is not always permanent. After periods varying from a few seconds to half an hour or more, occasional contractions begin to manifest themselves, or the contractions may even be resumed with but little apparent change in their character and frequency. But there are great differences in different species of covered-eyed Medusæ in these respects, and I have no time to enter into details. It must, therefore, be sufficient to say that, looking to the covered-eyed Medusæ *as a whole*, they differ from the naked-eyed Medusæ in that their ganglionic supply, although without question *principally* situated in the margin, is not *exclusively* so situated. It seems as though the ganglionic system, like the nutritive system, is more *diffused* in the one group than in the other.

Coming next to the question as to the relative value of the marginal bodies and of the intervening portions of the margin in respect of ganglionic function, I have a different answer to give in the case of the covered-eyed Medusæ from that which I have already given in the case of the naked-eyed Medusæ. For, in the covered-eyed Medusæ, all the remarkable paralyzing effects which follow upon cutting off the whole margin, follow equally well upon cutting out the marginal bodies alone; and any sized portion of contractile tissue left adhering to an excised marginal body will continue its rhythmical pulsations, while all other parts of the margin when detached at once cease to move. The differences, then, between the ganglionic system of the naked- and of the covered-eyed Medusæ may be roughly summed up thus:—The ganglionic system of a covered-eyed Medusa is usually more diffused than that of a naked-eyed Medusa, if we have regard to the *organism as a whole;* but it is less diffused if we have regard to the *margin alone.*

As the question concerning the presence of a nervous system in Medusæ has long been a warmly disputed one, I may here observe that there is no other instance in the whole animal kingdom of so great a disproportion between the mass of a ganglionic centre and that of the structure which it is capable of animating, as there is between the mass of a marginal body and that of the swimming-bell in the case of a large covered-eyed Medusa. Thus, in order to obtain an exact estimation in a good-sized jelly-fish weighing 30 lb., I cut out all the marginal bodies except one, and observed, as is always the case when this is done, that the single remaining ganglion continued to animate the entire swimming-bell. I then cut out this ganglion also, and thus paralyzed the bell. Lastly, I weighed the excised marginal body, and obtained the surprising result that it had been previously actively animating a structure 30,000,000 times its own weight!

STIMULATION.—All the tissues of all the Medusæ are keenly sensitive to all kinds of stimulation. When a swimming-bell, for instance, is paralyzed by excision of its margin, it invariably responds to a *single* stimulus by *once* performing that movement which it would have performed in response to that stimulus had it still been in an unmutilated state.

To mechanical stimulation the sensitiveness of the paralyzed bells is wonderfully great—a drop of water let fall from an inch in height upon the contractile tissue being sufficient, in some species, to elicit a responsive contraction.

Towards electrical stimulation the behaviour of the mutilated bells and of the severed margins differs in an important particular; for while the mutilated bells continue responsive to make and break of the constant current after they cease to be affected by strong induction shocks, or even by Faradaic electricity with the secondary coil pushed to zero, the reverse is true of the severed margins—these continuing responsive to induction shocks after they have ceased to be affected by make and break of the constant current (one Daniell's cell in all cases). This fact tends strongly to confirm the view that the specialized tissue of the margin is of a truly ganglionic nature.

By means of a Du Bois-Reymond induction apparatus and of needle-point terminals (the needles being passed through a small piece of cork as a support, and the cork being fixed to the stage-forceps on the mechanical stage of a Ross's microscope), I was able to investigate the distribution of excitable tracts in *Sarsia.* I found that there is an uninterrupted increase of excitability from the apex to the base of the swimming-bell, that the positions occupied by the radial tubes are tracts of comparatively high excitability, and that the eye-specks are the most excitable portions of the margin. Thus all my experiments, both in section and stimulation, confirm, in a very remarkable manner, the histological observations of Professor Haeckel.

Exhaustion of the excitable tissues may easily be shown by the ordinary methods. Exhausted tissue is much less sensitive to stimulation than is fresh tissue, and, so far as the eye can judge, the contractions are slower with the period of latent stimulation prolonged.

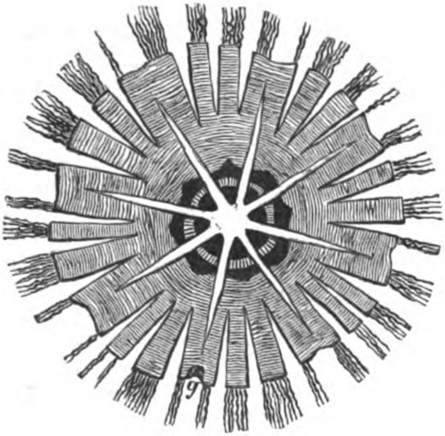
Tetanus produced by Faradaic electricity is not of the nature of an apparently prolonged contraction, but that of a number of contractions rapidly succeeding one another, as in a frog's heart under the influence of similar stimulation.

In their behaviour towards chemical stimuli, the excitable tissues of all the Medusæ conform in every respect to the rules which are followed by the nervo-muscular tissues of higher animals. Both the severed margins and the mutilated swimming organs, as well as severed tentacles and polypites, respond to applications of various acids, alkalis, solutions of various metallic salts, alcohol, ether, glycerine, &c. Fresh water is quickly fatal to the Medusæ.

SECTION.—Thus far, then, we have seen that the margin of a jelly-fish contains the ganglia or nerve-centres of the animal, and that after the removal of these ganglia the rest of the animal remains responsive to all the known modes of stimulation. The question now arises, How is the influence of the marginal ganglia distributed over the expanse of the muscular sheet? Are there anything resembling nerve-fibres leading from the ganglionic centres of the margin to all parts of the muscular sheet, or are the connections of a more primitive nature—the ganglionic impulse being discharged into the muscle-fibres that lie in the immediate vicinity of the ganglia, and these muscle-fibres propagating the impulse to the next adjacent muscle-fibres, and so on in the form of a wave of contraction, such as we see to occur in primitive protoplasm, where the living material has not as yet been differentiated into either muscle or nerve? This question is one of great interest both to the physiologist and to the evolutionist; but as I have no time to state all the issues which it involves, I will merely observe that, as the Medusæ are the first animals in the ascending scale of life where the presence of ganglia can be shown to occur, we have in them the opportunity of ascertaining whether ganglia appear upon the scene of life before nerve-fibres do, or whether the evolution of nerve-fibres proceeds *pari passu* with the evolution of ganglia. Now I fear it is impossible to answer the question before us by microscopical observation; for, if any general nerve-plexus is present in the Medusæ, its constituent fibres are too transparent to admit of being actually *seen*. There is, however, another method by which I hope to answer this question, and that is by section.

Aurelia aurita is a covered-eyed Medusa of a flattened form, and about the size of a soup-plate. It has eight marginal bodies, and when all these are removed the animal is usually as completely paralyzed as is a naked-eyed Medusa when deprived of its margin. If, however, only seven of the eight bodies be removed, the eighth one continues to animate the entire bell; and it may now be quite plainly seen that the contractions of the muscular sheet always originate in the immediate neighbourhood of the single remaining marginal body, and then spread out with the greatest rapidity until the whole muscular sheet takes part in the contraction. The appearance presented is that of two contractile waves which start at the same instant, one on each side of the marginal body, and which then course with equal rapidity in opposite directions, and so meet at the point of the flattened disc that is opposite to the marginal body. Now the question is, Do these contractile waves require the presence of nerve-fibres to convey them, or do they merely pass from one muscle-fibre to another? To answer this question I submitted these jelly-fish to various forms of section, some of which are here represented. If you imagine all the expanse of this muscular sheet to be pervaded by a nervous plexus, one would think that when it is cut as here represented, the conducting function of the nervous plexus would require to be destroyed. You see there is only one ganglion (*g*) left in its place, so that the contractile waves, when radiating from it, are obliged, if they pass at all, to zig-zag round and round the tops of these interdigitating cuts; and therefore a moment's thought will show how destructive such a mode of section must be to the continuity of anything at all resembling a nervous plexus. Yet the jelly-fish from which this drawing was taken continued to swim about after being cut in this way almost as well as it did before the operation.

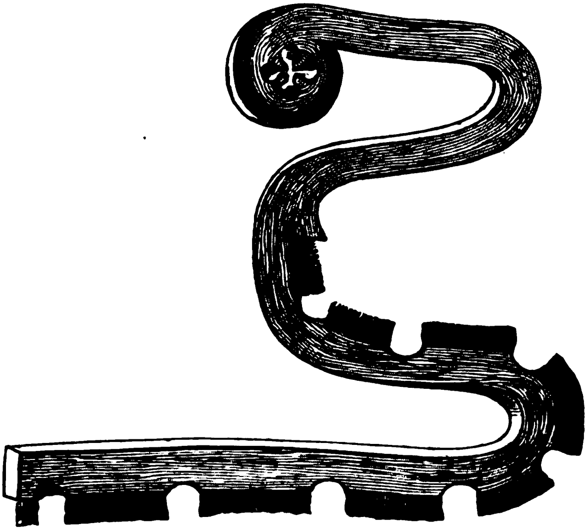
FIG. 2.



Even the *time* required for the contractile waves to pass from the ganglion to the opposite side of the disc was only very slightly prolonged by the sections.

Here, again, is another form of section. Seven marginal bodies having been removed as before, the eighth one was made the point of origin of a circumferential section, which was then carried round and round the disc in the form of a continuous spiral, the result, of course, being this long riband-shaped strip of tissue with the ganglion (*g*) at one end, and the remainder of the swimming-bell at the other. Well, as before, the contractile waves always originated at the ganglion; but now they had to course all the way along the strip until they arrived at its other extremity, and as each wave arrived at that extremity it delivered its influence into the remainder of the swimming-bell, which thereupon contracted. Hence, from this mode of section as from the last one, the deduction certainly appears to be that the passage of the contractile waves cannot be dependent on the presence of a nervous plexus; for nothing could well be imagined as more destructive of the continuity of such a plexus than this spiral mode of section must be.

Fig. 3.



Nevertheless, there is an important body of evidence to be adduced on the other side; but, as I can only wait to state a few of the chief points, I shall confine my observations to the spiral mode of section. First of all, then, I have invariably found it to be the case, that if this mode of section be carried on sufficiently far, a point is sooner or later sure to come at which the contractile waves cease to pass forward: they become blocked at that point. Moreover, the point at which such blocking of the waves takes place is extremely variable in different individuals of the same species. Sometimes the waves will become blocked when the strip is only an inch or less in length, while at other times they continue to pass freely from end to end of a strip that is only an inch broad and nearly a yard long; and between these two extremes there are all degrees of variation. Now if we suppose that the influence of the ganglion at the end of the strip is propagated as a mere muscle-wave along the strip, I cannot see why such a wave should ever become blocked at all, still less that the point at which it does become blocked should be so variable in different individuals of the same species. On the other hand, if we suppose the propagation of the ganglionic influence to be more or less dependent on the presence of a more or less integrated nerve-plexus, we encounter no difficulty; for on the general theory of evolution it is to be expected, that if such fibres are present in such lowly animals, they should not be constant as to position.

But there is a still stronger argument in favour of nerve-fibres, and it is this. At whatever point in a spiral strip which is being progressively elongated by section the blocking of the contractile wave takes place, such blocking is sure to take place completely and exclusively at that point. Now I cannot explain this invariable fact in any other way than by supposing that at that point the section has encountered a line of functionally differentiated tissue—has severed an incipient nerve.

On the whole, therefore, I provisionally adopt the supposition that all parts of the muscular sheet of the Medusæ are pervaded by a plexus of more or less rudimentary nerves, or "lines of discharge;" and I explain the fact of the tissues enduring the severe forms of section here represented without suffering loss of their physiological continuity, by supposing that all the rudimentary fibres composing the plexus are capable, in an extraordinarily high degree, of vicarious action.

There are a great many more things which I should like to say about these and other modes of section; but I must now direct your attention to another part of the inquiry. Various naturalists have speculated as to the possibility of the so-called eye-specks, or marginal bodies, of the Medusæ being incipient organs of vision. Such speculation, of course, was of little better value than a guess; for the marginal bodies of the Medusæ do not resemble any other form of visual apparatus with which we are acquainted. The guess, however, in this case happens to be correct.

Having placed several hundred *Sarsia* in a large bell-jar, and having completely shut out the daylight from the room in which the jar was standing, by means of a dark lantern and concentrating lens I then cast a beam of light through the water in which the *Sarsia* were swimming. The effect was most curious and interesting. From all parts of the jar the *Sarsia* crowded into the path of the beam, and were particularly numerous at the side of the jar nearest the light. Indeed, close against the glass at that side they formed an almost solid mass, which followed the light wherever it was moved. The animals moreover were very excited—dashing themselves against the glass very much as moths do under similar circumstances. There can thus be no doubt about the Medusæ being able to *see.* I therefore next took a dozen vigorous specimens, and placed them in another bell-jar by themselves. From nine of these I removed the marginal bodies, while the other three were left unmutilated. The three unmutilated specimens sought the light as before, but the nine which were deprived of their marginal bodies swam hither and thither without paying it any regard. I conclude, therefore, that they were blinded by the operation, and hence that the so-called eye-specks are, properly speaking, rudimentary organs of vision.

This fact I consider to be one of considerable interest, because, as already stated, the structure of the eye-speck does not resemble that of any other type of visual apparatus; so that it thus affords us another instance of the *plasticity* of nerve-organization—organization which is already known to be affected by light through such divers structures as are the eye of vertebrated animals, the compound eye of the higher articulated animals, and the ocellus of still lower types. And not only so, but in the Medusæ, where both nerves and sense-organs first appear upon the scene of life, the rudimentary sense-organs seem to revel, as it were, in undergoing in different species all sorts of modifications of structure. Another point of interest connected with these rudimentary sense-organs is, that the rays by which they are affected appear to be the same as those by which our own eyes are affected; for, on repeating the above-mentioned experiment with the non-luminous part of the spectrum, I found that the *Sarsia* did not appear to perceive the presence of the beam.

Poisons.—The influence of the nerve-poisons on the Medusæ constitutes an important branch of the inquiry on which I am engaged; for in them we have, as it were, a test whereby to ascertain whether nerve-tissue, where it can first be shown to occur in the animal kingdom, is of the same essential character as the nerve-tissue of higher animals.

Chloroform, ether, morphia, &c., all exert their anæsthesiating influence on the Medusæ quite as decidedly as they do on the higher animals. Soon after a few drops of the anæsthetic have been added to the water in which the Medusæ are contained, the swimming motions of the latter become progressively slower and feebler, until in a minute or two they cease altogether, the animals remaining at the bottom of the water apparently quite dead. No form or degree of stimulation will now elicit the slightest response; and this fact, it must be remembered, is quite as remarkable in the case of the Medusæ as in that of any other animal. Recovery in normal sea-water is exceedingly rapid, especially in the case of chloroform and ether.

The effects of strychnia may be best observed on a species called *Cyanæa capillata*, from the fact that, in water kept at a constant temperature, the ordinary swimming motions of this animal are as regular and sustained as the beating of a heart. But soon after the water has been poisoned with strychnia, unmistakable signs of irregularity in the swimming motions begin to show themselves. Gradually these signs of irregularity become more and more pronounced, until at last they develop into well-marked convulsions. The convulsions show themselves in the form of extreme deviations from the natural rhythm of this animal's motion. Instead of the heart-like regularity with which systole and diastole follow one another in the unpoisoned animal, we may now observe prolonged periods of violent contraction, amounting in fact to tonic spasm; and even when this spasm is momentarily relieved, the relaxation has no time to assert itself properly before another spasm supervenes. Moreover, these convulsions are very plainly of a *paroxysmal* nature; for after they have lasted from five to ten minutes, a short period of absolute repose comes on, during which the jelly-fish expands to its full dimensions, falls to the bottom of the water in which it is contained, and looks in every way like a dead animal. Very soon, however, another paroxysm sets in, and so on—prolonged periods of convulsion alternating with shorter periods of repose for several hours, until finally death puts an end to all these symptoms so characteristic of strychnine poisoning in the higher animals.

Curare, when administered in appropriate doses to the higher animals, has the remarkable effect of destroying the function of all the motor nerves in the body, while it leaves the function of the sensory nerves intact. My method of applying this poison to the Medusæ was very simple. Choosing a large and flat-shaped species of the naked-eyed group (viz. *Staurophora laciniata*), which always responds to stimulation with a peculiar spasm-like movement, I divided the specimen across its whole diameter, with the exception of a small piece of marginal tissue at one side, which I left to act as a connecting link between the two resulting halves. One of these halves I placed in normal sea-water, and the other in water poisoned with curare. The effects were most marked and beautiful. Previous to the administration of the poison both halves of the divided Medusa were, of course, contracting vigorously—the contractile waves now running from half A to half B, and now *vice versâ*. But after one of the halves had been sufficiently poisoned, all motion in it completely ceased, the unpoisoned half, however, continuing to contract as before. If the poisoned half were now irritated, by nipping with the forceps or otherwise, it did not itself move, but the other, or unpoisoned half, immediately responded to the stimulation by performing the highly distinctive spasmodic movement already referred to. This observation I consider to be one of the highest importance, because it shows that in those animals where we find the first indications of a nervous system, this system has already undergone a differentiation of function in its constituent parts, analogous to that which we observe in the sensory and motor tracts of higher animals.

Alcohol has the effect, in the first instance, of accelerating the frequency of the locomotor contractions, and also of impairing their regularity: the swimming motions are therefore of an excited and flurried character, very different from the rhythmical pulsations of the unpoisoned animal. Presently, however, a drowsiness begins to come on, and this increases more and more until finally the jelly-fish ceases its motions altogether, lying perfectly still in a state of stupid torpidity. In normal sea-water, however, this state of intoxication gradually passes away, and eventually the Medusa is swimming about again as soberly as ever.

It is interesting to observe, that if any of the narcotic poisons (including curare) be injected into one of the marginal bodies of a Medusa, that particular marginal body immediately ceases to perform its functions. Again, if any of these poisons be injected through one of the nutrient tubes which cross at right angles the contractile strip represented in Fig. 3 (the nutrient tubes are not represented), the contractile waves become blocked at the transverse line occupied by the injected tube, and this quite as sharply as if the contractile strip had been cut through at that line. Further, if any portion of the length of such a contractile strip be immersed in water containing any of the narcotic poisons, the contractile waves first have their rate diminished while travelling through the poisoned portion of the strip, and very shortly afterwards become blocked at the boundary of the poisoned water. On now transferring the poisoned portion of the strip to normal sea-water, the contractile waves again begin to force their passage—at first slowly and with difficulty, but every wave that passes renders the course more easy for its successor, until very soon the strip is again contracting from end to end at the same rate and with the same vigour as it did before it was submitted to the influence of the narcotic.

[G. J. R.]

ANNUAL MEETING,

Monday, May 1, 1876.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1875, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Stock sold out to rebuild and furnish the Laboratories has been more then replaced.

Fifty-six new Members were elected in 1875.

Sixty-one Lectures and Nineteen Evening Discourses were delivered in 1875.

The Books and Pamphlets presented in 1875 amounted to about 156 volumes, making, with those purchased by the Managers, a total of 323 volumes added to the Library in the year, exclusive of periodicals.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

Special thanks were voted to Dr. Spottiswoode, the Secretary, for his course of four lectures on Polarized Light in March and April last, for which he declined payment, esteeming the attention of his audience sufficient remuneration.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

President—The Duke of Northumberland, D.C.L.
Treasurer—George Busk, Esq. F.R.C.S. F.R.S.
Secretary—William Spottiswoode, Esq. M.A. LL.D. Treas.R.S.
Corresponding Member of Academy of Sciences, Paris.

Managers.

George Berkley, C.E.
William Bowman, Esq. F.R.S.
Sir Benjamin Collins Brodie, Bart. M.A. D.C.L. F.R.S.
Adm. Sir Henry John Codrington, K.C.B.
The Right Hon. John George Dodson, M.P.
Sir T. Frederick Elliot, K.C.M.G.
Lieut. Col. James Augustus Grant, C.B. C.S.I. F.R.S.
Cæsar H. Hawkins, Esq. F.R.S.
Joseph Dalton Hooker, C.B. M.D. D.C.L. Pres. R.S.
Alfred Latham, Esq.
Richard Liebreich, M.D. M.R.C.S.
The Lord Lindsay, M.P.
William Pole, Esq. M.A. F.R.S.
Sir W. Frederick Pollock, Bart. M.A.
C. William Siemens, Esq. D.C.L. F.R.S.

Visitors.

John R. Andrews, Esq.
Thomas Boycott, M.D. F.L.S.
John Charles Burgoyne, Esq.
Robert Pilkington Linton, Esq. F.R.C.S.
William Watkiss Lloyd, Esq.
William Longman, Esq. M.A.
Colonel William Pinney.
Wyndham Spencer Portal, Esq.
John Rae, M.D. LL.D.
The Rev. Arthur Rigg, M.A.
The Rev. William Rogers, M.A.
John Bell Sedgwick, Esq.
Basil Woodd Smith, Esq. F.R.A.S.
Benjamin Leigh Smith, Esq.
James Spedding, Esq.

A Silver Salver, with the following Inscription, was presented to Professor Tyndall by the Chairman:

"This Salver, together with the sum of Three Hundred Guineas, is presented to John Tyndall, F.R.S. Professor of Physics in the Royal Institution, upon his Marriage with Louisa Charlotte, daughter of Lord and Lady Claud Hamilton, on the 29th of February, 1876, in Westminster Abbey, by Members of the Royal Institution, to express their high esteem of his personal worth, and in grateful recognition of his eminent scientific services in its Laboratories and Lecture Theatre."

WEEKLY EVENING MEETING,

Friday, May 5, 1876.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor J. H. Gladstone, Ph.D. F.R.S.

Methods of Chemical Decomposition as illustrated by Water.

Among the most venerable of the Chinese classics is the 'Shoo King,' a collection of ancient historical records; and one of these records, the fourth book of Chow, contains a still more ancient document, "The Great Plan with its nine divisions," which purports to date from the early part of the Han dynasty,—according to Dr. Legge about 2000 B.C. This remarkable treatise bears on physical as well as ethical philosophy, and commences with an account of the five elements, viz. water, fire, wood, metal, and earth. The first element, water, is said to "soak and descend," and also to "become salt."

This seems to be the earliest known record of that doctrine of elements which spread widely over the ancient world. In the 'Institutes of Manu' we read of the elements also as five, but they are earth, fire, water, air, and ether; and according to the cosmogony of the Hindoo legislator, light or fire produced water, and water produced earth. There was, however, at least as late as two centuries ago, a sect in India who held it as a religious tenet that water was the prime or original element.

Similar opinions found their way to Europe. Thus Thales, of Miletus, who flourished in the sixth century B.C., taught that water was the origin of all things. The Greek philosophers generally adopted the theory of several elements, but reduced the number to four,—fire, air, earth, and water.

It is hard to say what was the precise meaning attached by the ancients to the term "element." It no doubt did not always convey the same idea. Water also, at least in the Aristotelian philosophy, was a generic expression for many bodies in a fluid condition, and signified not so much a special material substance as an inherent quality of things. Thus it was said to be cold and moist, and the opposite of fire which was hot and dry. In the philosophy of the middle ages we find the same views prevailing, and the early chemists still looked upon water in the same light. Thus Becker enumerated five elements, air, water, and inflammable, mercurial, and infusible earth; while Stahl adopted four,—water, acid, earth, and phlogiston. The ancient theory maintained its hold till the experimental philosophers at the latter part of the eighteenth century gave a definite meaning to the term element, and showed that water, air, and earth are compound bodies. Yet the idea of the elementary character of water was not easily abandoned.

In 1781 Cavendish found that when a mixture of what were then called "inflammable air" and "dephlogisticated air" is exploded by a spark in such proportions that the burnt air is almost entirely phlogisticated, pure water condenses on the sides of the vessel, and is equal in weight to the weight of the two airs. His theory was that water consists of "dephlogisticated air united to phlogiston," and that "inflammable air is water united to phlogiston." At the time of explosion, according to him, the excess of phlogiston was transferred from the inflammable to the dephlogisticated air, and thus both airs "turned into water." Cavendish also explained Priestley's production of inflammable air on heating iron filings strongly, by contending that the phlogiston of the iron united with the moisture from which they had not been freed. Lavoisier gave a different explanation of these phenomena. He held that "dephlogisticated air" is an elementary substance—oxygen—united with imponderable caloric, and that "inflammable" air, or hydrogen, is capable of taking the oxygen from the caloric, thus producing water and heat. "Water is not a simple substance, but is composed, weight for weight, of inflammable and vital air." Thus water was at length deposed from its rank as an element.

In the first year of this century, when the news of Volta's great discovery of the pile was made known in England, Messrs. Nicholson and Carlisle made various experiments with a series of halfcrowns, zinc plates, and pasteboard soaked in salt. Knowing that water conducted electricity, they inserted brass wires through corks at the two ends of a tube filled with water, which they are careful to tell us came from the New River. They were surprised to see a stream of minute bubbles rising from one pole while the other was corroded, and that this decomposition took place at each pole, though they were nearly 2 inches apart. They enlarged the distance, and found that 36 inches of water was too much for their force to traverse. Substituting flattened platinum for their brass wires, they found that the water was decomposed with the production of hydrogen at one end and oxygen at the other.

The old notion that water, by continuous boiling, was turned into stone had been previously dispelled by Lavoisier, but Davy found that some salts and earths remained behind when water was electrolyzed, and that when the experiment was conducted in two cells communicating with one another, the liquid in the one cell became acid, and in the other alkaline. He traced the origin of this in a masterly research, which formed the Bakerian lecture for 1806.[*] He found that the earthy substances were original impurities in the water, or came from the vessels employed; and using gold cones filled with distilled water, and united together by asbestos, he convinced himself that nitric acid was produced at the positive pole and ammonia at the negative. Suspecting that these were produced from the small quantities of nitrogen dissolved in the water combining with the liberated oxygen and hydrogen respectively, he took extraordinary precautions. Making use of water which he had carefully distilled in a silver still at 140° Fahr., and performing the experiment in vacuo, or rather in a space which he had twice filled with hydrogen and exhausted as thoroughly as the means at his disposal would permit, he then found that the water was decomposed without the least production of either acid or alkali. "It seems evident then," wrote Davy, "that water, chemically pure, is decomposed by electricity into gaseous matter alone, into oxygene and hydrogene."

In the following year Davy discovered the metals of the alkaline earths, potassium and sodium, and found that when these bodies are thrown upon water they decompose it, appropriating its oxygen and setting free its hydrogen. This is due to the superior chemical power or "affinity" of the alkaline metals.

In 1846 Mr., now Sir William, Grove observed that when steam was subjected to something like a white heat, small quantities of mixed oxygen and hydrogen gas were always produced.[†] It has since been shown that the gases are actually dissociated in one part of the flame of the oxy-hydrogen blowpipe, after their first combination.

It thus appears that there are three distinct ways in which water may be decomposed:—By an electric current;[‡] by some substance which has a superior attraction for one of its elements; or by heat alone.

It will readily be understood that the power of any one of these agents will be augmented by the co-operation of either of the others. Thus, the action of chemical affinity is usually augmented by heat; for instance, if a pellet of sodium be thrown upon cold water it melts, on account of the chemical action at once set up, but if upon boiling water it not only melts, but bursts into flame through the greater violence of the action. This is the reason why in Priestley's experiment iron at a red heat decomposed steam, though it will not do so at ordinary temperatures.

Similarly the electrolysis of water is much facilitated if there is some chemical affinity between the oxygen and the metallic conductors. It is generally said that it requires two cells to decompose water electrolytically. Now it is true that if platinum poles are employed there is no visible disengagement of gas when one cell only is used; but with zinc poles a single cell of Bunsen or Grove is amply sufficient. Zinc alone without the voltaic current is incapable of displacing the hydrogen in water; but it must be borne in mind that the tendency to combine with oxygen is a constant property of this metal, and is easily brought into activity by the co-operation of the feeble voltaic current. The increased effect upon electrolysis which is due to the nature of the poles is in proportion to the electromotive force of the different metals. For pure water the order is,—zinc, lead, iron, copper, silver, platinum, as tested by a galvanometer. This difference of result according to the nature of the metals employed in the electrolytic cell appears generally to have been overlooked, and it is the feeblest metal—platinum—which is usually employed for experimental purposes, doubtless because it is incapable of oxidation—the very reason of its feebleness.

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[*] 'Phil. Trans.' 1807, p. 1.

[†] *Ibid.* 1847, p. 1.

[‡] Though voltaic electricity alone is referred to in this discourse, it is well known that other forms of the same agent will effect chemical decompositions. Thus Professor Andrews has resolved pure water into its constituent gases by frictional electricity, and by that derived from the atmosphere.

When the other metals of the above list are used, not only does the positive pole oxidize, but the oxide, or rather hydrate, dissolves more or less in the pure water, and becomes itself an electrolyte. The consequence of this is that the positive electrode gradually wears away, while the metal is transferred to the negative electrode, and is deposited upon it in crystalline fringes or filaments. With silver these are particularly beautiful, as they assume arborescent forms, especially when able to spread over the surface of the containing vessel.

The temperature also of the liquid subjected to electrolysis has a great influence upon the result. Thus in an experiment where zinc poles and pure water were employed, the deflection of a galvanometer was found to increase about fourfold between 5° C. and 80° C., and the action augmented nearly *pari passu* with the temperature.

A similar result occurs, as might be expected, when two dissimilar metals, such as zinc and copper, are placed in cold water in connection with one another, and the water is heated. The deflection was found to double between about 30° and 80° C., but the difference for every 5° at the higher temperatures was several times greater than at the lower ones.

Another very important point in the electrolysis of water is to reduce to a minimum the very great resistance offered by the water itself. This is effected by bringing the electrodes as near to one another as possible: and for the same reason, if the force be generated by the action of two dissimilar metals upon water, they should be brought into the closest proximity.

A still more powerful means of decomposing water would evidently be a combination, not of two, but of all three agents, chemical affinity, heat, and voltaic force acting at an insensible distance. Thus zinc has a strong affinity for oxygen, but is unable of itself to displace the hydrogen of water: when united, however, with a more negative metal, such as copper, its power is enhanced to such a degree that a separation of the constituents does take place; but in the ordinary arrangement of a voltaic cell the action is so slight that no evolution of gas is perceptible. To produce a visible effect, the metals must not only be close together, but ought to touch one another at a myriad of points. This may be brought about by depositing the copper upon the zinc in a spongy condition; then the zinc will be oxidized, and bubbles of hydrogen will appear amongst the branches of the copper, even at the ordinary temperature, but the effect is greatly increased by the application of heat.

The arrangement just described is the "Copper Zinc Couple," which has been employed by Mr. Tribe and the speaker, and more recently by others, to effect a variety of chemical decompositions. Zinc foil is immersed in a solution of sulphate of copper until a black velvety deposit of the metal is produced; the soluble salts are then washed away, and the couple after being dried is ready to be placed in any liquid it is desired to decompose. Water was the first body experimented upon, and it was found that the action would go on as long as there was any metallic zinc left in union with the copper, the amount of hydrogen evolved gradually diminishing, though varying somewhat with the temperature of the day. The great influence exerted by heat is, however, better shown in the subjoined table, which gives the results of an experiment reduced to the unit of an hour's work.

At 2·2° C.	.. ..	1·1	c.c. of hydrogen produced.
22·2	.. ..	5·5	„
34·4	.. ..	13·9	„
55·0	.. ..	62·0	„
74·4	.. ..	174·6	„
93·0	.. ..	528·0	„

These figures strikingly exhibit the rapid acceleration of the action at the higher temperatures.

A greater effect may be produced by substituting for the copper a still more negative metal. Thus a zinc platinum couple acts with much greater energy upon water. Gold zinc couples, and many others also, have been tried, but gold has the practical disadvantage that the precipitated metal does not adhere well to the zinc. Aluminium alone does not decompose water, not even, according to Deville, at a red heat; but an aluminium copper couple decomposes it slowly, and an aluminium platinum couple more rapidly, even in the cold. One of the most recent discoveries is that aluminium when amalgamated with mercury is converted into hydrate even by the moisture of the air. The most powerful combination, however, might be expected to be that of the most positive and the most negative metal which can be conveniently brought together. These are magnesium and platinum; and in fact, if strips of magnesium foil be coated with finely divided platinum by immersing them in platinic chloride, and the resulting salts be washed away, a couple may be obtained which produces a most vigorous evolution of hydrogen when it is placed even in cold water.*

The decomposition of water by the copper zinc couple was of course a matter of little practical importance; it does, however, yield hydrogen in a state of purity, even though the zinc be largely contaminated with such a substance as arsenic—a fact which may prove of great consequence in medico-legal inquiries. These observations on water led to a long series of experiments on other bodies, especially organic compounds. The action of the two metals in conjunction frequently effects not only the splitting up of a compound, but a redistribution of its elements; and this has resulted not only in the discovery of a simple means of producing various substances previously known, but the formation of several others hitherto unknown. Thus the first trials were made on iodide of ethyl in the hope that Professor Frankland's beautiful process for making zinc ethyl might be simplified; and not only was a better result obtained in a shorter time, but when the experiment was performed in the presence of alcohol it was found that pure hydride of ethyl was given off, and a new substance, the iodoethylate of zinc, remained in the flask.

Among the bodies which may be prepared more easily or in greater purity by the copper zinc couple are the following:

Hydrogen.
Methyl hydride.
Ethyl hydride.
Propyl hydride.
Amyl hydride.
Olefiant gas.
Acetylene.
Propylene.
Diamyl.
Diallyl.
Zinc ethiodide.
Zinc ethyl.
Zinc amyl.

The substances that have been discovered by this agency are the following:

Zinc propiodide	$Zn(C_3H_7)I$.
,, propyl	$Zn(C_3H_7)_2$.
,, isopropyl	$Zn(C_3H_7)_2$.
,, ethylobromide	$Zn(C_2H_5)Br$.
,, iodoethylate	$Zn(C_2H_5O)I$.
,, bromethylate	$Zn(C_2H_5O)Br$.
,, chlorethylate	$Zn(C_2H_5O)Cl$.

Zinc propyl is a volatile liquid body, of specific gravity 1·098, which takes fire spontaneously in the air, burning with a bluish-white flame. The haloid ethylates are a new class of bodies which have been prepared from both ethyl iodide and iodoform, and their corresponding bromine and chlorine compounds.

The couple has also thrown some light upon the chemical structure of some of these organic bodies, as, for instance, by its different behaviour with the two isomeric bodies, chloride of ethylene and chloride of ethylidine. This is a direction in which future investigation is likely to be rewarded.[*]

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* Phenomena resulting from different metals in combination have frequently been observed by several experimenters, and some of them are described by Mr. W. N. Hartley in the 'Chemical News,' vol. xiv. p. 73; but it does not appear that the metals have ever been freed from concomitant salts, or their action understood or appreciated.

This method of quietly bringing about a chemical change has found a practical application in the hands of Professor Thorpe for determining the amount of nitrates in samples of water, a question of great importance which has hitherto been also one of great difficulty. The nitric acid is reduced by the couple to the condition of ammonia. In a similar way chlorates are reduced to chlorides.[†]

The progress of research by means of the copper zinc couple was interrupted by the discovery of a curious reaction, by which also water and other substances may be decomposed. Metallic aluminium does not attack water by itself, neither does iodine; but if the three are brought into contact, oxide of aluminium is formed and hydrogen gas is evolved; and not only this, but the solution so produced will cause the oxidation of any excess of aluminium with the formation of an equivalent amount of hydrogen. It is not even necessary that free iodine should be employed, for iodide of aluminium itself will determine the oxidation of any amount of metal. This action is greatly quickened by coupling platinum with the aluminium. By employing alcohol instead of water, a similar action is set up, and this has led to the discovery of aluminium ethylate, $Al_2(C_2H_5O)_6$, alcohol in which the replaceable hydrogen is substituted by aluminium. It is a solid body at the ordinary temperature, but easily melts, and is capable of being sublimed unchanged, its vapour burning with a luminous flame and white smoke of the oxide of the metal. Other compounds prepared by this singular reaction, and the nature of the chemical changes which occur, are at present the subject of study.[‡]

[J. H. G.]

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[*] Further particulars respecting the decomposition of water by this special kind of electrolysis may be found in 'Proc. Royal Soc.' 1872, p. 218; 'Report Brit. Assoc.' 1872, Abstracts, p. 75; 'Journal Chem. Soc.' 1873, p. 452; 'Phil. Mag.' 1875, pp. 284, 285. The account of "Researches on the Action of the Copper Zinc Couple on Organic Bodies" is given in the 'Journal Chem. Soc.' 1873, pp. 445, 678, 961; 1874, pp. 208, 406, 410, 615; 1875, p. 508. See also vol. vii. of these 'Proceedings,' p. 521.

[†] 'Journ. Chem. Soc.' 1873, p. 541.

[‡] Since this discourse was delivered, this peculiar reaction has been elucidated in a paper read before the Chemical Society, on "The Simultaneous Action of Iodine and Aluminium on Ether and Compound Ethers." An intermediate body, the aluminium iodoethylate, $Al_2(C_2H_5O)_3I_3$, is there described.

GENERAL MONTHLY MEETING,

Monday, May 8, 1876.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:

Sir T. Frederick Elliot, K.C.M.G.
Sir W. Frederick Pollock, Bart. M.A.
Dr. Joseph Hooker, C.B. D.C.L. Pres. R.S.
George Busk, Esq. F.R.S. Treasurer.
William Spottiswoode, Esq. LL.D. F.R.S. Secretary.

The Hon. George Elliot,
Ernest H. Goold, Esq. M.R.I.A.
Charles Rose Lucas, Esq.
Major T. Myles Sandys,
Edward W. Stanton, Esq. M.A.
were *elected* Members of the Royal Institution.

John Tyndall, Esq. D.C.L. LL.D. F.R.S.
was re-elected Professor of Natural Philosophy.

The special thanks of the Members were returned to Dr. Warren De la Rue, F.R.S. for his valuable present of a Thomson's Quadrant Electrometer.

The decease of Mr. Robert P. Linton, Visitor, through an accident, was announced, to the great regret of the Members.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Roma—Atti, Serie II. Vol. I. II. 4to. 1873–5.
Artom, Rev. B. Chief Rabbi of the Spanish and Portuguese Congregations of England—Sermons, First Series. 2nd ed. 16to. 1876.
Asiatic Society of Bengal—Journal, 1875, Part I. No. 4. 8vo.
Proceedings, 1875. No. 10. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVI. No. 5. 8vo. 1876.

Bavarian Academy of Sciences, Royal—Abhandlun, Band XII. Abthgen. 1. 4to. 1875.

L. A. Buchner, Festrede. 4to. 1875.

Almanach. 1875. 16to.

Bayley, Major J. A. (the Author)—The Assault of Delhi: a Vindication of H.M. 52nd Light Infantry. (L 16) 8vo. 1876.

British Architects, Royal Institute of—Sessional Papers, 1875–6, No. 9. 4to.

British Museum Trustees—Guide to Græco-Roman Sculptures. Part 2. 12mo. 1876.

Browning, George, Esq. (the Author)—The Edda Songs and Sagas of Iceland. (O 16) 16to. 1876.

Chemical Society—Journal for March, 1876. 8vo.

Cox, Serjeant E. W. M.R.I. (the Author)—The Mechanism of Man: an Answer to the Question "What am I?" Vol. I. 16to. 1876.

Editors—American Journal of Science for April, 1876. 8vo.

Athenæum for April, 1876. 4to.

Chemical News for April, 1876. 4to.

Electrical News for April, 1876.

Engineer for April, 1876. fol.

Journal for Applied Science for April, 1876. fol.

Nature for April, 1876. 4to.

Pharmaceutical Journal for April, 1876. 8vo.

Telegraph Journal for April, 1876. 8vo.

Frankland, Professor E. D.C.L. F.R.S. M.R.I.—Reports of the Rivers Commission, 1868. 2 vols. fol. 1870–4.

Franklin Institute—Journal, No. 604. 8vo. 1876.

Geological Survey of India—Records, Vol. VIII. Parts 1–4. 8vo. 1875.

Palæontologia Indica. Series IX. 2, 3. fol. 1875.

Hofmann, Professor A. W. F.R.S. (the Author)—Life-work of Liebig. (Faraday Lecture for 1875.) 8vo. 1876.

Mechanical Engineers' Institution—Proceedings, Jan. 1876. 8vo.

Medical and Chirurgical Society, Royal—Proceedings, Part 42. 8vo. 1876.

Meteorological Office—Physical Geography of the Atlantic, by Capt. Toynbee. 8vo. 1876.

Musée Teyler, Haarlem (the Directors)—Archives, Vol. IV. Fasc. 1. 8vo. 1876.

Photographic Society—Journal, No. 266. 8vo. 1876.

Physical Society—Proceedings, Part 3. 8vo. 1875.

Preussische Akademie der Wissenschaften—Monatsberichte: Jan. 1876. 8vo.

Royal Society of London—Proceedings, No. 168. 8vo. 1976.

Smith, Willoughby, Esq. (the Author)—Action of Light on Selenium. (K 101) 8vo. 1876.

Stewart, Charles, M.A. (the Author)—International Correspondence by means of Numbers. 12mo. 1874.

Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, April, 1876. 8vo.

Société Hollandaise des Sciences, Haarlem—Natuurkundige Verhandelingen. 3e Serie. Tome II. 5. 4to. 1875.

Archives Néerlandaises, Tome X. Liv. 5, 6, Tome XI. Liv. 1. 8vo. 1875–6.

Twining, Thomas, Esq. M.R.I. (the Author)—Science made Easy: a Series of Familiar Lectures. 4 parts. 4to. 1876.

United Service Institution, Royal—Journal, No. 85. 8vo. 1876.

Vereins zur Beförderung des Gewerbfleisses—Verhandlungen, Juli–Dec. 1875, Jan. Feb. 1876. 4to.

Woodward, Henry, Esq. F.R.S. (the Author)—Papers on Crustacea, &c. 1868–76.

Zoological Society—Transactions, Vol. IX. Parts 5, 6, 7. 4to. 1876.

Proceedings, 1875. Part 4. 8vo.

WEEKLY EVENING MEETING,

Friday, May 12, 1876.

Sir T. Frederick Elliot, K.C.M.G. Vice-President, in the Chair.

W. Froude, Esq. F.R.S.

The Fundamental Principles of the Resistance of Ships.

I propose to consider those principles of fluid motion which influence what is termed the "resistance" of ships. By the term resistance, I mean the opposing force which a ship experiences in its progress through the water. Considering how great an expenditure, whether of sail or steam power, is involved in overcoming this resistance, it is clearly most important that its causes should be correctly appreciated.

This subject is a branch of the general question of the forces which act on a body moving through a fluid, and has within a comparatively recent period been placed in an entirely new light by what is commonly called the theory of stream-lines.

This theory as a whole involves mathematics of the highest order, reaching alike beyond my ken and my purpose; but so far as we shall have to employ it here, in considering the question of the resistance of ships, its principles are perfectly simple and are easily understood without the help of technical mathematics; and I will endeavour to explain the course which I have myself found most conducive to its apprehension.

In order, however, to show you clearly what light the theory of stream-lines has thrown on the question, I must first describe the old method of treating it, which is certainly at first sight the most natural one, and we shall thus see what germs of truth that method contained, and how far these were developed into false conclusions.

It is a crude but instinctive idea, that the resistance experienced either by a ship, or by a submarine body such as a fish, moving through water, is due to the necessity of the body ploughing or forcing or cleaving a passage for itself through the water; that it has to drive the water out of its way and then to draw it in again after itself.

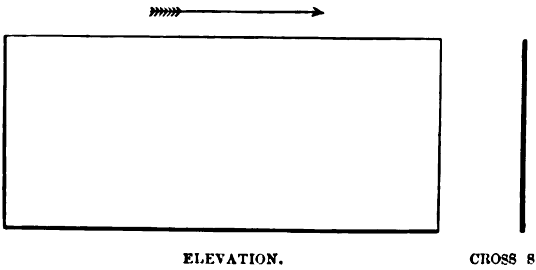
When, however, an attempt was made to deal with the matter in a scientific manner, it was seen that an explanation was needed of how it was that water required force to move it out of the way. For it may naturally be asked, How can there be reaction or resistance in a perfectly mobile material such as water seems to be? We can understand earth, for instance, resisting a ploughshare dragged through it, and we can understand that even a perfectly thin flat plane would make resistance if dragged edgeways through a sea of sand, or even through a sea of liquid mud, owing to the friction against its sides. But water appears, at first sight, altogether unlike this, and seems totally indifferent to change of form of any kind. If we stir water, the different currents seem to flow freely past one another, as if they would go on flowing almost for ever without stopping. But we find, that although to push a thin oar blade through the water edgeways seems to require no force, yet, if we push it flatways, as in rowing, it offers a considerable reaction. The distinction, then, which suggests itself is that the particles of water, although they offer no resistance to anything merely sliding past them, offer great resistance to anything pushing against them, because the thing which is pushing against them sets them in motion out of its way, and to set anything heavy in motion, requires the exertion of force to overcome what is called its inertia.

This, then, appears *prima facie* to be the characteristic of water, that to set the particles in motion, or what is the same thing, to divert them from a straight path, requires force to overcome their inertia, although, when once set in motion, they are able to glide freely past one another, or past a smooth surface. This supposition embodies the natural conception of a fluid, and if it were absolutely exemplified in water, then water would be what we should call a perfectly frictionless fluid.

Now, though water is not absolutely frictionless, yet it is true that in many of the more familiar ways of handling it, the forces developed by its slight frictional qualities are small compared to those due to its inertia, and it is therefore not surprising that those who theorised on the resistance of ships, thought it quite accurate enough to treat of the effect of the inertia only, and to neglect the comparatively small frictional qualities.

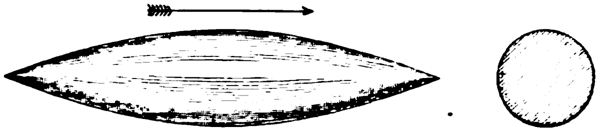
It was assumed then for the purposes of calculation, that the fluid being frictionless, would offer no resistance to a perfectly thin, flat, smooth plane, such as that shown in Fig. 1, moving edgeways through it, since this would in no way tend to set its particles in motion. But it is obvious that a ship, or fish, or other body, such as that

FIG. 1.



shown in Fig. 2, moving through the water, has to be continually setting the particles of water in motion, in order, first to get them out of its way, and afterwards to close them together again behind it, and that the inertia of the particles thus set in motion will supply forces reacting against the surface of the body. And it seemed certain, at first sight, that these reactions or forces on the surface of the body would necessarily so arrange themselves as to constitute resistance.

FIG. 2.



ELEVATION. CROSS SECTION.

On this view, various formulæ were constructed by mathematicians to estimate these reactions, and to count up the sum total of resistance which they would cause to a ship or moving body of any given form. These formulæ were not all alike, but they were mostly based on the supposition that the entire forward part of the body had to exert pressure to give the particles motion outwards, and that the entire afterpart had to exert suction to give them motion inwards, and that there was, in fact, what is termed *plus* pressure throughout the head-end of the body, and *minus* pressure or partial vacuum throughout the tail-end. And as it seemed that the number of particles which would have to be thus dealt with, would depend on the area of maximum cross section of the body, or area of ship's way as it was sometimes termed, the resistance was supposed to bear an essential proportion to the midship section of the ship. This idea has sometimes been emphatically embodied in the proposition that the work a ship has to do in performing a given voyage, is to excavate in the surface of the sea, from port to port, a canal the cross section of which is the same as the midship section of the ship.

This theory of resistance was at first sight natural and reasonable; it was generally admitted for many years to be the only practicable theory, and was embodied in all the most approved text-books on hydraulics and naval architecture. But when the theory of stream-lines was brought to bear upon the question, then it was discovered that the reactions, which the inertia of the fluid would cause against the surface of the body moving through it, and which were supposed to constitute the resistance, arranged themselves in a totally different manner from what had previously been supposed, and that, therefore, the old way of estimating their total effect upon the ship was fundamentally wrong. How wrong, I can best tell you by stating that according to the theory of stream-lines, a submerged body, such as a fish for example, moving at a steady speed through the assumed frictionless fluid, would experience no resistance at all. In fact, when once put into motion it would go on for ever without stopping.

The revelation, then, which was brought about by the application of the stream-line theory to the question, amounted to this, that the approved formulæ for estimating the resistance of bodies moving through water were not only wrong in detail, but that the supposed cause of resistance, with which alone they professed to be dealing, was in reality no cause at all; and that the real cause of resistance, whatever it might be, was entirely left out.

It is easy to imagine how fruitful, in false aims and false principles of nautical construction, would be the assignment of the resistance of ships to a supposed cause which has no existence at all. And the old theory, though now discarded by scientific men, has obtained such a hold on the minds of the general public, that I hope you will excuse my devoting considerable space to its refutation.

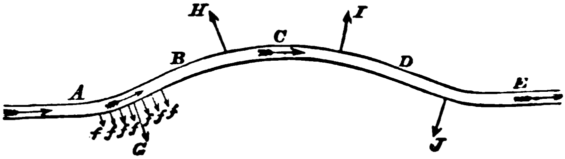
I will now briefly sketch an elementary view of the stream-line theory so far as it is relevant to our present purpose. Let it be understood that I am still dealing only with the supposed frictionless fluid; that for reasons which will hereafter appear, I am dealing not with a ship at the surface, but with a submerged body; and that I am supposing it to be travelling at a steady speed in a straight line. I am going to prove to you that under these circumstances the inertia of the fluid which has to be set in motion to make way for the body, will cause no resistance to it. Not that such inertia will cause no pressures and suctions acting upon the surface of the body; far from it; but that the pressures and suctions so caused must necessarily so arrange themselves, that the backward forces caused to the body on some parts of its surface, will be neutralised by the forward forces caused on other parts. In effect, although the inertia of the fluid resists certain portions of the body, it propels the other portions of the body with a precisely equal force.

In showing how this comes about, I prefer to substitute for the submerged body moving through a stationary ocean of fluid, the plainly equivalent conception of a stationary submerged body surrounded by a moving ocean of fluid. The proposition that such a body will experience no total endways push from the fluid flowing past it arises from a general principle of fluid motion, which I shall presently put before you in detail, namely, that to cause a frictionless fluid to change its condition of flow in any manner whatever, and ultimately to return to its original condition of flow, does not require, nay, does not admit of, the expenditure of any power; whether the fluid be caused to flow in a curved path, as it must do in order to get round a stationary body which stands in its way; or to flow with altered speed, as it must do in order to get through the local contraction of channel which the presence of the stationary body practically creates. Power, it may indeed be said, is being expended, and force exerted to communicate certain motions to the fluid; but that same power is also being given back, and the force counterbalanced, where the fluid is yielding up the motion which has been communicated to it, and is returning to its original condition.

In commencement, I will illustrate these two actions by considering the behaviour of fluid flowing through variously-shaped pipes; and I will begin with a very simple instance, which I will treat in some detail, and which will serve to show the nature of the argument I am about to submit to you.

Suppose a rigid pipe of uniform sectional area, of the form shown in Fig. 3, something like the form of the water-line of a vessel.

FIG. 3.



The portions AB, BC, CD, DE are supposed to be equal in length, and of the same curvature, the pipe terminating at E in exactly the same straight line in which it commenced at A, so that its figure is perfectly symmetric on either side of C, the middle point of its length.

Let us now assume that the pipe has a stream of frictionless fluid running through it from A towards E, and that the pipe is free to move bodily endways.

It is not unnatural to assume at first sight that the tendency of the fluid would be to push the pipe forward, in virtue of the opposing surfaces offered by the bends in it—that both the divergence between A and C from the original line at A, and the return between C and E to that line at E, would place parts of the interior surface of the pipe in some manner in opposition to the stream or flow, and that the flow thus obstructed would drive the pipe forward; if however we endeavour to build up these supposed causes in detail, we shall find the reasoning to be illusory, and I will now trace the results which can be established by correct reasoning.

The surface being assumed to be smooth, the fluid, being a frictionless fluid, can exercise no drag by friction on the side of the pipe in the direction of its length, and in fact can exercise no force on the side of the pipe, except at right angles to it. Now the fluid flowing round the curve from A to B will, no doubt, have to be deflected from its course, and its inertia, by what is commonly known as centrifugal action, will cause pressure against the outer side of the curve, and this with a determinable force. The magnitude and direction of this force at each portion of the curve of the pipe between A and B, are represented by the small arrows marked *f*; and the aggregate of these forces between A and B is represented by the larger arrow marked G. In the same way the forces acting on the parts BC, CD, and DE are indicated by the arrows H, I, and J · and as the conditions under which the fluid passes along each of the successive parts of the pipe, are precisely alike, it follows that the four forces are exactly equal, and, as shown by the arrows in the diagram, they exactly neutralise one another in virtue of their respective directions; and therefore the whole pipe from A to E, considered as a rigid single structure, is subject to no disturbing force by reason of the fluid running through it.

Though this conclusion that the pipe is not pushed endways, may appear on reflection so obvious as to have scarcely needed proof, I hope that it has not seemed needless, even though tedious, to follow somewhat in detail the forces that act, and which, under the assumed conditions, are the only forces that act, on a symmetrical pipe such as I have supposed.

Having shown that in the instance of this special symmetrically curved pipe, the flow of a frictionless fluid through it does not tend to push it endways, I will now proceed to show that this is also the case whatever may be the outline of the pipe, provided that its beginning and end are in the same straight line.

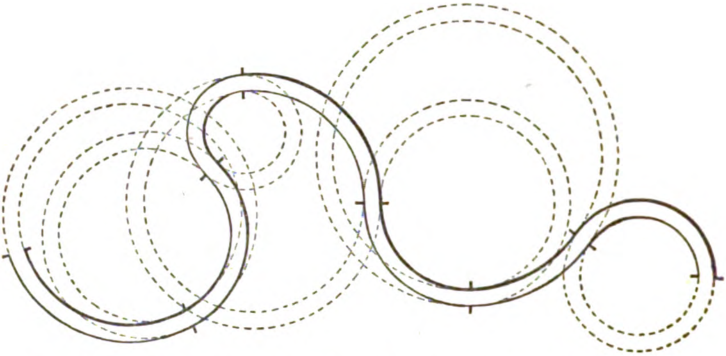
Assume a pipe bent into a complete circular ring with its ends joined, and the fluid within it running with velocity round the circle. The inertia of this fluid, by centrifugal force, exercises a uniform outward pressure on every part of the uniform curve; and this is the only force the fluid can exert. This outward pressure tends to enlarge or stretch the ring, and thus causes a uniform circumferential tension on each part of the ring.

Now take a ring of twice the diameter and suppose the fluid to be running round it with the same linear velocity as before. The diameter of the curve being doubled, and the speed being the same, the outward pressure due to centrifugal force on each linear inch of the ring will be halved; but since the diameter is doubled, the number of linear inches in the circumference of the ring will be doubled. Since, then, we have twice the number of inches acting, each with half the force, the total force tending to enlarge the ring will be unaltered, and the circumferential tension on the ring caused by the centrifugal force of the fluid, will be just the same as before.

In the same way we can prove that in any number of rings of any diameters, if the linear velocity of the fluid in each is the same, the circumferential tension caused by the centrifugal force of the fluid will also be the same in each.

Now let us take each of these rings and cut out a piece, and then join all these pieces together so as to form a continuous pipe, as in Fig. 4, and suppose the stream of fluid flowing through the combined pipe, with the same linear velocity as that with which it was before flowing round each of the rings. The fluid in each of the segments will now be in precisely the same condition as when the segment formed part of a complete ring, and will subject each piece of ring to the same strains as before, namely, to a longitudinal tension or strain, and to that only. And since we have already seen that the tension is the same in amount in each ring, the tension will be the same at every point in the combined pipe.

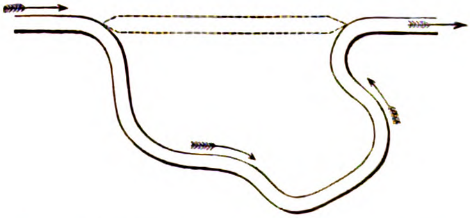
FIG. 4.



This being so, if we imagine the pipe to be flexible (but not elastic), and to be fastened at the ends, the pipe, although flexible, will not tend to be disturbed in its shape by the inertia of the fluid which is running through it; because the fluid does not cause any lateral force, but only a longitudinal stretching force, and that, the same in amount at every point. And this will clearly be so in a pipe of any outline, because any curve may be made up by thus piecing together short bits of circular arcs of appropriate radii.

Let us then take a flexible pipe having the two ends in the same straight line, but pointing away from one another, as in Fig. 5,

FIG. 5.



the intermediate part being of any outline you please. If the ends are fixed we have seen that the flow of fluid will not tend to disturb the pipe, and therefore all that will be necessary to hold it in its position, will be an equal and opposite tension supplied by the anchorages at the ends, to prevent the ends being forced towards one another. And if, instead of anchoring the ends, we put a strut between them to keep them apart, the pipe thus fitted will require no external force to keep it in position. In other words, whatever be the outline of a pipe, provided its beginning and end are in the same straight line, a frictionless fluid flowing through it, will have no tendency to push it bodily endways.

So far I have dealt only with pipes having uniform sectional area throughout their length, an assumption which has been necessary to the treatment pursued, as the velocity has in each case been assumed to be uniform throughout the length of the pipe. I will now proceed to consider the behaviour of fluid flowing through pipes of varying sectional area, and consequently flowing with varying velocity.

It is, I think, a very common impression, that a fluid in a pipe, meeting a contraction of diameter (see Fig. 6), exercises an excess of pressure against the entire converging surface which it meets, and that, conversely, as it enters an enlargement (see Fig. 7), a relief of pressure is experienced by the entire diverging surface of the pipe. Further, it is commonly thought that there is in the narrow neck of a contracted passage (see Fig. 8) an excess of pressure due to the squeezing together of the fluid at that point.

Fig. 6.

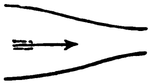


Fig. 7.

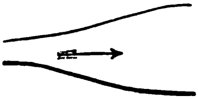
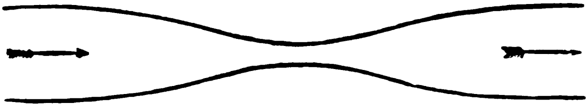


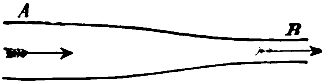
Fig. 8.



These impressions are in every respect erroneous; the pressure at the smallest part of the pipe is, in fact, less than that at any other point, and *vice versâ*.

If a fluid be flowing along a pipe A B which has a contraction in it (see Fig. 9), the forward velocity of the fluid at B must be greater than that at A, in the proportion in which the sectional area of the pipe at B is less than that at A; and, therefore, while passing from A to B the forward velocity of the fluid is being increased. This increase of velocity implies the existence of a force acting in the direction of the motion, to overcome the inertia of the fluid; that is to say, each particle which is receiving an increase of forward velocity must have a greater fluid pressure behind it than in front of it; for no other condition will cause that increase of forward velocity. Hence a particle of fluid, at each stage of its progress along the tapering contraction, is passing from a region of higher pressure to a region of lower pressure, so that there must be a greater pressure in the larger part of the pipe than in the smaller, the diminution of pressure at

Fig. 9.



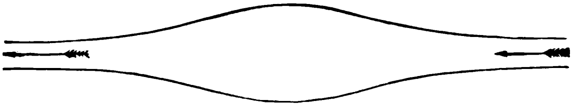
each point corresponding with the diminution of sectional area, corresponding, that is to say, with the additional forward velocity assumed by the fluid at each point of its advance along the contraction. Consequently, differences of pressure at different points in the pipe depend solely upon the velocities, or, in other words, on the relative sectional areas of the pipe, at those points.

It is easy to apply the same line of reasoning to the converse case of an enlargement. Here the velocity of the particles is being reduced through precisely the same series of changes, but in an opposite order. The fluid in the larger part of the pipe moves more slowly than that in the smaller, so that, as it advances along the enlargement, its forward velocity is being checked; and this check implies the existence of a force acting in a direction opposite to the motion of the fluid, so that each particle which is being thus retarded, must have a greater fluid pressure in front of it than behind it; thus a particle of fluid at each stage of its progress along a tapering enlargement of a pipe, is passing from a region of lower pressure to a region of higher pressure, the change of pressure corresponding to the change of velocity required. Hence we see that a given change of sectional area will require the same change of pressure, whether the pipe be an enlargement or a contraction.

Therefore, in a pipe in which there is a contraction and a subsequent enlargement to the same diameter as before (see Fig. 8), since the differences of pressure at different points depend on the differences of sectional area at those points, by a law which is exactly the same in an enlarging as in a contracting pipe, the points which have the same sectional areas will have the same pressures, the pressures at the larger areas being larger, and those at the smaller areas smaller.

Precisely the same result will follow in the case of an enlargement followed by a contraction (see Fig. 10).

FIG. 10.



Were water a frictionless fluid these propositions could be exactly verified by experiment as follows.

Figs. 11 and 12 show certain pipes, the one a contraction followed by an enlargement, the other an enlargement followed by a contraction. At certain points in each pipe there are small holes, communicating with vertical gauge-glasses. The height at which the fluid stands in each of these vertical glasses, of course indicates the pressure in the pipe at the point of attachment.

In Fig. 11 the sectional areas at E and P are equal to one another. Those at C and K are likewise equal to one another, but are smaller than those at E and P. The area at I is the smallest of all. Now, the fluid being frictionless, the pressures at E and P indicated by the heights E D and P Q would be equal, these being greater than C H and KN. CH and KN would also be equal to one another, and would be themselves greater than IJ.

FIG. 11.

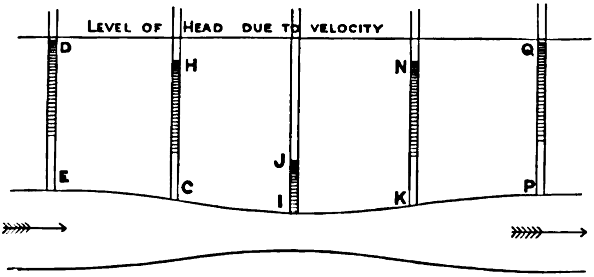
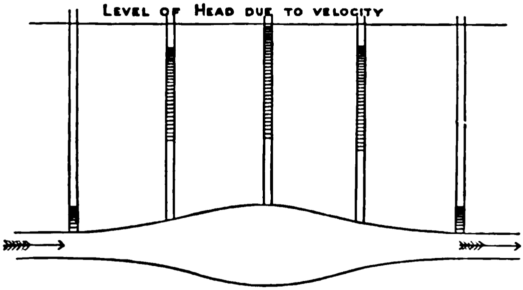

LEVEL OF HEAD DUE TO VELOCITY
D
H
N
Q
E
C
J
I
K
P

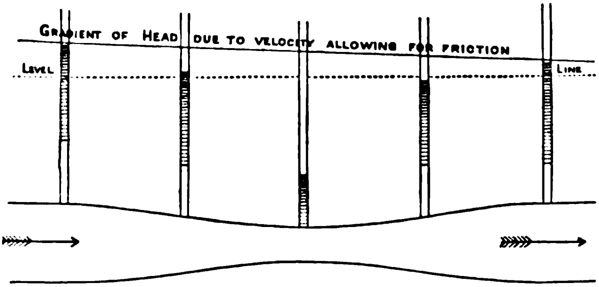
FIG. 12.


LEVEL OF HEAD DUE TO VELOCITY

The results shown in Fig. 12 are similar in kind, equal pressures corresponding to equal sectional areas.

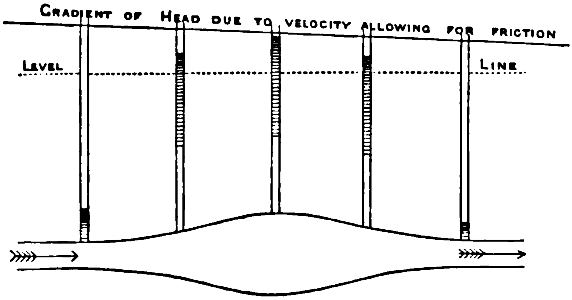
But if the experiment were tried with water, some of the pressure at each successive point would be lost in friction, and this growing defect in pressure, or "gradient," would be indicated in the successive gauge-glasses in the manner shown in Figs. 13 and 14.

FIG. 13.


GRADIENT OF HEAD DUE TO VELOCITY ALLOWING FOR FRICTION
LEVEL
LINE

I have here arranged an experiment which conveniently illustrates those propositions, making allowance for the frictional gradient.

FIG. 14.



h k l e f g a b c (see Fig. 15) is a continuous series of glass tubes, through which water is flowing from the cistern *n* to the outlet *m*. The cistern is kept full to a certain level. The tube from *h* to *l* is what I have called an enlargement followed by a contraction (like Fig. 10); from *e* to *g*, the diameter is the same throughout; and from *a* to *b*, the tube is a contraction followed by an enlargement (like Fig. 8). Just as in Figs. 11, 12, 13, 14, gauge-glasses are here fitted to the various tubes, to show the pressures of the water in them at various points.

Let us first consider the parallel pipe *e g*. If the fluid were frictionless, the diameter being uniform, the pressure would be uniform throughout, and the fluid would stand at the same level in each of the three gauge-glasses. But, owing to the friction, the water surfaces in the three glasses do not come up to a level line, but form a descending line, namely the frictional gradient.

Now take the pipe *a c*. Here the smallest pressure denoted by the water level at *b′*, is in the middle at *b*, where the diameter is smallest, and the greatest pressure denoted by the water levels at *a′*, *c′*, is at the two ends *a*, *c*, where the diameter is greatest. And if the fluid were frictionless, the pressure at the two ends, which have the same diameter, would be the same, but with water there is, as in the parallel pipe *e g*, a gradient or loss of pressure due to the friction.

The frictional gradient, according to well-known hydraulic rules, has a definite law of variation in terms of diameter and velocity, consequently it has been possible by calculation to so arrange the diameters of the pipes, that the parallel pipe *e g* should, according to the rule, have the same frictional gradient as the pipe *a c*, and as we see that the gradients are in fact the same, the result not merely illustrates but verifies the propositions.

In the pipe *h k l* we have the smallest diameter at the two ends

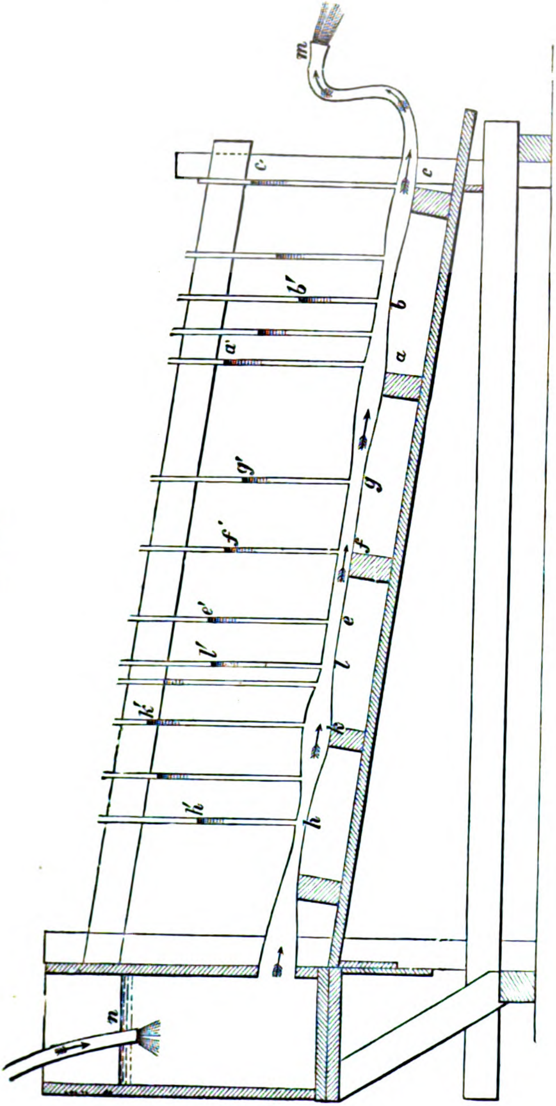


FIG. 15.

h' and *l*, and the largest diameter at the middle point *k*, and consequently we have the smallest pressures denoted by the water levels at *h'* and *l'*, at the two ends, and the greatest pressure in the middle denoted by the water level at k', and we again have the fall or gradient from end to end due to friction.

Fig. 16.

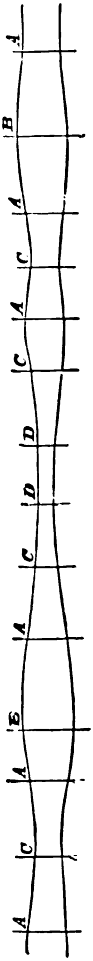
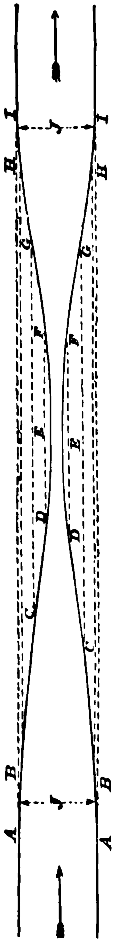


Fig. 17.



These experiments afford a good verification of the proposition which I just now explained, namely, that in a frictionless fluid flowing through a pipe of varying diameter, the pressure at each point depends on the sectional area at that point, there being equal pressures at the points of equal sectional area. Hence if in the pipe shown in Fig. 16 the areas at all the points marked A are equal, if also the areas at all the points marked B are equal, and so also with those at C and D, then the pressures at all the points A will be the same, the pressures at all the points B will be the same, and so with those at C and D.

Since, then, the pressure at each point depends on the sectional area at that point and on that only, it is easy to show that the variations in pressure due to the flow are not such as can cause any total endways force on the pipe, provided its sectional area at each end is the same.

Take for instance the pipe shown in Fig. 17. The conical portion of pipe A B presents the same area of surface effective for endways pressure as does the conical portion H I, only in opposite directions. They are both subject to the same pressure, being that appropriate to their effective mean diameter J. Consequently the endways pressures on these portions are equal and opposite, and neutralise one another. Precisely in the same way it may be seen that the endways pressures on B C, C D, D E, exactly counteract those on G H, F G, E F; and it may be similarly shown, that in any combination whatever of enlargements and contractions, provided the sectional area and direction of the pipe at the two ends are the same, the total endways force impressed on the pipe by the fluid flowing through it must be *nil.*

We see then that a frictionless fluid flowing through a pipe of any form, whether tortuous or of varying diameter, will not tend to push it endways, as long as the two ends of the pipe are in the same straight line, and have the same sectional area; in a word, as long as the speed and direction of flow of the fluid are the same in leaving the pipe as in entering it; and in this compound proposition concerning the flow of fluid through pipes, I have laid the necessary foundation for the treatment of the case of the flow of an ocean of frictionless fluid past a submerged body.

I have dealt with the instance of a single stream of uniform sectional area (and therefore of uniform velocity of flow) enclosed in a pipe of any outline whatever, and I have dealt with the instance of a single stream of varying sectional area and velocity of flow; and in both these cases I have shown that, provided the streams or pipe-contents finally return to their original direction and velocity of flow, they administer no total endways force to the pipe or channel which causes their deviations.

I am now going to deal with a combination of such streams, each to some extent curved and to some extent varying in sectional area, which, when taken together, constitute an ocean of fluid, flowing steadily past a stationary submerged body, see Fig. 18; and here also, since the combination of curved streams surrounding the body, which together constitute the ocean flowing past it, return finally to their original direction and velocity, they cannot administer to the body any endways force.

Every particle of the fluid composing this ocean, as it passes the body, must undoubtedly follow some path or other, though we may not be able to find out what path; and every particle so passing is preceded and followed by a continuous stream of particles all following the same path, whatever that may be. We may then, in imagination, divide the ocean into streams of any size and of any cross section we please, provided they fit into one another so as to occupy the whole space, and provided the boundaries which separate the streams exactly follow the natural courses of the particles.

If we trace the streams to a sufficient distance ahead of the body, we shall there find the ocean flowing steadily on, completely undisturbed by, and, so to speak, ignorant of the existence of the body which it will ultimately have to pass. There, all the streams must have the same direction, the same velocity of flow, and the same pressure. Again, if we pursue their course backwards to a sufficient distance behind the body, we shall find them all again flowing in their original direction; they will also have all resumed their original velocity; for otherwise, since the velocity of the ocean as a whole cannot have changed, we should have a number of straight and parallel streams having different velocities side by side with one another. This, in a frictionless fluid, would be clearly an impossible state of things, for we have seen that in a frictionless fluid the velocities exactly correspond with the pressures, so that if the velocities of these streams were different the pressures would be different, and if the pressures were different the fluid would begin to flow from the greater pressures towards the less, and the streams would thus become curved instead of straight.

Thus, although in order to get past the body these streams follow some courses or other, various both in direction and velocity, settling themselves into these courses in virtue of the various reactions which they exert upon one another and upon the surface of the body, yet ultimately, and through the reverse operation of corresponding forces, they settle themselves into their original direction and original velocity. Now the sole cause of the original departure of each and all of these streams from, and of their ultimate return to, their original direction and velocity, is the submerged stationary body; consequently the body must receive the sum total of the forces necessary to thus affect the streams. Conversely this sum total of force is the only force which the passage of the fluid is capable of administering to the body. But we know that to cause a single stream, and therefore also to cause any combination or system of streams, to follow any courses changing at various points both in direction and velocity, requires the application of forces the sum total of which in a longitudinal direction is *nil*, provided that the end of each stream has the same direction and velocity as the beginning. Therefore the sum total of the forces (in other words the only force) brought to bear upon the body by the motion of the fluid in the direction of its flow, is *nil*.

Another instructive way of regarding the same problem is this. Suppose each and every one of the streams into which we have subdivided the ocean, to be enclosed in an imaginary rigid pipe made exactly to fit it, throughout, the skin of each pipe having no thickness whatever. The innermost skin of the innermost layer of pipes (I mean that layer which is in contact with the side of the body), the innermost skin, I say, of this layer is practically neither more nor less than the skin or surface of the body. The other parts of the skins of this layer, and all the skins of all the other pipes, simply separate fluid from fluid, which fluid *ex hypothesi* would be flowing exactly as it does flow if the skins of the pipes were not there; so that, in fact, if the skins were perforated, the fluid would nowhere tend to flow through the holes. Under these circumstances the flow of the fluid clearly cannot bring any force to bear on any of the skins of any of the pipes, except on the innermost skin of the innermost layer. Now we know that the fluid flowing through this system of pipes administers no total endways force to any one of the pipes or to the system as a whole. But it produces, as we have just seen, no force whatever upon any of the skins which separate fluid from fluid ; consequently, if these are removed altogether, the force administered to the remainder of the system, will be the same as is administered to the whole system, namely, no total endways force whatever. But what is this remainder of the system which has no total endways force upon it? Simply the surface of the body, which is formed, as I have already said, by the innermost skins of the innermost layer of pipes. Therefore no total endways force is administered to the body by the flow of the fluid.

I have now shown that an infinite ocean of frictionless fluid flowing past a stationary submerged body cannot administer to it any endways force, whatever be the nature of the consequent deviations of the streams of fluid. The question, what will be in any given case the precise configuration of those deviations, is irrelevant to the proof I have given of this proposition. Nevertheless it is interesting to know something at least, of the general character which these deviations, or "stream-lines," assume in simple cases; therefore I show some in Figs. 18 and 19, which are drawn according to the method explained by the late Professor Rankine.

The longitudinal lines represent paths along which particles flow; they may therefore be regarded as boundaries of the streams into which we imagined the ocean to be divided.

We see that, as the streams approach the body, their first act is to broaden, and consequently to lose velocity, and therefore, as we know, to increase in pressure. Presently they begin to narrow, and therefore quicken, and diminish in pressure, until they pass the middle of the body, by which time they have become narrower than in their original undisturbed condition, and consequently have a greater velocity and less pressure than the undisturbed fluid. After passing the middle they broaden again until they become broader than in their original condition, and therefore have less velocity and greater pressure than the undisturbed fluid. Finally, as they recede from the body they narrow again until they ultimately resume their original dimension, velocity, and pressure. Thus, taking the pressure of the surrounding undisturbed fluid as a standard, we have an excess of pressure at both the head and stern ends of the body, and a defect of pressure along the middle.

We proved just now that, taken as a whole, the pressures due to the inertia of the fluid could exert no endways push upon the stationary body. We now see something of the way in which the separate pressures act, and that they do not, as seems at first sight natural to expect, tend all in the direction in which the fluid is flowing; on the contrary, pressure is opposed to pressure, and suction to suction, and the forces neutralise one another and come to nothing; and thus it is that an ocean of frictionless fluid, flowing at steady speed past a stationary submerged body, does not tend to push it in the direction of the flow. This being so, a submerged body travelling at a steady speed through a stationary ocean of frictionless fluid will experience no resistance.

Since then a frictionless fluid would offer no resistance to a submerged body moving through it, we have next to consider what are the real causes of the resistance which such a body experiences when moving through water.

The difference between the behaviour of water, and that of the frictionless fluid is twofold, as follows:

First, the particles of water, unlike those of a frictionless fluid, exert a drag or frictional resistance upon the surface of the body as they glide along it. This action is commonly called surface-friction or

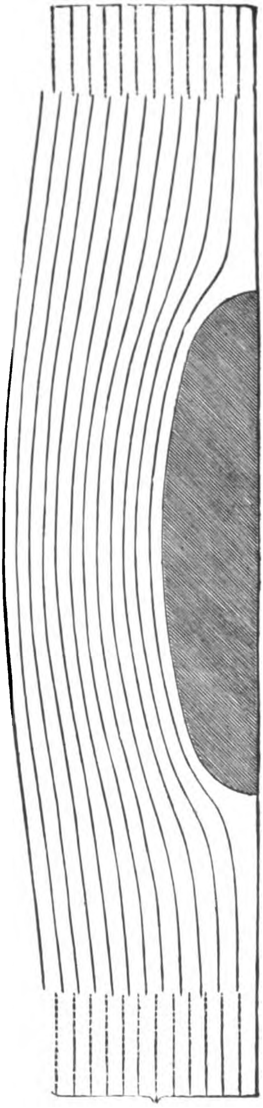


FIG. 18.

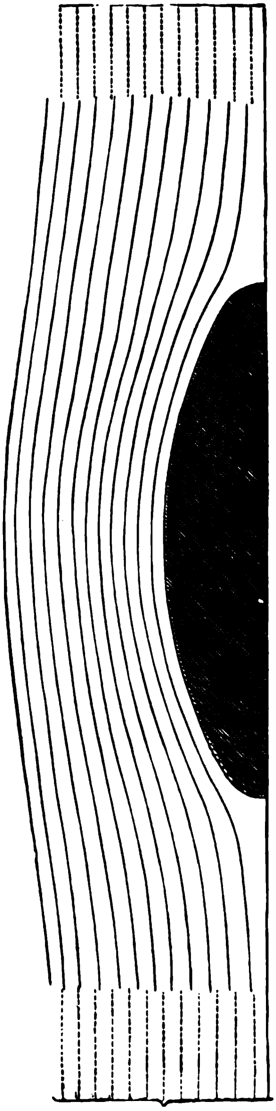


FIG. 19.

skin-friction, and its amount in any given case can be calculated from general experimental data. The resistance due to the surface-friction of a body such as that which we have been considering is practically the same as that of a plane surface of the same length and area, moving at the same speed edgeways through the water.

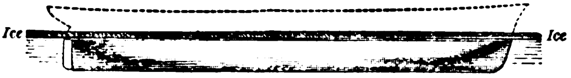
The second difference between the behaviour of water, and that of the imaginary frictionless fluid surrounding the moving submerged body, is that the mutual frictional resistance experienced by the particles of water in moving past one another, somewhat hinders the necessary stream-line motions, alters their nice adjustment of pressures and velocities, defeats the balance of forward and backward forces acting against the surface of the body, and thus induces resistance. This action, however, seems imperceptible in forms of fairly easy shape such as that shown in Fig. 2, and only operates tangibly where there are angular features, or very blunt sterns, like the blunt round tail, for instance, of the bodies shown in Figs. 18 and 19. In such a case, the stream-lines, instead of closing in round the stern, as shown in the figures, form a swirl or eddy, from which it results that the excess of pressure which would exist at the tail-end in a frictionless fluid, and which would there counterbalance the similar excess of pressure at the nose of the body, becomes in water greatly reduced, and in part converted into negative pressure, and thus a very great resistance may result. It is worth mentioning, however, that it is blunt tails rather than blunt noses that cause these eddies, and thus a body with one end round and the other sharp, no doubt experiences least resistance when going with the round end first.

I call this source of resistance "eddy-making resistance," and as I have said, it will be imperceptible in forms of fairly easy shape, such, for example, as Fig. 2. Such a form of submerged body will experience practically no resistance except that due to surface-friction, and will therefore experience practically only the same total resistance as a thin plane, like Fig. 1, moving edgeways, which possesses the same area of wetted skin. In fact, we may say generally, that all submerged bodies of fairly fine lines, experience no resistance except surface-friction.

I have hitherto, throughout the whole of this reasoning, been dealing with submerged bodies only, by which I mean bodies travelling at a great depth below the surface of the fluid; and I have shown the sole causes of their resistance to be the two I have termed respectively surface-friction and eddy-making resistance. But when we come to the case of a ship, or any other body travelling at or indeed near the surface, we find a new cause of resistance introduced; a cause, the consideration of which is often of most vital importance in the design of the forms of ships, and which renders the question of the form of least resistance for a ship, entirely different from that of the form of least resistance for a submerged body. This new cause of resistance, like the eddy-making resistance, operates by altering the stream-line motions and defeating their balance of forward and backward forces. It arises as follows:

Imagine a ship travelling at the surface of the water, and first let us suppose the surface of the water to be covered with a sheet of rigid ice, and the ship cut off level with her water-line, so as to travel beneath the ice, floating, however, exactly in the same position as before (see Fig. 20). As the ship travels along, the stream-line motions will be the same as for a submerged body, of which the ship may be regarded as the lower half; and the ship will move without resistance, except that due to the two causes I have just spoken of, namely surface-friction and eddy-making resistance. The stream-line motions being the same in character as those we have been considering, we shall still have at each end an excess of pressure, and along the sides a defect of pressure, which will tend the one to force up the sheet of ice and the other to suck it down. If now we remove the ice, the water will obviously rise in level at each end, in order that excess of hydrostatic head may afford the necessary reaction against the excess of pressure, and the water will sink by the sides, in order that defect of hydrostatic head may afford reaction against the defect of pressure.

FIG. 20.



The hills and valleys which thus commence to be formed in the water are, in a sense, waves, and though originating in the stream-line forces of the body, yet when originated, they come under the dominion of the ordinary laws of wave-motion, and to a large extent behave as independent waves; and in virtue of their independent action they modify the stream-line forces which originated them, and alter the pressures which are acting upon the surface of the ship.

The exact nature of this alteration of pressure, in any given case, we have no means of predicting; but we can be quite sure it must operate to alter the balance of forward and backward forces in such a way as to cause resistance; for we see that the final upshot of all the different actions which take place is this—that the ship in its passage along the surface of the water has to be continually supplying the waste of an attendant system of waves, which, from the nature of their constitution as independent waves, are continually diffusing and transmitting themselves into the surrounding water, or, where they form what is called broken water, crumbling away into froth. Now, waves represent energy, or work done, and therefore all the energy represented by the waves wasted from the system attending the ship, is so much work done by the propellers or tow-ropes which are urging the ship. So much wave-energy wasted per mile of travel is so much work done per mile, and so much work done per mile is so much resistance.

The surface of the water thus admits of an escape as it were, of the pressures which arise from the inertia of the particles of the fluid which have to be set in motion by the body. But so far from thereby rendering less obstruction to the passage of the body, these pressures are enabled by that very escape to result in a resistance, which, if they were confined by the fluid overhead, as with a submerged body, they would have been unable to produce; in fact at the surface the particles are able to escape the duty of restoring to the body the power which the body employed to set them in motion. There can be no doubt that in this way a fish, when swimming so close to the surface as to make waves, experiences more resistance than when deeply immersed.

It is worth remark that this cause of resistance, "wave-genesis" or "wave-making resistance," as it has been termed, would be equally a cause of resistance in a frictionless fluid, and it is for this reason that in proving to you just now that a body would experience no resistance in moving through a frictionless fluid, I limited the case to that of a submerged body. It is true that in a frictionless fluid the wave system generated by a ship would not waste away, as in water, by its internal friction; but it would none the less be diffused into the surrounding fluid, and thus, as the ship proceeded, she would cover a larger and larger area of ocean surface with the waves she was making.

Having arrived at this point, I think it will be useful briefly to review the several cases of motion through fluid, in order to trace where the several causes of resistance we have dealt with, come into operation.

Case I.—A plane moving edgeways through frictionless fluid. Here there will be no resistance.

Case II.—A plane moving edgeways through frictional fluid. Here there will be resistance due to surface friction.

Case III.—A submerged body moving through frictionless fluid. The inertia of the fluid undergoing stream-line motion, causes excess of pressure at the two ends, and defect of pressure along the middle. The forward and backward pressures balance one another, and therefore cause no resistance.

Case IV.—A submerged body moving through frictional fluid. Here there is resistance due to surface friction. Also, if the body is abrupt enough to cause eddies, part of the excess of pressure at the tail-end will be converted by the friction of the particles of fluid into defect of pressure, and so will destroy the balance between the forward and backward pressures, thus causing eddy-making resistance.

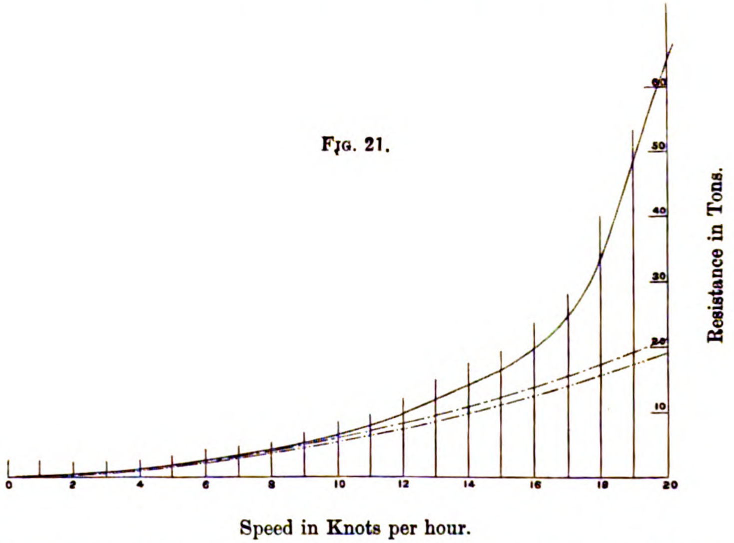
Case V.—A body moving through frictionless fluid, but at or near the surface. The direct pressures on the surface of the body, are altered by the operation of the wave system which has been created, thus destroying the balance of forward and backward forces, and introducing "wave-making resistance."

Case VI.—A body moving through frictional fluid at or near the surface. Here, surface-friction, eddy-making resistance, and wave-making resistance will act in combination, and will together make up the total resistance.

Having thus reviewed the several operations which will combine to cause resistance to a ship moving at the surface of the water, it will be interesting to see in what proportion they are combined in an actual ship of ordinary form; and to take a single instance I show the "curves of resistance," as they are called, of the SS. 'Merkara,' a mercantile ocean steamship of 3980 tons. It is perhaps necessary to explain that a curve of resistance is a diagram constructed to show at a glance the resistance at any speed, so that if any point be taken on the scale of speed forming the base-line, the ordinate or vertical height from the point to the curve above, measured by the scale of force, will show the amount of resistance at that speed. Thus, in Fig. 21, where the uppermost line represents the total resistance of the ship, we see that at a speed of twelve knots the resistance as indicated by the height up to the line is 9·3 tons.

The plain line on Fig. 21, is the curve of total resistance of the 'Merkara' deduced from experiments made with a model of that ship.

Fig. 21.



The lowest of the two dotted lines is the curve of surface-friction resistance of the ship, calculated from experiments made upon the resistance of thin planes moving edgeways through water. The space between the foregoing line and the dotted line immediately above it, represents the amount of resistance due to eddy-making, deduced from data which it would take too long to describe here. The space between this upper dotted line and the plain line above it is the wave-making resistance.

We see then, that with this ship the eddy-making resistance is about eight per cent. of the surface-friction, at all speeds. We see further that at eight knots the wave-making resistance is practically *nil*, that at eleven knots it is only twelve per cent. of the whole resistance at that speed, and that at thirteen knots, which is the maximum speed of the ship, it is seventeen per cent. of the whole. As we go further up in the scale of speed the wave-making resistance mounts up very largely, and at nineteen knots is fully sixty per cent. of the whole resistance.

The curve of resistance here given may be taken as a fair sample of those of ships of good build. It may be said generally that the eddy-making resistance is a comparatively small amount, and that it bears at all speeds nearly a constant proportion to the surface-friction. The wave-making resistance, on the contrary, always increases with increase of speed at a more rapid rate than the surface-friction, being generally *nil* at a very low speed, and becoming, at very high speeds, more than half of the whole resistance. Large ships, however, do not often attain under steam, speeds at which the wave resistance is more than some forty per cent. of the whole.

It is a point worth noticing here, what an exceedingly small force, after all, is the resistance of a ship, compared with the apparent magnitude of the phenomena involved. Scarcely anyone, I imagine, seeing for instance the new frigate 'Shah' steaming at full speed, would be inclined at first sight to credit, what is nevertheless the fact, that the whole propulsive force necessary to produce that apparently tremendous effect is only 27 tons, in fact less than one two-hundredth part of the weight of the vessel. And of this small propulsive force, at least 15 tons, or more than one-half, is employed in overcoming surface-friction simply.

Thus, although the vessel carries at her bow a wave seven feet high, the forces which produce this are so far neutralised by other similar forces that the whole of her resistance, exclusive of surface-friction, might be represented by the sternward pressure on her bow which would be due to a single wave fourteen inches high. Indeed, a wave thirty inches high would represent a sternward pressure equal to the whole resistance of the ship.

The truth is, that the forces which are at work, namely the stream-line pressures due to the inertia of the fluid, are indeed very great; what we have to deal with, in the shape of eddy-making or wave-making resistance, is nothing but a minute difference or defective balance between these great forces, and fortunate it is that they balance as well as they do. With a well-shaped ship at moderate speed we have scarcely any resistance but skin friction, for the balance of stream-line pressures is almost perfect; but nevertheless they are all the while in full operation, a forward force counteracting a backward force, each equal to perhaps five times the existing total resistance of the ship. We can easily imagine, then, that when we once begin to tamper with this balance, we may produce unexpectedly great resistance; and thus when we are dealing with speeds at which the wave-making resistance comes into play, a small variation in form may cause a comparatively large variation in the wave-making resistance. It is this fact which gives the wave-making resistance such a vital importance in connection with the designing of ships; but unfortunately, although the surface friction element of resistance is easily calculated in all cases from general experimental data, neither theory nor general experiment have as yet supplied means of calculation applicable to the wave-making resistance. In the absence of this knowledge we have to rely on direct experiments with different forms of vessels, and to supply these is one of the objects of the experiments upon the resistances of models of various forms which I am now conducting for the Admiralty.

By these experiments I hope not only to obtain a great many comparisons, showing at once the superiorities of some forms over others; but to deduce general laws by which the influence of variation of form upon wave-making resistance may be predicted. Already, indeed, some most instructive propositions concerning the operations of this cause of resistance have shaped themselves; but it would take far too long to describe them in this discourse. I will merely refer to one broad principle which underlies most of the important peculiarities of the wave-making element of resistance.

We have seen that the waves originate in the local differences of pressure caused in the surrounding water by the vessel passing through it; let us suppose, then, that the features of a particular form are such that these differences of pressure tend to produce a variation in the water level shaped just like a natural wave, or like portions of a natural wave, of a certain length.

Now an ocean wave of a certain length has a certain appropriate speed, at which only it naturally travels, just as a pendulum of a certain length has a certain appropriate period of swing natural to it. And just as a small force recurring at intervals corresponding to the natural period of swing of a pendulum will sustain a very large oscillation, so, when a ship is travelling at the speed naturally appropriate to the waves which its features tend to form, the stream-line forces will sustain a very large wave. The result of this phenomenon is, that as a ship approaches this speed the waves become of exaggerated size, and run away with a proportionately exaggerated amount of power, causing corresponding resistance. This is the cause of that very disproportionate increase of resistance experienced with a small increase of speed when once a certain speed is reached, an instance of which is exhibited at a speed of about eighteen knots in the curve of resistance shown in Fig. 21.

We thus see that the speed at which the rapid growth of resistance will commence, is a speed somewhat less than that appropriate to the length of the wave which the ship tends to form. Now, the greater the length of a wave is, the higher is the speed appropriate to it; therefore the greater the length of the waves which the ship tends to form, the higher will be the speed at which the wave-making resistance begins to become formidable. We may therefore accept it as an approximate principle, that the longer are the features of a ship which tend to make waves, the longer will be the waves which tend to be made, the higher will be the speed she will be able to go before she begins to experience great wave-making resistance, and the less will be her wave-making resistance at any given speed.

This principle is the explanation of the extreme importance of having at least a certain length of form in a ship intended to attain a certain speed; for it is necessary, in order to avoid great wave-making resistance, that the "wave features," as we may term them, should be long in comparison with the length of the wave which would naturally travel at the speed intended for the ship.

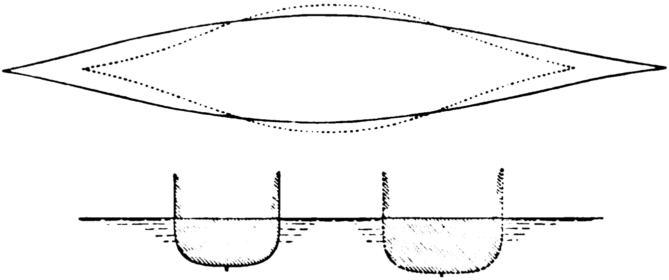
Time will not admit of my describing to you in detail how the principles I have been explaining, affect the practical question of how to shape ships. I must leave you to imagine for yourselves, if you feel interested in following up the question, how the desirability of length of "wave features," for lessening wave-resistance, is to a greater or less extent counteracted by the desirability of shortness of ship for lessening surface-friction; and how in many other ways a certain variation of form, while it is a gain in one way is a loss in another, so that in every case the form of least resistance is a compromise between conflicting methods of improvement.

My principal object has been to combat the old fallacy of "head-resistance," as it has been sometimes called, due to the inertia of the water acting against the area of the ship's way. I hope I have made it clear to you, that the inertia of a frictionless fluid could offer no opposing force to a submerged body of any shape moving through it, for that the forces there developed by the inertia against the body, must of necessity push it forwards exactly as much as they push it backwards, and that when the body is moving through a frictional fluid, or when it is moving at the surface of a fluid, this balance is only more or less destroyed through the operation of conditions which are totally independent of the area of midship section or area of ship's way.

For this reason, the only instances I have time to give you of the application of our knowledge of the causes of resistance to practical questions, shall be directly applicable as illustrations of the fallacy of the midship section theory.

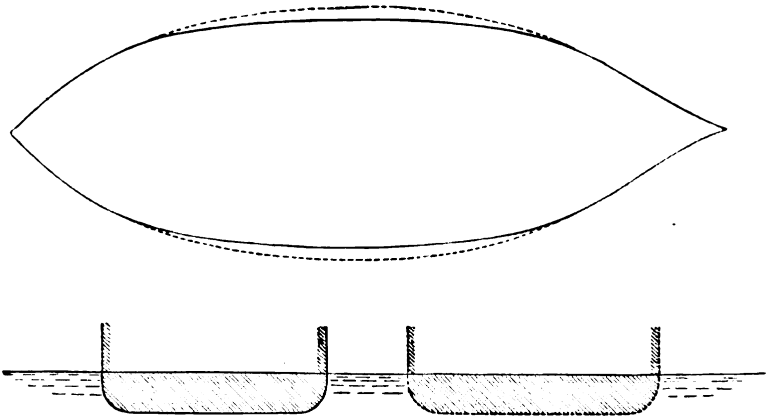
Let us suppose, that Fig. 22 represents the respective water-lines of two vessels of the same tonnage but of different proportions of length to breadth. Now it is true that the shorter of the two, when the speed of the wave appropriate to its wave features is approached, will experience great wave-making resistance, and will therefore probably experience greater total resistance than the longer ship. But it is certain that at low speeds when the wave-making resistance of both ships is practically *nil*, the shorter ship will make the least resistance, because the long and narrow one has the largest area of skin, and will therefore have the greatest surface-friction resistance. Judging, however, by the midship section theory, we should have erroneously concluded that the short and broad ship would make the greatest resistance of the two at all speeds.

FIG. 22.



Next let us take the two ships, whose waterlines are shown in Fig. 23. It may be seen that the one shown in dotted lines has the same length, and the same sharpness of ends as the other, but is filled out amidships to a larger cross section. On the midship section theory, this one would clearly have the greatest resistance of the two. Nevertheless, in the trial of two models of those lines it appeared

FIG. 23.



that at the higher speeds, the form with the largest cross section made considerably the least resistance. The explanation of this lies of course in the fact that the addition amidships, though increasing the displacement, forms a prolongation of the wave features of the two ends, and thus lessens the wave-making resistance.

In conclusion let me again insist, and with the greatest urgency, on the hopeless futility of any attempt to theorise on goodness of form in ships, except under the strong and entirely new light which the doctrine of stream-lines throws on it.

It is, I repeat, a simple fact that the whole framework of thought by which the search for improved forms is commonly directed, consists of ideas which, if the doctrine of stream-lines is true, are absolutely delusive and misleading. And real improvements are not seldom attributed to the guidance of those very ideas which I am characterising as delusive, while in reality those improvements are the fruit of painstaking, but incorrectly rationalised, experience.

I am but insisting on views which the highest mathematicians of the day have established irrefutably; and my work has been to appreciate and adapt these views when presented to me.*

No one is more alive than myself to the plausibility of the unsound views against which I am contending; but it is for the very reason that they are so plausible that it is necessary to protest against them so earnestly; and I hope that in protesting thus, I shall not be regarded as assuming too dogmatic a tone.

In truth, it is a protest of scepticism, not of dogmatism ; for I do not profess to direct anyone how to find his way straight to the form of least resistance. For the present we can but feel our way cautiously towards it by careful trials, using only the improved ideas which the stream-line theory supplies, as safeguards against attributing this or that result to irrelevant, or rather, non-existing causes.

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* I cannot pretend to frame a list of the many eminent mathematicians who originated or perfected the stream-line theory; but I must name from amongst them, Professor Rankine, Sir William Thomson, and Professor Stokes, in order to express my personal indebtedness to them for information and explanations, to which chiefly (however imperfectly utilised) I owe such elementary knowledge of the subject as alone I possess.

[W. F.]

WEEKLY EVENING MEETING,

Friday, May 19, 1876.

Sir Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Charles T. Newton, C.B.

KEEPER OF THE GREEK AND ROMAN ANTIQUITIES IN THE BRITISH MUSEUM.

The Recent Discoveries at Olympia.

The discourse began with a brief notice of the topography of the Olympian plain in Elis, in the Peloponnesus, and a description of the nature and objects of the ancient Olympic festival, revived by Iphitus, King of Elis, to promote harmony in Greece, of which we have records, beginning with the victory of Corœbus, 776 B.C., and ending with its abolition by Theodosius, A.D. 394. The games included races on foot, on horseback, and in chariots, wrestling, boxing, and other athletic exercises; and the contests were open to free Greeks of all ranks, of pure Hellenic blood, an Olympian prize being to them the highest honour conceivable. During the festival a sacred armistice was established if held in time of war; and treaties of peace were often published at Olympia. One of the objects was military training, to enable a small number of Greeks to resist a vast number of foes; but the chief motive was undoubtedly religious. The hymns were more in honour of Zeus than of the victor in the contests, and Pindar's odes are more admonitory than laudatory.

In the second century of our era Olympia was visited by Pausanias, who gives in his 'Itinerary' a most interesting description of the then state of the Temple of Zeus, Phidias's colossal statue of the god in ivory and gold, the large number of statues of victors, with edifices abounding in treasures (occupying a space of about a square mile), which no doubt conduced to the abolition of the festival.

After alluding to the visits of Chandler, Leake, and Stanhope, Mr. Newton described the explorations of the French expedition in 1828, when the site of the temple was ascertained, and a plan made, of which he exhibited a copy; and the sculptures then discovered were conveyed to the Louvre. At the part where the French left off the work was taken up, in the autumn of 1875, by the German expedition, conducted by Messrs. Hirschfeld and Bötticher, whose discoveries have been of transcendent interest. Some of these were described by Mr. Newton, who, in company with Professor Colvin, lately visited the spot. He specially commented on a noble but mutilated statue of Victory, most probably the work of Pæonios, a contemporary of Phidias, of which a description is given by Pausanias, and he also described some sculptures of the eastern pediment of the temple, stated by Pausanias to represent the preparation for a chariot-race between Pelops and Œnomaus, King of Elis. These and many of the torsos found are of very unequal merit, and proved, as was remarked, that the decorations of the Greek temples were often committed to inferior artists. After alluding to several other works, and commenting on some interesting bronze tablets, with inscriptions, recording treaties or granting civic privileges, the discourse concluded with a warm tribute of gratitude to the Germans for their noble enterprise, and the expression of a confident hope of still more important results.

WEEKLY EVENING MEETING,

Friday, May 26, 1876.

William Spottiswoode, Esq. M.A. LL.D. F.R.S. Secretary and Vice-President, in the Chair.

J. Fletcher Moulton, Esq.

The Verification of Modern Scientific Theories.

The speaker began by calling attention to the rapid advances made by science in recent times as well in the world of organic as of inorganic matter. This advance had consisted partly of the discovery of a vast number of isolated scientific truths, and partly of the discovery of wide general principles. Such is found to be the case in any age that is rich in discoveries; and as the wide general principles thus arrived at embrace many of the less general truths previously discovered, the people in such an age have the choice of two methods of demonstrating these truths; the one by direct induction from facts and experiments specially relating thereto, and the other by deduction from some wide generalization which has itself been established by an induction proper to itself. Different minds will be affected differently by the two processes, accordingly as they are more keenly alive to a sense of general or of special harmony. A parallel is to be found in the two rival styles of mathematical teaching so well exemplified in our English text-books—one class of text-books prefer to prove particular theorems by special proofs, each of which is applicable to its own theorem alone, while the others prefer to face the difficulties of the demonstration of general theorems at the outset, and deduce from them the theorems they need as particular cases.

The fact that there are classes of minds to which each method is in turn specially convincing makes them both alike necessary and valuable even in cases where either would be sufficient alone, for in science our aim is to convince, and truths are often arrived at and firmly held by the better scientific minds long before the evidence for them is sufficient to render their demonstration incontestable. Such cases, i. e. of truths to reject which is not insane but only stupid and unprofitable, constitute the most interesting part of science in every age, and it is the attitude of a mind towards these nascent discoveries that measures its enlightenment. But though both methods are useful in the attainment of scientific truth, the one that seeks to deduce particular truths from general ones is the one to which we should ultimately advance, and is, as we shall see, of far greater value as a training to the scientific faculty. It is thus of great importance that we should go over our scientific beliefs from time to time, especially just after some wide generalization has been made, to see how many of the special truths that we hold are instances of it either taken alone or in combination with other generalizations, and we should accustom ourselves, so far as is possible, to use these great fundamental truths in our scientific reasoning.

This review of the grounds of our beliefs is highly useful for another reason. There is a very remarkable difference between the amount of evidence demanded by the mind in this re-examination—this verification—of its beliefs, and that which sufficed to convince it at first. It is true that the demonstration may need to be stricter, but in many cases the mind is rightly less exacting in its requirements as to proof than it was at first. To use a simile that is justly somewhat unsavoury at the present time, a new theory is like an unexpected claimant to an estate. It is true that reflection may lead us to see flaws in evidence that at first sight appeared irrefragable, but on the other hand it may be that we shall discover that we have been unjustly incredulous. In such cases every little peculiarity is counted suspicious; and the unfortunate aspirant is required to explain away many a difficulty which makes neither for nor against his claim, and he is refused credence until he has done so. Let him, however, once establish his claim to be the rightful heir, and let people become accustomed to the idea of his being so, and they straightway begin to see how much more proof they required than was sufficient for the purpose, and half the evidence that was needed to win the estate would enable him to hold it. In a similar way it will be found that some of the greatest discoveries of this age will be considered by future ages to have been sufficiently proved by less evidence than that which now is considered by large numbers of intelligent men to leave them debatable questions.

The speaker then examined the recently discovered truths which form the group known as the Conservation of Force or Energy. These constitute the continuation, or we might almost say the completion of Newton's work on matter and motion, and, together with the complementary theorem of the Dissipation of Energy, they give the governing laws of the working power of the universe. These laws, as now known, are exact quantitative laws; they are universally active, and, in fact, all phenomena are only illustrations of their working. Numberless apparently distinct theorems follow directly from them, and in addition they constitute the most valuable check to the correctness of our hypothesizing, and must sooner or later be the final test of all theories. Their universality and exactness are immediately useful in enabling us to calculate the effect of forces acting on a scale immensely greater or immensely more minute than that on which they act in the cases that usually come under our notice, and of which we have experience. Apart from some such guide as this, it would be well-nigh impossible to guess the effect of even well-known forces acting on a wholly unaccustomed scale. In other words, when the scale on which a force acts is such as to admit of the disturbing influence of the feeling of wonder in our reasonings, it is hopeless, to attempt to arrive at reliable conclusions by processes that do not rest on accurate numerical laws. By taking the problem of the influence of the Gulf Stream on climate, the new theories of oceanic circulation, and the controversy between the uniformitarian and catastrophic schools of geologists, the speaker contrasted the unreliable guesses of earlier theorists with the distinct utterances of these universal physical laws, when interrogated in a right manner, and investigated the extent of the verification which we thus obtain for our modern theories on these important matters.

Although in their nature these laws are exact and quantitative, yet the problems to which we have to apply them are often too complicated to permit of our making use of them in this their most complete form, and we are compelled to content ourselves with drawing conclusions from them as to the general phenomena that characterize the processes of change in nature. For this they are eminently suited, inasmuch as they are, so far as we can ascertain, universal both in space and time, and have been the shaping laws of the universe during the whole of its existence. It is from them alone that we derive information about the earliest form of the earth, and its condition and history during those ages whose geologic record is too blurred by time to be any longer legible. Examples of this application of these laws are to be found in Laplace's Nebular Hypothesis, and in the more definite conclusions of Sir William Thomson as to the Plutonic history of the earth. So useful are they for purposes such as this, that by their aid we speedily arrive at a general knowledge of the types of change that will most frequently present themselves in the universe; and in this way we arrive at a scientific measure of the probability of theories, and are enabled to determine how great or how small an amount of evidence will be needed to support them. Apparent strangeness is no longer a ground for rejecting a theory; its credibility is measured by its harmony with some one of the prevailing types of change; and we have thus taken a further step towards freeing ourselves from the disturbing influence of unenlightened wonder—the faculty which is the most detrimental of all to scientific thought, and the greatest hindrance to scientific progress. This determination of the amount of evidence necessary to support a Theory may be considered as an indirect verification (though an incomplete one) of all such theories as are shown thereby to be highly probable, even in the absence of direct evidence for them.

Such considerations as these have led some eminent philosophers to believe that we can arrive at secondary laws of change, by the aid of which we shall be able in all cases to foretell the course of development without going back to the fundamental laws. This the speaker held to be an error. No such secondary laws have been arrived at inductively, and none can be obtained deductively, so far as we are able to see at present; those proposed are vague and unmeaning, and no interpretation that can be given to them will enable them to stand the test of a rigorous examination. Nor is this failure to be wondered at. The most marked characteristics of any special type of change are often less directly the consequences of the fundamental laws that ultimately govern the change, as of the special circumstances that distinguish the cases in which it occurs. The most remarkable instance of this is to be found in the class of problems that relate to living things. We do not doubt that the laws of physics and chemistry are ultimately as supreme over the organic world as they are over the inorganic; but it would be very unwise to approach the one solely through the other, or to expect the prevalence of the same types of change in both. The special circumstances which make life possible, and those in connection with which it alone occurs, entail consequences which are much more evident, and influence to a much greater degree the prevailing types of change than do the great laws of physics, considered in their most general and abstract form. No further illustration of this is needed than the fact that one of the most important laws of all organic life is heredity, to which no trace of a sound analogy can be found in all the universe of inorganic matter; every proposed analogy turns out on examination to be false and delusive. If then so important a shaping law of organic nature is exclusively confined to it, it must be that in living things we have so peculiar a type of phenomena, that the peculiarities of the type have more direct influence on its laws of change than have the fundamental laws that ultimately govern all change of whatever kind it be. So far then from seeking to find secondary laws of change common to the two, we shall do wisely to keep as distinct as possible our investigations into the laws of organic and inorganic nature, and to view with suspicion, as being probably accidental and superficial, any striking resemblances or analogies between their governing laws; and the speaker then proceeded to point out the special circumstances which are essential to the existence of life, and which make it improbable that the difficulties of biology will be successfully approached from the side of physics or chemistry.

The speaker then drew attention to the fundamental laws which in the case of life take the place of those laws of physics which he had been discussing, and analyzed the process of verification of the Descent-Theory when we start from these laws. He showed that the truth of this theory is an immediate corollary from one or two facts which physics and geology tell us about the history of the earth in the past, coupled with what we know of the laws of change of the organic world. The amount of evidence necessary to establish this theory was greatly exaggerated at first. This error is to be traced to two causes: in the first place, Mr. Darwin, to whom the triumph of the theory is due, had, as the object of his great work on the Origin of Species, the establishment of another and totally distinct theory, i.e. that the development that accompanied this descent was primarily due to Natural Selection. The two theories have thus become so identified in the minds of most people, that they confound the amount of evidence requisite to prove the fact of the descent of our present world of animated nature from simple organisms, with that requisite to settle the infinitely more difficult question of the forces that have guided and modified it during the process of that descent. And secondly, the novelty of the theory has caused the world to over-estimate the amount of evidence requisite to establish it, as might have been expected from the considerations dwelt upon at the commencement of the lecture.

The attempt to verify the theory which more peculiarly belongs to Mr. Darwin, i.e. that of the part played by natural selection in evolution, leads to very different results to the above, though it soon becomes evident that natural selection has had a most important influence in directing development. Any attempt to estimate how large a part it has played, necessarily implies that we have either some quantitative estimate of its effects, or that we have obtained a knowledge of some of the fundamental laws of organic life in their exact and quantitative form. Without some such knowledge as this, any attempt to arrive at the accumulated effect of natural selection during millions of years, is mere guess-work, and is made under circumstances which, as we have seen, are specially likely to induce error, through the opportunity they give for the disturbing influence of the highly unscientific faculty of Wonder. No such quantitative estimate has ever yet been made, nor indeed are we at present prepared for it, for we are still so ignorant of the laws of variability, or, in other words, our enunciation of the law of heredity is still so imperfect, that we cannot hope speedily to arrive at sound conclusions as to the effect of the combined action of that variability, and the selective influence of the struggle for existence; in fact, it is not too much to say that but a very small portion of the problem of development can be considered as solved; the greater part of it has perhaps not yet been enunciated. The crowning merit of Mr. Darwin's discovery, and the cause of our especially keen feeling of gratitude to him, is that he has solved the part that troubled us most. Up to his time the peculiar adaptations so common in the world of life were supposed to be beyond the power of the blind, purposeless laws of nature to effect; and he was the first to show that this apparent motivation might be caused by natural laws, without the interference that the teleologist asserted to be necessary.

[J. F. M.]

WEEKLY EVENING MEETING,

Friday, June 2, 1876.

William Bowman, Esq. F.R.S. Manager, in the Chair.

Professor Roscoe, Ph.D. F.R.S.

Recent Discoveries about Vanadium.

The name Vanadium, from Vanadis, a designation of the Scandinavian goddess Freia, was given by the Swedish chemist Sefström to a new metal obtained for the first time by him in the most minute quantities in the year 1830.

From that time up to 1868 our knowledge respecting the habits and proclivities of vanadium was derived entirely from the one research of Berzelius, so that its relations to the other elements were but ill understood. In the above year the speaker brought before a Royal Institution audience [*] the fact that this metal which had hitherto been wandering astray amongst her fellow elements had at last been brought under the civilizing influences of law and order, so that now she occupies a well-recognized position, and is looked upon as a settled member of the society to which she is acknowledged to belong.

One of the most important aids which the chemist possesses in the classification of the elements is the doctrine of isomorphism. Thus the analogy and family likeness between the elements phosphorus, arsenic, and vanadium, is pointed out in the isomorphism of the minerals pyromorphite, mimetesite, and vanadinite, all of which crystallize in hexagonal prisms, terminated with hexagonal pyramids having the same angles and the same relative length of axes.

In the year 1868 the fact was ascertained that the chemical constitution of the vanadium compounds assigned to them by their first investigator, Berzelius, did not accord with that which their crystallographic relations required. Thus whilst to the highest oxides of phosphorus and arsenic, contained respectively in the isomorphous minerals pyromorphite and mimetesite, the formulæ P_2O_5 and As_2O_5 had been given by common consent, the formula which, according to Berzelius, denotes the constitution of the highest and the corresponding oxide of vanadium is V_2O_3. Here then we have either to do with an exception to the law of isomorphism, or the experimental conclusions of Berzelius are incorrect.

On the occasion above referred to, the speaker stated that the key to the enigma had been obtained, inasmuch as it had been shown that Berzelius had overlooked the existence of two additional atoms of oxygen in the highest oxide of vanadium to which, therefore, the formula V_2O_5 must be given instead of V_2O_3. According to Berzelius the atomic weight of vanadium is 67·3, and the highest oxide, V_2O_3, has a molecular weight of 182·6. If, however, two more atoms of oxygen really occur in this oxide, and if the substance supposed by Berzelius to be a metal is in fact an oxide, we must subtract 16, or the weight of one atom of oxygen, from 67·3, and we get 51·3 as the true atomic weight of the metal. This then proves the analogy of vanadium (V = 51·3) with phosphorus (P = 31) and with arsenic (As = 75), and thus explains the observed isomorphism in the series of minerals above referred to.

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[*] Friday, February 14, 1868: 'Proceedings,' vol. v. p. 287.

The speaker then reminded his audience that vanadium is the fourth substance supposed by its discoverer to be a metal which had recently been shown to be a compound body.

Titanium.	Uranium.	Niobium.	Vanadium.
Wollaston .. 1823	Klaproth .. 1789	Hatchett .. 1801	Sefström and Berzelius 1831
Wöhler .. 1849	Peligot ., 1849	Rose .. 1842–64	
		Marignac .. 1865	Roscoe .. 1867

Further research only tends to prove more conclusively the striking analogy between vanadium and the other members of the triad group of elements. Thus we now know that the elements can be grouped according to a natural system in families, and vanadium with the new atomic weight, fits exactly into the system, being placed between phosphorus and arsenic, and near to the analogous elements chromium and niobium, whereas when Berzelius's atomic weight is employed these relations are altogether ignored. This relation is seen in the following table.

TABLE SHOWING THE GROUPING OF SOME OF THE ELEMENTS (MENDELEJEFF).

Monads ..	F 19	Cl 35·5	..	Br 80	..	I 127	..	..
Dyads ..	O 16	S 32	..	Se 79·5	..	Te 128	..	..
Triads ..	N 14	P 31	V 51·3	As 75	Nb 94	Sb 122	Ta 182	Bi 210
Tetrads ..	C 12	Si 28	Ti 50	..	Zr 89·6	Sn 118	..	..
Hexads ..	..	..	Cr 52·4	..	Mo 96	..	W 184	U 240

In many other respects the close analogy between vanadium and phosphorus has been confirmed. Thus the speaker has shown that the salts of vanadic acid, like the salts of phosphoric acid, are capable of existing in three distinct series, the ortho-salts, the pyro-salts and the meta-salts. The analogous composition of these salts is clearly shown in the following table.

	Phosphates.	Vanadates.
Ortho-Salts	Na_3PO_4	Na_3VO_4
	Ag_3PO_4	Ag_3VO_4
	$Pb_3(PO_4)_2$	$Pb_3(VO_4)_2$
Pyro-Salts	$Na_4P_2O_7$	$Na_4V_2O_7$
	$Ag_4P_2O_7$	$Ag_4V_2O_7$
	$Ca_2P_2O_7$	$Ca_2V_2O_7$
	$Pb_2P_2O_7$	$Pb_2V_2O_7$
Meta-Salts	$NaPO_3$	$NaVO_3$
	NH_4PO_3	NH_4VO_3

The three series of phosphates and vanadates do not, however, agree in order of stability. Thus it is well known that the tri-basic phosphoric acid and the soluble ortho-phosphates are more stable than the members of the pyro-series, and these again more stable than the meta-compounds. In the case of the vanadates the order is reversed, for at the temperature of boiling water the soluble ortho-vanadates split up into the pyro-salt and free caustic soda, thus:

$$2(Na_3VO_4) + H_2O = Na_4V_2O_7 + 2NaOH.$$

Whilst, again, the pyro-salts are readily decomposed by the weakest acids such as carbonic acid with formation of the meta-salt and a carbonate, thus:

$$Na_4V_2O_7 + CO_2 = 2NaVO_3 + Na_2CO_3.$$

Another proof of the comparatively greater stability of the meta-compounds is found in the fact that meta-vanadic acid HVO_3 has recently been prepared by Dr. Gerland. It has recently been stated by M. Antony Guyard * that the substance supposed to be meta-vanadic acid is an ammonium salt. The speaker in consequence had carefully analyzed a sample prepared by Dr. Gerland, which gave the following results, proving that this assertion is incorrect, and that meta-vanadic acid really exists:

(1) 0·1110 substance dried at 100° yielded on ignition 0·0983 V_2O_5.

(2) 0·2713 of the same substance after continued washing, dried at 100° yielded on ignition 0·2435 V_2O_5.

Hence we have:—

	Calculated.	Found. (1)	Found. (2)
Percentage of V_2O_5	91·05	88·57	89·84

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* 'Bull. Soc. Chim.' xxv. 356.

It is a most remarkable substance—a yellow solid body crystallizing in bright golden plates, which have so bright a lustre and are so permanent in the air that they may possibly be used as a substitute for gold-leaf in gilding. Ortho-vanadic acid H_3VO_4 and pyro-vanadic acid $H_4V_2O_7$ have not as yet been isolated.

In many of their physical as well as their chemical properties the vanadium and phosphorus compounds exhibit a close analogy. Thus Gladstone[*] has shown that the specific refractive energy for phosphorus (as deduced from the refractive indices of $POCl_3$) is 0·361, whilst that of vanadium, deduced from $VOCl_3$, is 0·494. If vanadium were an ordinary metal having the atomic weight 51·2, we should expect its refractive energy to be only about 0·13. Hence it is entirely thrown out of the group of the heavy metals, and brought into the group of one of the most refractive and dispersive bodies, viz. phosphorus.

In the same way the atomic volumes of the corresponding oxychlorides $POCl_3$ and $VOCl_3$ have been shown by Thorpe[†] to stand in the same relation to each other as the atomic volumes of the tetrachlorides of silicon and titanium do to one another. Thus the difference both in molecular weight and specific volume is in both cases nearly constant.

	Molecular Weight.	Difference.	Specific Volume.	Difference.
$POCl_3$	153·38	20·35	101·58	4·96
$VOCl_3$	173·73		106·54	
$SiCl_4$	169·94	21·90	121·13	4·90
$TiCl_4$	191·84		126·03	

Hence it appears that the constitution of vanadyl trichloride is similar to that of phosphoryl trichloride, and that vanadium is to be represented as a triad, the oxygen having the specific volume of 7·8 assigned by Kopp to that element when attached by one combining unit only.

The close relationship exhibited between vanadium, phosphorus, and arsenic, is clearly shown by the very peculiar poisonous action of the vanadium compounds on the animal system. A most careful and extended series of experiments on this subject, made by Mr. John Priestley, in the Owens College physiological laboratory, under Professor Arthur Gamgee's superintendence, has yielded the following results. The poisonous effects of vanadium are indicated by paralysis of motion; convulsions, local and general; drowsiness or indifference to external circumstances; congestion of the alimentary mucous membrane; discharge of sanguinolent fæces; presence of glairy fluid mucus in the intestine; certain changes in respiration, and coincidently a fall in temperature. The effects which vanadium exerts upon the respiration are at first an acceleration, then a retardation, and ultimately an arrest of respiration, due to an action of the poison on the respiratory centre situated in the medulla oblongata, and which is at first stimulated, but afterwards paralyzed.

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[*] 'Phil. Trans.' 1870.

[†] 'Proc. Roy. Soc.' xxiv. 284.

The circulation is also notably affected by vanadium, which produces a fall in the blood pressure, and an intermittance or stoppage of the heart's action. This first action is partly due to a paralysis of the vaso-motor centres, the second to a direct poisonous action which vanadium compounds exert upon the intrinsic nervous apparatus of the heart. These results point to a close relationship between the poisonous action of arsenic and vanadium, and, therefore, go to establish a physiological as well as a chemical resemblance between these two metals. From experiments, which are as yet unpublished, but the results of which have been kindly communicated to the speaker by Dr. Arthur Gamgee, it appears that although the physiological action is in kind apparently the same in the case of the different vanadium compounds, yet the intensity of the poisonous activity is influenced by the nature of the compound, and this relationship in the case of the vanadium compounds and of the phosphorus compounds appears to be a very close one. Thus, if we examine the comparative poisonous and lethal actions of the salts of ortho- and meta- and pyro-vanadic acids, we find that the poisonous activity is least in the case of the first, and greatest in that of the third of these compounds. A subsequent examination of the poisonous action of the corresponding phosphoric acids has exhibited the remarkable fact that whilst the ortho-phosphates appear to be inert, the pyro-phosphates are almost as poisonous as arsenic compounds.

The speaker had recently been able to forge still closer in another direction the links connecting vanadium with the older members of the triad group, by the discovery of a new vanadium mineral, which forms the third member of the following series of phosphorus, arsenic, and vanadium compounds, all of which doubtless crystallize in the same form, and certainly have an analogous chemical composition. To the new mineral the name of Mottramite, from the locality where it was first obtained, has been given, and the complete series is:

Dihydrite	$Cu_3P_2O_8 + 2\,Cu(OH)_2$
Erinite	$Cu_3As_2O_8 + 2\,Cu(OH)_2$
Mottramite	$(CuPb)_3V_2O_8 + 2\,(CuPb)\,(OH)_2$

A second new vanadium mineral has lately been discovered by Dr. James Blake, of San Francisco, and analyzed by the speaker. It is a very interesting substance, and contains 28 per cent. of vanadic acid combined with alumina, potash, and silica. The formula of this mineral, to which Dr. Blake proposes to give the name of Roscoelite, is:

$$4\,Al'''VO_4 + K_4Si_9O_{20} + H_2O.$$

It is a greenish talc-like mineral found in a gold mine, serving as a matrix for the gold; it occurs in small bunches, filling cavities in a schistose porphyry.

It is not, however, so much in relation to other elements that vanadium is interesting, as by virtue of the remarkable properties which the metal itself possesses. Thus the lowest stage of oxidation which the element can assume, viz. V_2O_2, is a powerful reducing agent, bleaching indigo by reduction almost as quickly as chlorine does by oxidation. On allowing the solution thus bleached to stand, the hypovanadous salt at once takes up oxygen from the air, and indigo blue appears again. On the other hand, the highest oxide V_2O_5 parts with its oxygen readily, and is easily reduced by organic matter to a lower oxide; acting in this respect like the highest oxide of chromium CrO_3.

This property of vanadic acid enables this substance to be employed in photography.* If gelatine be mixed with sodium divanadate, and the film unequally exposed to light, the portions strongly insolated become slightly less soluble in warm water than the non-exposed portion, so that it is possible to print from such a film. Again, if paper which does not contain any animal size is coated with a solution of sodium ortho-vanadate and then exposed to light, the portion insolated assumes a dark tint dependent upon the length of exposure and the strength of the solution employed. If the paper thus prepared be immersed, after exposure, in a solution of silver nitrate, the colour in the exposed part instantly changes to a dark brown or black colour, doubtless due to the reduction of the silver salt by the vanadous compound formed in the paper. Paper thus prepared may be used for photographic printing. The unexposed portions of the print are in this process coated with yellow silver ortho-vanadate; but this can be completely removed by ammonia or by sodium hyposulphite. Silver ortho-vanadate is capable of forming a latent image, like the chloride or bromide, and this may be developed by the ordinary ferrous developer. Two or three minutes' exposure to sunlight is needed. In the development little or no silver nitrate must be present.

By far the most important and interesting application of vanadium is, however, that recently suggested for the preparation of a permanent black which is now largely coming into use amongst dyers and calico printers, and is already extensively employed as a permanent marking ink.

Of the commercial value of a permanent rich black dye it is difficult for the uninitiated in such matters to form an idea. Suffice it to say that it is very great. We must, however, remember that this application of vanadium is only in its infancy, and whether the vanadium black will realize all the requirements of practice is a question which can only be settled by long and patient inquiry; still it has already so far proved a success that we may look forward with confidence to its future.

It is not at first sight easy to understand how a rare substance like vanadium, the price of which not long ago was 1*s.* 6*d.* per grain, and which even now cannot be obtained for less than ½*d.* a grain, can be employed for the production of a black colour, which, if not cheap and able to compete with the other common dyes, is of course useless in a practical and commercial point of view.

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* This proposal was first made by Mr. James Gibbons in 1874.

In order to understand the possibility of the technical application of one of Nature's rarest gifts, the history of the preparation of aniline black must be noticed. The splendid red, crimson, violet, green, and blue colours which are obtained from aniline are now universally known and appreciated, and their wide-spread manufacture serves as a striking illustration of the value of original scientific investigation. It is, however, not so generally known that not only the bright and gay colours, but also sombre browns and jetty blacks—by far the most valuable because by far the most generally used of colours—can be obtained from aniline.

In the year 1860, Mr. John Lightfoot, calico printer, of Accrington, applied to the processes of calico printing a black colouring matter which had been previously obtained in the manufacture of mauve from aniline by Messrs. Roberts, Dale and Co., of Manchester.

This black colouring matter is invariably formed when either aniline or toluidine, or mixtures of these two substances, are subjected to oxidizing actions; but in spite of several researches which have recently been published on aniline black, we are as yet unacquainted with its chemical formula, nor indeed can we say that it even possesses a constant chemical composition.

In order that a colouring matter shall be fixed or permanent, it must be fastened in some way to the fibre of the cloth. In the case of cotton this is generally effected (1) either by the precipitation of the soluble colouring matter in the fibre by means of a mordant which forms an insoluble compound termed a lake with the colour, as in madder dyeing and steam-colour printing; or (2) by the fixation of the colour by means of albumen, as in pigment printing; or (3) by the gradual oxidation and consequent precipitation of the colouring matter in the fibre, as in indigo printing. It is to this latter class of processes that aniline-black dyeing or printing belongs; for the aniline salt under the action of certain oxidizing agents passes more or less quickly from the condition of a colourless solid readily soluble in water, into that of a black amorphous insoluble powder not to be distinguished at first sight from soot. Hence if the cloth can be impregnated with the aniline and with the oxidizing agent at the same time, and if the process of oxidation can be allowed to go on in the fibre, the black will be formed and will be permanently fixed in the fabric.

Many oxidizing agents, such as chlorine, ozone, or electrolytic oxygen, have the power of transforming aniline into this black pigment. In most cases a high temperature is needed for this purpose. Thus, for instance, if aniline is heated with chlorate of sodium, and if then hydrochloric acid be carefully added, a deep black almost solid mass is produced. In order, however, that the process may be employed in dyeing and calico printing, it is absolutely necessary to avoid high temperatures as well as the action of strong acids, because when exposed to these the cloth invariably is rotted or becomes "tender." If a mere mixture of aniline salt and chlorate of potash be heated strongly enough, the black is formed; but the heat necessary to produce the colour is sufficient, together with the hydrochloric acid which is at the same time liberated by the decomposition, to make the cloth rotten, and therefore to render this process useless.

It was found by Lightfoot that if an addition of 4 ounces of nitrate of copper solution was made to the pound of aniline and to the chlorate, the oxidation of the aniline went on at a lower temperature than when the copper salt was absent, and hence, when carefully worked, the black could be formed by this process without tendering the cloth. Certain technical objections to this process, however, soon arose; and in 1865 Lauth proposed to use the insoluble copper sulphide instead of the soluble nitrate, by which means he prevented the deposition of copper on the "rollers" and on the "doctors" which took place in Lightfoot's process. The method thus modified has been and is now extensively used for the production of black, and the chief, if not the only, objection which can be urged against it is that the black thus obtained is not perfectly permanent, but is liable to become green when exposed to reducing agents, such as the sulphurous acid contained in the impure air of our towns. This is, however, a serious drawback, and one which those practically engaged in solving such problems have not been able to remove. So much so indeed is this the case, that it is generally believed that the property of aniline black to become green when exposed to sulphurous acid, and to return to the black when treated with alkalies, is an essential property of the substance, which may be compared with the property of litmus to change colour in presence of acids and alkalies.

That the aniline black can not only be produced in presence of copper but also, as Mr. Lightfoot showed in the year 1871, in presence of vanadium salts, and that by vanadium alone can the black be obtained of the requisite permanent character, has now been proved beyond doubt. Moreover, the quantity of the vanadium necessary in order to produce the oxidation of the aniline is about one thousand times less than that of the copper. Thus if a piece of calico be dipped into a solution of 2·5 grains of vanadate of ammonia dissolved in a gallon of water and then dried, the cloth thus prepared is capable of producing an intense black if treated with the mixture of aniline salt and chlorate. In the same way if 1 gallon of colour be made containing 20 ounces of aniline hydrochlorate, 10 ounces of chlorate of soda, and 3 grains of vanadate of ammonia, a mixture is obtained with which no less than from 20 to 25 pieces, or from 500 to 600 yards of cloth, such as that exhibited, can be thus printed of a permanent black.

In dyeing also, the vanadium will be extensively used; and in the same way only mere traces of this rare metal are requisite, whereas the copper black cannot be used for dyeing. Thus, for instance, 1 gallon of colour intense enough to dye 40 lbs. of cotton yarn black is obtained by mixing 8 ounces of aniline hydrochlorate, 4 ounces of sodium chlorate, and 8 grains of vanadate of ammonia. Cotton, wool, or silk dipped twice into this mixture and then aged, or allowed to oxidize, and "raised" in a solution of carbonate of soda, is dyed a deep rich and permanent blue black. The goods may also be allowed to steep in a bath of the above strength for three days, then well washed in warm water, or boiled in a weak solution of acetic acid, to remove any bronze colour found on the surface of the silk or wool. The permanent black is then formed, and the fibre found to be quite strong.

The part played by vanadium in the formation of the black colour may be easily explained, when we remember the case with which the metal passes from one degree of oxidation to another; thus from V_2O_5 the highest degree, to V_2O_4, and *vice versâ.* In this way it doubtless acts, as M. Guyard has suggested, as a carrier of the oxygen of the chlorate to the aniline, being alternately reduced and re-oxidized, so that an infinitely small quantity of vanadium compound will convert an infinitely large quantity of aniline salt into aniline black, reminding one of the action of nitrous fumes in the leaden chamber.

Some time after the discovery of aniline black, Mr. Robert Pinkney, of the firm of Messrs. Blackwood and Co., of London, discovered, independently of Mr. Lightfoot, that vanadium can be most advantageously substituted for copper in the formation of aniline black; and he employed this reaction for the preparation of a permanent marking ink termed "Jetoline," of which many thousands of bottles have been sold. A few grains of vanadium—say from seven to twelve—being sufficient to produce, together with hydrochlorate of aniline and chlorate of soda, a gallon of marking ink.

The subject of the use of vanadium as a valuable dyeing agent was next taken up by the Magnesium Metal Company, of Patricroft, near Manchester; and, thanks to the unwearied exertions of Mr. Samuel Mellor, this firm have now succeeded not only in securing a very considerable supply of the rare element which occurs in the Keuper sandstone as the new mineral Mottramite, but are now in a position to produce a vanadium black for both calico printing and dyeing which is perfectly permanent. This is the more remarkable, as up to this time no aniline black made with copper has been produced in commerce which will withstand the reducing action of sulphurous acid.

As the result of a large number of experiments made with various qualities of commercial aniline, and by varying the strengths of solutions, proportions of aniline and sodium chlorate employed, and also by altering the temperature and the conditions of ageing, Mr. Mellor has found (1) that within certain limits the purer the aniline used, the deeper and more permanent is the black obtained. (2) That there is a maximum density of colour, beyond which if larger proportions of aniline salt and chlorate are used, corresponding advantages of colour are not obtained. This maximum colour is yielded by 16 ounces to 20 ounces of hydrochlorate of aniline per gallon of colour. (3) That for the formation of a permanent black, the amount of aniline salt and sodium chlorate used for 1 gallon of colour must bear a definite relation to each other, the weight of sodium chlorate being about one-half that of the aniline hydrochlorate used. (4) That the permanency of the black depends very much upon the care and skill shown in "ageing" the cloth. If the cloth is aged in a moist atmosphere a blue-black is developed, which is very fleeting; but if aged in a dry air, and at a high temperature, a permanent black is obtained. It is also interesting to learn that for other colours also, the use of vanadium appears to be of value, as in the production of catechu browns as well as in some of the brighter aniline dyes.

It is indeed impossible to say what important technical functions this rare and hitherto unapplied substance may not fulfil. Only the other day vanadium was accounted one of our greatest chemical curiosities, and the investigation of its properties would have been thought, by the practical Englishman, to be a mere waste of time.

Now, however, we have in vanadium a new example of the value of pure scientific research, which must carry conviction even to the most utilitarian of minds.

[H. E. R.]

GENERAL MONTHLY MEETING,

Monday, June 5, 1876.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

General the Lord Sandhurst, G.C.B. G.C.S.I.
Major Henry Collett,
George Alexander Dick, Esq. C.E.
Thomas George Barrett Lennard, Esq.
Sydney McHenry, Esq.
The Rev. Stewart Dixon Stubbs, M.A.

were *elected* Members of the Royal Institution.

Stephen Busk, Esq. was elected Visitor in the room of Mr. Robert P. Linton, deceased.

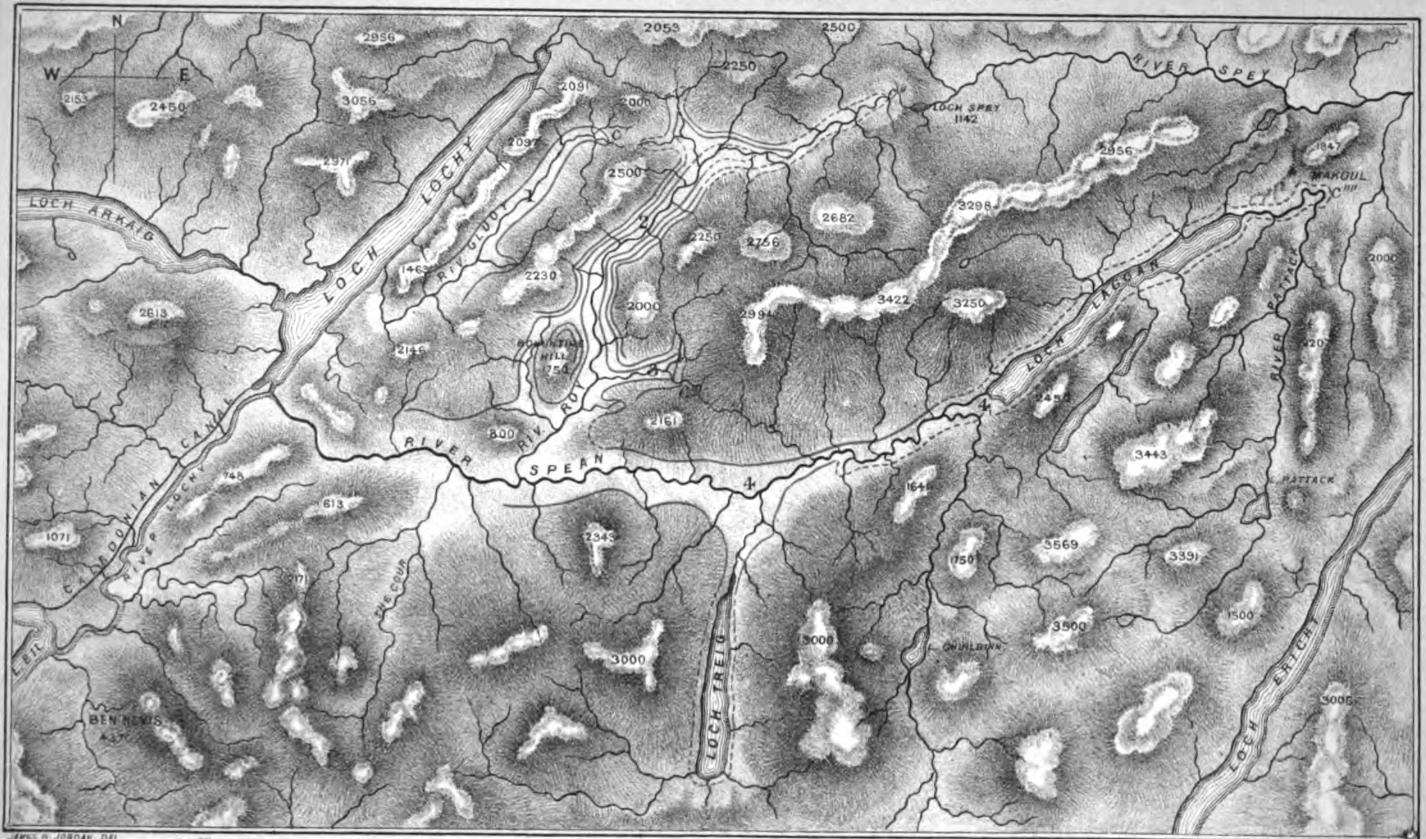
The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Agricultural Society, Royal—Minutes of the Chemical Committee. 8vo. 1876.
Armit, Lieut. R. H. (the Author)—History of New Guinea. (K 101) 8vo. 1876.
Asiatic Society, Royal—Journal, New Series, Vol. VIII. Part 2. 8vo. 1876.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVI. No. 6. 8vo. 1876.
Belgique, Académie Royale des Sciences—Bulletins: 1874–5. 4 vols. 8vo. Annuaire, 1875–6. 12mo.
British Architects, Royal Institute of—Sessional Papers, 1875–6. No. 10. 4to.
Chemical Society—Journal for April, 1876. 8vo.
Crosbie-Dawson, G. J. Esq. (the Author)—Street Pavements. (K 101) 8vo. 1876.
De Candolle, C. (the Author)—Sur la Structure et les Mouvements des Feuilles du Dionæa Muscipula. (Archives des Sciences, Avril, 1876.)
Editors—American Journal of Science for May, 1876. 8vo.
Argonaut for May, 1876. 8vo.
Athenæum for May, 1876. 4to.
Chemical News for May, 1876. 4to.
Electrical News for May, 1876.
Engineer for May, 1876. fol.
Journal for Applied Science for May, 1876. fol.
Nature for May, 1876. 4to.
Nautical Magazine for May, 1876. 8vo.
Pharmaceutical Journal for May, 1876. 8vo.
Telegraph Journal for May, 1876. 8vo.

Franklin Institute—Journal, No. 605. 8vo. 1876.
Geographical Society, Royal—Journal, Vol. XLV. 8vo. 1876.
Geological Society—Quarterly Journal, No. 126. 8vo. 1876.
Hayden, Dr. F. V. United States Geologist—Report of the Geological and Geographical Survey of the Territories. 8vo. 1876.
Lewins, Robert, M.D. (the Author)—Life and Mind. (K 101) 8vo. 1876.
Linnean Society—Journal, Nos. 63, 83. 8vo. 1876.
Longmans and Co. Messrs.—W. N. Hartley; Air and its Relations to Life. 2nd ed. 16to. 1876.
Manchester Geological Society—Transactions, Vol. XIII. Part 11; XIV. Part 3. 8vo. 1876.
Preussische Akademie der Wissenschaften—Monatsberichte: Feb. 1876. 8vo.
Royal Society of London—Proceedings, No. 169. 8vo. 1876.
Royal Society of Tasmania—Monthly Notices for 1874. 8vo. 1875.
Société Hollandaise des Sciences, Haarlem—Notice Historique, &c. 8vo. 1876.
Archives Néerlandaises, Tome X. Liv. 3; Tome XI. Liv. 2, 3. 8vo. 1875–6.
Statistical Society—Journal, Vol. XXXIX. Part 1. 8vo. 1876.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, May, 1876. 8vo.
Tyndall, Professor, F.R.S. (the Author)—Fragments of Science. 5th edition. 12mo. 1876.
Yorkshire Archæological and Topographical Association—Journal: Supplementary Part to Vol. III. 8vo. 1876.

MAP SHEWING THE PARALLEL ROADS OF GLEN ROY.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1. Glen Gluoy – 2. Glen Roy – 3. Glen Glaister – 4. Glen Spean   Parallel roads shewn in red
Heights marked in feet   0 1 2 3 4 5 6 7 8 9 10 Scale of Miles.

WEEKLY EVENING MEETING,

Friday, June 9, 1876.

His Majesty the King of Hanover, K.G. in the Chair.

Professor Tyndall, D.C.L. LL.D. F.R.S. &c.

The Parallel Roads of Glen Roy.

When, after many months of assiduous work, I ventured to bring some of its fruits before you on the 21st of last January, I thought that my tribute to the Friday evening discourses of the present year had been fairly and honestly paid.* But your excellent Honorary Secretary seems to have thought otherwise when he marked me down for the concluding evening of the season. And considering all that our Honorary Secretary does for us here, I should feel ashamed to demur to any arrangement which he might think agreeable to the members, or otherwise conducive to the interests of the Institution. Nevertheless, Friday evening discourses are not to be developed out of consciousness at will, and my friend I fear must accept a portion of the responsibility, if the subject introduced to your attention to-night should appeal to some of you as a twice-told tale.

To some, but not perhaps to all. Once only has the subject been introduced here, in a discourse commended by a great charm of delivery, and a full report of which appears in the 'Proceedings' of this Institution.† To the views ennunciated on that occasion I am unable to subscribe, and it is well that the readers of the 'Proceedings' should know that there are two sides to this question, This is one reason why I chose the subject. Another is that it is not yet considered to be settled, for a new communication regarding it has been recently laid before the Royal Society of Edinburgh by a very meritorious member of that body. Under the circumstances, it can hardly be considered inappropriate on the part of an old student of glacier action to state briefly the side he is disposed to take in the discussion.

The first published allusion to the Parallel Roads of Glen Roy occurs in the appendix to the third volume of Pennant's 'Tour in Scotland,' a work published in 1776. "In the face of these hills," says this writer, "both sides of the glen, there are three roads at small distances from each other and directly opposite on each side. These roads have been measured in the complete parts of them, and found to be 26 paces of a man 5 feet 10 inches high. The two highest are pretty near each other, about 50 yards, and the lowest double that distance from the nearest to it. They are carried along the sides of the glen with the utmost regularity, nearly as exact as drawn with a line of rule and compass."

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* The work then described has been extended and confirmed in various ways since that time.

† Vol. iii. p. 341.

The correct heights of the three roads of Glen Roy are respectively 1150, 1070, and 860 feet above the sea. Hence a vertical distance of 80 feet separates the two highest, while the lowest road is 210 feet below the middle one.

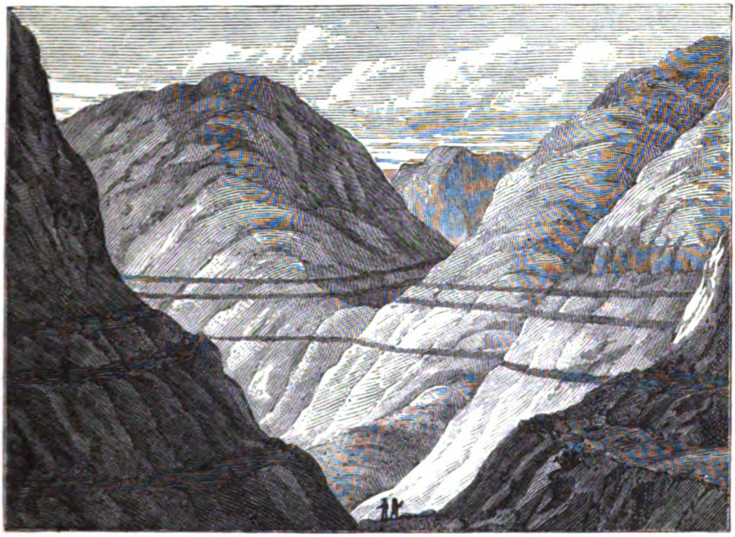
These "roads" are usually shelves or terraces formed in the yielding drift which here covers the slopes of the mountains. They are all sensibly horizontal and therefore parallel. Pennant accepted as reasonable the explanation of them given by the country people, who thought "they were designed for the chase, and that the terraces were made after the spots were cleared in lines from wood, in order to tempt the animals into the open paths after they were rouzed in order that they might come within reach of the bowmen who might conceal themselves in the woods above and below."

In these attempts of "the country people" we have an illustration of that impulse to which all scientific knowledge is due—the desire to know the causes of things; and it is a matter of surprise that in the case of the parallel roads, with their weird appearance challenging inquiry, this impulse did not make itself more rapidly and energetically felt. Their remoteness may perhaps account for the fact that until the year 1817 no systematic description of them, and no scientific attempt at an explanation of them, appeared. In that year Dr. MacCulloch, who was then President of the Geological Society, presented to that Society a memoir, in which the roads were discussed, and regarded as the margins of lakes once embosomed in Glen Roy.

To Dr. MacCulloch succeeded a man, possibly not so learned as a geologist, but obviously fitted by nature to grapple with her facts and to put them in their proper setting. I refer to Sir Thomas Dick-Lauder, who presented to the Royal Society of Edinburgh, on the 2nd of March, 1818, his paper on the Parallel Roads of Glen Roy. In looking over the literature of this subject, which is now copious, it is interesting to observe the differentiation of minds, and to single out those who went by a kind of instinct to the core of the question, from those who erred in it, or who learnedly occupied themselves with its analogies, adjuncts, and details. There is no man, in my opinion, connected with the history of the subject, who has shown, in relation to it, this spirit of penetration, this force of scientific insight, more conspicuously than Sir Thomas Dick-Lauder. Two distinct mental processes are involved in its treatment. Firstly, the faithful and sufficient observation of the data; and secondly, that higher mental process in which the constructive imagination comes into play, connecting the separate facts of observation with their common cause, and weaving them into an organic whole. In neither of these requirements did Sir Thomas Dick-Lauder fail.

Adjacent to Glen Roy is a valley called Glen Gluoy, along the sides of which ran a single shelf, or terrace, formed obviously in the same manner as the parallel roads of Glen Roy. The two shelves on the opposing sides of the glen were at precisely the same level, and Dick-Lauder wished to see whether, and how, they became united at the head of the glen. He followed the shelves into the recesses of the mountains. The bottom of the valley, as it rose, came ever nearer to them, until finally, at the head of Glen Gluoy, he reached a col, or watershed, of precisely the same elevation as the road which swept round the glen.

The correct height of this col is 1170 feet above the sea. It is therefore 20 feet above the highest road in Glen Roy.



Parallel Roads of Glen Roy.
After a Sketch by Sir Thomas Dick-Lauder.

From this col a lateral branch-valley led towards Glen Roy. Our explorer descended from the col to the highest road in that glen, and pursued it exactly as he had pursued the road in Glen Gluoy. For a time it belted the mountain sides at a considerable height above the bottom of the valley; but this rose as he proceeded, coming ever nearer to the highest shelf, until finally he reached a col, or watershed, looking into Glen Spey, and of precisely the same elevation as the highest parallel road of Glen Roy.

He then dropped down to the lowest of these roads, and followed it towards the mouth of the glen. Its elevation above the bottom of the valley gradually increased; not because it rose, but because it remained level while the valley sloped downwards. He found this lowest road doubling round the hills at the mouth of Glen Roy, and running along the sides of the mountains which flank Glen Spean. He followed it eastwards. The Spean Valley, like the others, gradually rose, and therefore gradually approached the road on the adjacent mountain-side. He came to Loch Laggan, the surface of which rose almost to the level of the road, and beyond the head of this lake he found, as in the other two cases, a col, or watershed, of exactly the same level as the single road in Glen Spean, which, it will be remembered, is a continuation of the lowest road in Glen Roy.

Here we have a series of facts of obvious significance as regards the solution of this question. The effort of the mind to form a coherent image from such facts, might be compared with the effort of the eyes to cause the pictures of the stereoscope to coalesce. For a time we exercise a certain strain, the object remaining vague and indistinct. Suddenly its various parts seem to run together, the object starting forth in clear and definite relief. Such, I take it, was the effect of his ponderings upon the mind of Sir Thomas Dick-Lauder. His solution was this: Taking all their features into account, he was convinced that water only could have produced the terraces. He saw clearly that, supposing the mouth of Glen Gluoy to be stopped by a barrier, if the water from the mountains flanking the glen were allowed to collect, it would form behind the barrier a lake, the surface of which would gradually rise until it reached the level of the col at the head of the glen. The rising would then cease; the superfluous water of Glen Gluoy discharging itself over the col into Glen Roy. As long as the barrier stopping the mouth of Glen Gluoy continued, we should have in that glen a lake at the precise level of its shelf, which lake, acting upon the loose drift of the flanking mountains, would actually form the shelf revealed by observation.

So much for Glen Gluoy. But suppose the mouth of Glen Roy also stopped by a barrier sufficiently high. Behind it, the water from the adjacent mountains would collect. The surface of the lake thus formed would gradually rise, until it had reached the level of the col which divides Glen Roy from Glen Spey. Here the rising of the lake would cease; its superabundant water being poured over the col into the valley of the Spey. This state of things would continue as long as the barrier remained at the mouth of Glen Roy. The lake thus dammed in, with its surface at the level of the highest parallel road, would act, as in Glen Gluoy, upon the friable drift overspreading the mountains, and would form the highest road or terrace of Glen Roy.

And now let us suppose the barrier to be so far removed from the mouth of Glen Roy as to establish a connection between it and the upper part of Glen Spean, while the lower part of the latter glen continued blocked up. Upper Glen Spean and Glen Roy would then be occupied by a continuous lake, the level of which would obviously be determined by the col at the head of Loch Laggan. The water in Glen Roy would sink from the level it had previously maintained, to the level of its new place of escape. This new lake-surface would correspond exactly with the lowest parallel road, and it would form that road by its action upon the drift of the adjacent mountains.

In presence of the observed facts, this solution commends itself strongly to the scientific mind. The question next occurs, What was the character of the assumed barrier which stopped the glens? There are at the present moment vast masses of detritus in certain portions of Glen Spean, and of such detritus Sir Thomas Dick-Lauder imagined his barriers to have been formed. By some unknown convulsion, this detritus had been heaped up. But, once given, and once granted that it was subsequently removed, the single road of Glen Gluoy and the highest and lowest roads of Glen Roy would be explained in a satisfactory manner.

To account for the second or middle road of Glen Roy, Sir Thomas Dick-Lauder invoked a new agency. He supposed that at a certain point in the breaking down or waste of his dam, a halt occurred, the barrier holding its ground at a particular level sufficiently long to dam a lake rising to the height of, and forming the second road. This point of weakness was at once detected by Mr. Darwin, and adduced by him as proving that the levels of the cols did not constitute an essential feature in the phenomena of the parallel roads. Though not destroyed, Sir Thomas Dick-Lauder's theory was seriously shaken by this argument, and it became a point of capital importance, if the facts permitted, to remove such source of weakness. This was done in 1847 by Mr. David Milne, now Mr. Milne-Home. On walking up Glen Roy from Roy Bridge, we pass the mouth of a lateral glen, called Glen Glaster, running eastward from Glen Roy. There is nothing in this lateral glen to attract attention, or to suggest that it could have any conspicuous influence in the production of the parallel roads. Hence, I think, the failure of Sir Thomas Dick-Lauder to notice it. But Mr. Milne-Home entered this glen, on the northern side of which the middle and lowest roads are fairly shown. The principal stream running through the glen turns at a certain point northwards and loses itself among hills too high to offer any outlet. But another branch of the glen turns to the south-east; and, following up this branch, Mr. Milne-Home reached a col, or watershed, of the precise level of the second Glen Roy Road. When the barrier blocking the glens had been so far removed as to open this col, the water in Glen Roy would sink to the level of the second road. A new lake of diminished depth would be thus formed, the surplus water of which would escape over the Glen Glaster col into Glen Spean. The margin of this new lake, acting upon the detrital matter, would form the second road. The theory of Sir Thomas

Dick-Lauder, as regards the part played by the cols, was re-riveted by this new and unexpected discovery.

I have referred to Mr. Darwin, whose powerful mind swayed for a time the convictions of the scientific world in relation to this question. His notion was—and it is a notion which very naturally presents itself—that the parallel roads were formed by the sea; that this whole region was once submerged and subsequently upheaved; that there were pauses in the process of upheaval, during which these glens constituted so many fiords, on the sides of which the parallel terraces were formed. This theory will not bear close criticism; nor is it now maintained by Mr. Darwin himself. It would not account for the sea being 20 feet higher in Glen Gluoy than in Glen Roy. It would not account for the absence of the second and third Glen Roy roads from Glen Gluoy, where the mountain flanks are quite as impressionable as in Glen Roy. It would not account for the absence of the shelves from the other mountains in the neighbourhood, all of which would have been clasped by the sea had the sea been there. Here then, and no doubt elsewhere, Mr. Darwin has shown himself to be fallible; but here, as elsewhere, he has shown himself equal to that discipline of surrender to evidence which girds his intellect with unassailable moral strength.

But, granting the significance of Sir Thomas Dick-Lauder's facts, and the reasonableness, on the whole, of the views which he has founded on them, they will not bear examination in detail. No such barriers of detritus as he assumed could have existed without leaving traces behind them; but there is no trace left. There is detritus enough in Glen Spean, but not where it is wanted. The two highest parallel roads stop abruptly at different points near the mouth of Glen Roy, but no remnant of the barrier against which they abutted is to be seen. It might be urged that the subsequent invasion of the valley by glaciers has swept the detritus away; but there have been no glaciers in these valleys since the retreat of the lakes. Professor Geikie has favoured me with a drawing of the Glen Spean shelf near the entrance to Glen Trieg. The shelf forms a belt round a great mound of detritus which, had a glacier followed the formation of the shelf, must have been cleared away. Taking all the circumstances into account, you may, I think, with safety dismiss the detrital barrier as incompetent to account for the present condition of Glen Gluoy and Glen Roy.

Hypotheses in science, though apparently transcending experience, are in reality experience modified by scientific thought and pushed into an ultra experiential region. At the time that he wrote, Sir Thomas Dick-Lauder could not possibly have assigned the cause subsequently assigned for the blockage of these glens. A knowledge of the action of ancient glaciers was the necessary antecedent to the new explanation, and experience of this nature was not possessed by the distinguished writer just mentioned. The extension of Swiss glaciers far beyond their present limits, was first made known by a Swiss engineer named Venetz, who established, by the marks they had left behind, their former existence in places which they had long forsaken. The subject of glacier extension was subsequently followed up with distinguished success by Charpentier, Studer, and others. Agassiz grappled with it with characteristic vigour, extending his evidences far beyond the domain of Switzerland. He came to this country in 1840, and found in various places indubitable marks of ancient glacier action. England, Scotland, Wales, and Ireland he proved to have once given birth to glaciers. He visited Glen Roy, surveyed the surrounding neighbourhood, and pronounced, as a consequence of his investigation, the barriers which stopped the glens and produced the parallel roads to have been barriers of ice. To Mr. Jamieson, above all others, we are indebted for the thorough testing and confirmation of this theory.

And let me here say that Agassiz is only too likely to be misrated and misjudged by those who fail to grasp in their totality the motive powers invoked in scientific research. He lacked mechanical precision, but he abounded in that force and freshness of the scientific imagination which in some sciences, and probably in some stages of all sciences, are essential to the creator of knowledge. To Agassiz was given, not the art of the refiner, but the instinct of the discoverer, and the strength of the delver who brings ore from the recesses of the mine. That ore may contain its share of dross, but it also contains the precious metal which gives employment to the refiner, and without which his occupation would depart.

Let us dwell for a moment upon this subject of ancient glaciers. Under a flask containing water, in which a thermometer is immersed, is placed a Bunsen's lamp. The water is heated, reaches a temperature of 212°, and then begins to boil. The rise of the thermometer then ceases, although heat continues to be poured by the lamp into the water. What becomes of that heat? We know that it is consumed in the molecular work of vaporization. In the experiment here arranged, the steam passes from the flask through a tube into a second vessel kept at a low temperature. Here it is condensed, and indeed congealed to ice, the second vessel being plunged in a mixture cold enough to freeze the water. As a result of the process we obtain a mass of ice. That ice has an origin very antithetical to its own character. Though cold, it is the child of heat. If we removed the Bunsen lamp, there would be no steam, and if there were no steam there would be no ice. The mere cold of the mixture surrounding the second vessel would not produce ice. The cold must have the proper material to work upon; and this material—aqueous vapour—is, as we here see, the direct product of heat.

It is now, I suppose, fifteen or sixteen years since I found myself conversing with an illustrious philosopher regarding that glacial epoch which the researches of Agassiz and others had revealed. This profoundly thoughtful man was of opinion that, at a certain stage in the history of the solar system, the sun's radiation had suffered diminution, the glacial epoch being a consequence of this central chill. The celebrated French mathematician Poisson had another theory. Astronomers have shown that the solar system moves through space, and the temperature of space is a familiar conception with scientific men. It was considered probable by Poisson that our system, during its motion, had traversed portions of space of different temperatures; and that, during its passage through one of the colder regions of the universe, the glacial epoch occurred. Notions such as these were more or less current not many years ago, and I therefore thought it worth while to show how incomplete they were. Suppose the temperature of our planet to be reduced, by the subsidence of solar heat, the cold of space, or any other cause, say one hundred degrees. Four-and-twenty hours of such a chill would bring down as snow nearly all the moisture of our atmosphere. But this would not produce a glacial epoch. Such an epoch would require the continuous generation of the material from which the ice of glaciers is derived. Mountain snow, the nutriment of glaciers, is derived from aqueous vapour raised mainly from the tropical ocean by the sun. The solar fire is as necessary a factor in the process as our Bunsen lamp in the experiment referred to a moment ago. Nothing is easier than to calculate the exact amount of heat expended by the sun in the production of a glacier. It would, as I have elsewhere shown,* raise a quantity of cast iron five times the weight of the glacier not only to a white heat, but to its point of fusion. If, as I have urged elsewhere, instead of being filled with ice, the valleys of the Alps were filled with white-hot metal, of quintuple the mass of the present glaciers, it is the heat, and not the cold, that would arrest our attention and solicit our explanation. The process of glacier making is obviously one of distillation, in which the fire of the sun which generates the vapour plays as essential a part as the cold of the mountains which condenses it.†

It was their ascription to glacier action that first gave the parallel roads of Glen Roy an interest in my eyes; and in 1867, with a view to self-instruction, I made a solitary pilgrimage to the place, and explored pretty thoroughly the roads of the principal glen. I traced the highest road to the col dividing Glen Roy from Glen Spey, and, thanks to the civility of an Ordnance surveyor, I was enabled to inspect some of the roads with a theodolite. As stated by Pennant, the width of the roads amounts sometimes to more than twenty yards; but near the head of Glen Roy the highest road ceases to have any width, for it runs along the face of a rock, the effect of the lapping of the water on the more friable portions of the rock being perfectly distinct to this hour. My knowledge of the region was, however, far from complete, and nine years had dimmed the memory even of the portion which I had thoroughly examined. Hence my desire to see the roads once more before venturing to talk to you about them. The Easter holidays were to be devoted to this purpose; but at the last moment a telegram from Roy Bridge informed me that the roads were snowed up. I was thus thrown back upon books and memories; but these proving only a poor substitute for the flavour of facts, I resolved subsequently to make another effort to see the roads. Accordingly on Thursday fortnight, after lecturing here, I packed up, and started (not this time alone) for the North. Next day at noon we found ourselves at Dalwhinnie, whence a drive of some five-and-thirty miles brought us to the excellent hostelry of Mr. Macintosh, at the mouth of Glen Roy.

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* 'Heat a Mode of Motion,' fifth edition, chap. vi.: Forms of Water, §§ 55 and 56.

† In Lyell's excellent 'Principles of Geology,' the remark occurs that "several writers have fallen into the strange error of supposing that the glacial period must have been one of higher mean temperature than usual." The really strange error was the forgetfulness of the fact that in the production of glaciers, heat played quite as important a part as cold.

We might have found the hills covered with mist, which would have wholly defeated us; but Nature was good-natured, and we had two successful working days among the hills. Guided by the excellent Ordnance map, on the Saturday morning we went up the glen, and on reaching the stream called Allt Bhreac Achaidh faced the hills to the west. At the watershed between Glen Roy and Glen Fintaig we bore northwards, struck the ridge above Glen Gluoy, came in view of its road, which we persistently followed as long as it continued visible. It is a feature of all the roads that they vanish before reaching the cols over which fell the waters of the lakes which formed them. One reason doubtless is that at their upper ends the lakes were shallow, and incompetent on this account to raise wavelets of any strength to act upon the mountain drift. A second reason is that they were land-locked in the higher portions and protected from the south-westerly winds, the stillness of their waters causing them to produce but a feeble impression upon the mountain sides. From Glen Gluoy we passed down Glen Turrit to Glen Roy, and through it homewards, thus accomplishing two or three and twenty miles of rough and honest work.

Next day we thoroughly explored Glen Glaster, following its two roads as far as they were visible. We reached the col discovered by Mr. Milne-Home, and which stands at the level of the middle road of Glen Roy. Thence we crossed southwards over the mountain *Creag Dhubh*, and examined the erratic blocks upon its sides, and the ridges and mounds of moraine matter which cumber the lower flanks of the mountain. The observations of Mr. Jamieson upon this region, including the mouth of Glen Trieg, are in the highest degree interesting. We entered Glen Spean, and continued a search begun on the evening of our arrival at Roy Bridge—the search, namely, for glacier polishings and markings. We did not find them copious, but they are indubitable. One of the proofs most convenient for reference, is a great rounded rock by the road side, 1000 yards east of the milestone marked three-quarters of a mile from Roy Bridge.

Farther east other cases occur, and they leave no doubt upon the mind that Glen Spean was at one time filled by a great glacier. To the disciplined eye the aspect of the mountains is perfectly conclusive on this point; and in no position can the observer more readily and thoroughly convince himself of this than at the head of Glen Glaster. The dominant hills here are all intensely glaciated.

But the great collecting ground of the glaciers which dammed the glens and produced the parallel roads, were the mountains south and west of Glen Spean. The monarch of these is Ben Nevis, 4370 feet high. The position of Ben Nevis and his colleagues, in reference to the vapour-laden winds of the Atlantic, is a point of the first importance. It is exactly similar to that of Carrantual and the Macgillicuddy Reeks in the south-west of Ireland. These mountains are, and were, the first to encounter the south-western Atlantic winds, and the precipitation, even at present, in the neighbourhood of Killarney, is enormous. The winds, robbed of their vapour, and charged with the heat set free by its precipitation, pursue their direction obliquely across Ireland; and the effect of the drying process may be understood by comparing the rainfall at Cahirciveen with that at Portarlington. As found by Dr. Lloyd, the ratio is as 59 to 21—fifty-nine inches annually at Cahirciveen to twenty-one at Portarlington. During the glacial epoch this vapour fell as snow, and the consequence was a system of glaciers which have left traces and evidences of the most impressive character in the region of the Killarney Lakes. I have referred in other places to the great glacier which, descending from the Reeks, moved through the Black Valley, took possession of the lake-basins, and left its traces on every rock and island emergent from the waters of the upper lake. They are all conspicuously glaciated. Not in Switzerland itself do we find clearer traces of ancient glacier action.

What the Macgillicuddy Reeks did in Ireland, Ben Nevis and the adjacent mountains did, and continue to do, in Scotland. We had an example of this on the morning we quitted Roy Bridge. From the bridge westward rain fell copiously, and the roads were wet; but the precipitation ceased near Loch Laggan, whence eastward the roads were dry. Measured by the gauge, the rainfall at Fort William is 86 inches, while at Laggan it is only 46 inches annually. The difference between west and east is forcibly brought out by observations at the two ends of the Caledonian Canal. Fort William at the south-western end has, as just stated, 86 inches, while Culloden, at its north-east end, has only 24. To the researches of that able and accomplished meteorologist, Mr. Buchan, we are indebted for these and other data of the most interesting and valuable kind.

Adhering to the facts now presented to us, it is not difficult to restore in idea the process by which the glaciers of Lochaber were produced and the glens dammed by ice. When the cold of the glacial epoch began to invade the Scottish hills, the sun at the same time acting with sufficient power upon the tropical ocean, the vapours raised and drifted on to these northern mountains were more and more converted into snow. This slid down the slopes, and from every valley, strath, and corry south of Glen Spean, glaciers were poured into that glen. The two great factors here brought into play are the nutrition of the glaciers by the frozen material above, and their consumption in the milder air below. For a period supply exceeded consumption, and the ice extended, filling Glen Spean to an ever-increasing height, and abutting against the mountains to the north of that glen. But why, it may be asked, should the valleys south of Glen Spean be receptacles of ice at a time when those north of it were receptacles of water? The answer is to be found in the position and the greater elevation of the mountains south of Glen Spean. They first received the loads of moisture carried by the Atlantic winds, and not until they had been in part dried, and warmed by the liberation of their latent heat, did these winds touch the hills north of the Glen.

An instructive observation bearing upon this point is here to be noted. Had our visit been in the winter we should have found all the mountains covered; had it been in the summer we should have found the snow all gone. But happily it was at a season when the aspect of the mountains north and south of Glen Spean exhibited their relative powers as snow collectors. Scanning the former hills from many points of view, we were hardly able to detect a fleck of snow, while heavy swaths and patches loaded the latter. Were the glacial epoch to return, the relation indicated by this observation would cause Glen Spean to be filled with glaciers from the south, while the hills and valleys on the north, visited by milder and drier winds, would remain comparatively free from ice. This flow from the south would be reinforced from the west, and as long as the supply was in excess of the consumption the glaciers would extend, the dams closing the glens increasing in height. By-and-by supply and consumption becoming approximately equal, the height of the glacier barriers would remain constant. Then, as milder weather set in, consumption would be in excess, and a retreat of the ice would be the consequence. But for a long time the conflict between supply and consumption would continue, retarding indefinitely the disappearance of the barriers, and keeping the imprisoned lakes in the northern glens. But however slow its retreat, the ice in the long run would be forced to yield. The dam at the mouth of Glen Roy, which probably entered the glen sufficiently far to block up Glen Glaster, would gradually retreat. Glen Glaster and its col being opened, the subsidence of the lake 80 feet, from the level of the highest to that of the second parallel road, would follow as a consequence. I think this the most probable course of things, but it is also possible that Glen Glaster may have been blocked by a glacier from Glen Trieg. The ice dam continuing to retreat, at length permitted Glen Roy to connect itself with upper Glen Spean. A continuous lake then filled both glens, the level of which, as already explained, was determined by the col at Makul, above the head of Loch Laggan. The last to yield was the portion of the glacier which derived nutrition from Ben Nevis, and probably also from the mountains north and south of Loch Arkaig. But it at length yielded, and the waters in the glens resumed the courses which they pursue to-day.

For the removal of the ice barriers no cataclysm is to be invoked; the gradual melting of the dam would produce the entire series of phenomena. In sinking from col to col the water would flow over a melting barrier, the surface of the imprisoned lake not remaining sufficiently long at any particular level to produce a shelf comparable to the parallel roads. By temporary halts in the process of melting due to atmospheric conditions or to the character of the dam itself, or through local softness in the drift, small pseudo-terraces would be formed which, to the perplexity of some observers, are seen upon the flanks of the glens to-day.

In presence then of the fact that the barriers which stopped these glens to a height, it may be, of 1500 feet above the bottom of Glen Spean, have dissolved and left not a wreck behind; in presence of the fact, insisted on by Professor Geikie, that barriers of detritus would undoubtedly have been able to maintain themselves had they ever been there; in presence of the fact that great glaciers once most certainly filled these valleys—that the whole region, as proved by Mr. Jamieson, is filled with the traces of their action; the theory which ascribes the parallel roads to lakes dammed by barriers of ice has, in my opinion, an amount of probability on its side which amounts to a practical demonstration of its truth.

Into the details of the terrace formation I do not enter. Mr. Darwin and Mr. Jamieson on the one side, and Sir John Lubbock on the other, deal with true causes. The terraces, no doubt, are due in part to the descending drift arrested by the water, and in part to the fretting of the wavelets, and the rearrangement of the stirred detritus, along the belts of contact of lake and hill. The descent of matter must have been frequent when the drift was unbound by the rootlets which hold it together now. In some cases, it may be remarked, the visibility of the roads is materially exalted by differences of vegetation. The grass upon the terraces is not always of the same character as that above and below them, while on heather-covered hills the absence of the dark shrub from the roads greatly enhances their conspicuousness.

Reviewing our work, we find three considerable steps to have marked the solution of the problem of the Parallel Roads of Glen Roy. The first of these was taken by Sir Thomas Dick-Lauder, the second was the pregnant conception of Agassiz regarding glacier action, and the third was the testing and verification of this conception by the very thorough researches of Mr. Jamieson.* To these may be added the important observation of Mr. Milne-Home in Glen Glaster; with other remarks and reflections scattered through the literature of the subject, or suggested by the latest visit to the spot.

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* No circumstance, or incident, connected with this discourse gives me greater pleasure than the recognition of the value of these researches. They are marked throughout by unflagging industry, by novelty and acuteness of observation, and

Thus ends our rapid survey of this brief episode in the physical history of the Scottish hills,—brief, that is to say, in comparison with the immeasurable lapses of time through which, to produce its varied structure and appearances, our planet must have passed. In the survey of such a field two things are specially worthy to be taken into account—the widening of the intellectual horizon and the reaction of expanding knowledge upon the intellectual organ itself. At first, as in the case of ancient glaciers, through sheer want of capacity, the mind refuses to take in revealed facts. But by degrees the steady contemplation of these facts so strengthens and expands the intellectual powers, that where truth once could not find an entrance it eventually finds a home.

[The formation, connection, successive subsidence, and final disappearance of the glacial lakes of Lochaber were illustrated in the discourse here reported, by a model constructed under the supervision of my assistant, Mr. John Cottrell. Glen Gluoy with its lake and road and the cataract over its col; Glen Roy and its three roads with their respective cataracts at the head of Glen Spey, Glen Glaster, and Glen Spean, were all represented. The successive shiftings of the barriers, which were formed of plate glass, brought each successive lake and its corresponding road into view, while the entire removal of the barriers caused the streams to flow down the glens of the model as they flow down the real glens of to-day. A map of the district, with the parallel roads shown in red, is annexed.]

[J. T.]

LITERATURE OF THE SUBJECT.

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THOMAS LAUDER DICK (afterwards **SIR THOMAS DICK-LAUDER**, Bart.).—On the Parallel Roads of Lochaber. Edin. Roy. Soc. Trans. 1818, vol. ix. p. 1.

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by reasoning power of a high and varied kind. These pages had been returned "for press" when I learned that the relation of Ben Nevis and his colleagues to the vapour-laden winds of the Atlantic had not escaped Mr. Jamieson. To him obviously the exploration of Lochaber, and the development of the theory of the Parallel Roads, has been a labour of love.

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Robert Chambers.—Ancient Sea Margins. Edinburgh, 1848.
H. D. Rogers.—On the Parallel Roads of Glen Roy. Royal Inst. Proceedings, 1861, vol. iii. p. 341.
Thomas F. Jamieson.—On the Parallel Roads of Glen Roy, and their Place in the History of the Glacial Period. Quart. Journal Geol. Soc. 1863, vol. xix. p. 235.
Sir Charles Lyell.—Antiquity of Man. 1863, p. 253.
Rev. R. Boog Watson.—On the Marine Origin of the Parallel Roads of Glen Roy. Quart. Journ. Geol. Soc. 1865, vol. xxii. p. 9.
Sir John Lubbock.—On the Parallel Roads of Glen Roy. Quart. Journ. Geol. Soc. 1867, vol. xxiv. p. 83.
Charles Babbage.—Observations on the Parallel Roads of Glen Roy. Quart. Journ. Geol. Soc. 1868, vol. xxiv. p. 273.
James Nicol.—On the Origin of the Parallel Roads of Glen Roy. 1869. Geol. Soc. Journal, vol. xxv. p. 282.
James Nicol.—How the Parallel Roads of Glen Roy were formed. 1872. Geol. Soc. Journal, vol. xxviii. p. 237.
Major-General Sir Henry James, R.E.—Notes on the Parallel Roads of Lochaber. 4to. 1874.

GENERAL MONTHLY MEETING,

Monday, July 3, 1876.

Sir T. Frederick Elliot, K.C.M.G. Vice-President, in the Chair.

Casimir de Candolle, Esq.
Lewis Lœffler, Esq.
Charles Heneage, Esq. F.R.G.S.
The Rev. John Eade Pryor, M.A.
W. Shore Smith, Esq.
John L. Walker, Esq.

were *elected* Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

American Academy of Arts and Sciences—Proceedings, Vol. XI. 8vo. 1876.
Antiquaries, Society of—Proceedings, Second Series, Vol. VI. No. 65. 8vo. 1876.
Armit, Lieut. R. H. R.N. (the Author)—History of New Guinea, &c. (K 101) 8vo. 1876.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVI. No. 7. 8vo. 1876.
Bombay Branch of the Royal Asiatic Society—Journal, Vol. XI. 8vo. 1876.
British Architects, Royal Institute of—Sessional Papers, 1875–6, No. 13. 4to.

Cernuschi, M. H. (the Author)—M. Michel Chevalier et le Bimétallisme. 8vo. 1876. (Two copies.)
Chemical Society—Journal for May, 1876. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vol. XLIII. 8vo. 1876.
Crosbie-Dawson, G. J. Esq. (the Author)—Street Pavements. (K 101) 8vo. 1876.
De Laveleye, M. E. (the Author)—La Monnaie Bimétallique. (L 16) 8vo. 1876.
Dutton, Francis S. Esq. C.M.S. M.R.I.—W. Harcus: South Australia: its History, Resources, and Productions. 8vo. 1876.
Editors—American Journal of Science for June, 1876. 8vo.
Argonaut for June, 1876. 8vo.
Athenæum for June, 1876. 4to.
Chemical News for June, 1876. 4to.
Electrical News for June, 1876.
Engineer for June, 1876. fol.
Journal for Applied Science for June, 1876. fol.
Nature for June, 1876. 4to.
Nautical Magazine for June, 1876. 8vo.
Pharmaceutical Journal for June, 1876. 8vo.
Telegraph Journal for June, 1876. 8vo.
Franklin Institute—Journal, No. 606. 8vo. 1876.
Geographical Society, Royal—Proceedings, Vol. XX. No. 4. 8vo. 1876.
Gladstone, Professor J. H. F.R.S.—The Argonaut, Vol. III. 8vo. 1876.
Hayden, Dr. F. O. (United States Geologist)—Report of the Geological and Geographical Survey of the Territories. 8vo. 1876.
Photographic Society—Journal, Nos. 267, 268. 8vo. 1876.
Preussische Akademie der Wissenschaften—Monatsberichte: Marz, 1876. 8vo.
Sands, Admiral B. F. (the Superintendent)—Washington Astronomical and Meteorological Observations, 1873. 4to. 1875.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, June, 1876. 8vo.
British Rainfall, 1875. 8vo. 1876.
Victoria Institute—Journal, No. 37. 8vo. 1876.
Zoological Society of London—Transactions, Vol. IX. Part 8. 4to. 1876.
Proceedings for 1876, Part 1. 8vo. 1876.

GENERAL MONTHLY MEETING,

Monday, November 6, 1876.

Admiral Sir Henry John Codrington, K.C.B. Manager, in the Chair.

Robert James Mann, M.D.
John Ralph Shaw, Esq.

were *elected* members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

French Government—Documents Inédits sur l'Histoire de France:
Négociations avec la Toscane: Tome V. 4to. 1875.
Inscriptions de la France: Tome II. 4to. 1875.
Recueil des Diplômes Militaires. Ed. L. Renier. 4to. 1876.
Lettres Missives de Henri IV.: Tome IX. Supplément. 4to. 1876.
Brazilian Government—Empire of Brazil at the Universal Exhibition of 1876. Philadelphia. 8vo. 1876.
H. de Mello: Subsidies to the Formation of a Physical Map of Brazil (with Maps).
The Hon. Hamilton Fish (*Sec. State U.S.*)—Reports of the Commissioners of the United States to the International Exhibition, Vienna, 1873. 4 vols. 8vo. 1876.
New Zealand Government—Statistics of New Zealand for 1875. fol. 1876.

Agricultural Society of England, Royal—Journal, No. 24. 8vo. 1876.
Asiatic Society of Bengal—Journal, 1875, Part II. No. 3. 8vo.
Proceedings, 1876. Nos. 1, 2. 8vo.
Asiatic Society, Royal, Bombay Branch—Journal, No. 33. 8vo. 1876.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVI. Nos. 8, 9. 8vo. 1876.
Bavarian Academy of Sciences, Royal—Abhandlungen, Band XII. Abth 2. 4to. 1876.
Sitzungberichte, 1875, Heft 3. 1876, Heft 1. 8vo. 1876.
Boston Society of Natural History—Memoirs, Vol. II. Part 4. Nos. 2, 3, 4. 4to. 1875–6.
Proceedings, Vol. XVII. Parts 3, 4. Vol. XVIII. Parts 1, 2. 1875–6.
Occasional Papers, II. N. M. Hentz: Spiders of the United States. 8vo. 1875.
British Architects, Royal Institute of—Sessional Papers, 1875–6, Nos. 11, 12. 4to.
British Association for the Advancement of Science—Report of the 45th Meeting: Bristol, August, 1875. 8vo. 1876.
Cernuschi, M. H. (*the Author*)—Silver Vindicated. 8vo. 1876.
Chemical Society—Journal for June–Oct., 1876. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vols. XLIV. XLV. XLVI. 8vo. 1876.

Connecticut Academy of Arts and Sciences—Transactions, Vol. III. Part 1. 8vo. 1876.

Devonshire Association for the Advancement of Literature, Science, and Art—Report and Transactions, Vol. VIII. 8vo. 1876.

Dublin Society, Royal—Journal, Nos. 42, 43. 8vo. 1874–5.

Editors—American Journal of Science for July–Oct. 1876. 8vo.
Argonaut for July–Oct. 1876. 8vo.
Athenæum for July–Oct. 1876. 4to.
Chemical News for July–Oct. 1876. 4to.
Engineer for July–Oct. 1876. fol.
Horological Journal for July–Oct. 1876. 8vo.
Journal for Applied Science for July–Oct. 1876. fol.
Nature for July–Oct. 1876. 4to.
Nautical Magazine for July–Oct. 1876. 8vo.
Pharmaceutical Journal for July–Oct. 1876. 8vo.
Quarterly Journal of Sciences for July and Oct. 1876. 8vo.
Telegraph Journal for July–Oct. 1876. 8vo.

Franklin Institute—Journal, Nos. 607, 608, 609, 610. 8vo. 1876.

Geographical Society, Royal—Proceedings, Vol. XX. Nos. 5, 6. 8vo. 1876.

Geological Institute, Imperial, Vienna—Jahrbuch, 1876. No. 2. 8vo. 1876.
Verhandlungen, 1876. Nos. 7–10. 8vo. 1876.

Geological Society—Journal, No. 127. 8vo. 1876.

Geological Survey of India—Memoirs, Vol. XI. Part 2. 8vo. 1875.
Records, Vol. IX. Parts 1, 2, 3. 8vo. 1876.
Palæontologia Indica: Jurassic Fauna of Kutch. Vol. I. No. 4. 1875.

Glasgow Philosophical Society—Proceedings, Vol. X. No. 1. 8vo. 1876.

Heywood, James, Esq. F.R.I. M.R.I. (the Editor)—Professor Heer: Primæval World of Switzerland. 2 vols. 8vo. 1876.

Hull Literary and Philosophical Society—Report, 1875–6. 8vo. 1876.

Iron and Steel Institute—Journal, 1876, No. 1. 8vo. 1876.

Jackson, L. D'A. Esq. M.R.I. (the Author)—Simplified Weights and Measures. 8vo. 1876.

Linnean Society—Transactions: Second Series: Botany, Vol. I. Part 3; Zoology, Vol. I. Part 3. 4to. 1876.
First Series: General Index, Vols. XXVI.–XXX. 4to. 1876.
Proceedings, Nos. 64, 65; 84, 85, 86. 8vo. 1876.

Manchester Geological Society—Transactions, Vol. XIV. Part 4. 8vo. 1876.

Manchester Literary and Philosophical Society—Memoirs. 3rd series. Vol. V 1876.
Proceedings, Vol. XV. 8vo. 1876.
Catalogue of Library. 8vo. 1875.

Mechanical Engineers' Institution—Proceedings, May, 1876. 8vo.

Medical and Chirurgical Society, Royal—Proceedings, Part 43. 8vo. 1876.

Meteorological Office—Report of Meteorological Committee of the Royal Society for 1875. 8vo. 1876.
Meteorology of Japan. 4to. 1876.

Meteorological Office of Canada—Reports of Observatories of Canada, 1875. 8vo.

Meteorological Society—Quarterly Journal, No. 19. 1876.
Catalogue of Library. 8vo. 1876.

Musical Association—Proceedings: Second Session. 1875–6. 8vo.

Norfolk and Norwich Naturalists' Society—Transactions. 1869–76. 2 vols. 8vo.

North of England Institute of Engineers—Transactions, Vol. XIX.–XXIV. 8vo. 1869–75.

Philadelphia Academy of Natural Sciences—Proceedings, 1875. 8vo.

Photographic Society—Journal, Nos. 267, 268. 8vo. 1876.

Preussische Akademie der Wissenschaften—Monatsberichte: April–July, 1876. 8vo.

Royal Society of Literature—Transactions, Vol. XI. Part 2. 8vo. 1876.

Royal Society of London—Proceedings, Nos. 170, 171, 172, 173. 8vo. 1876.
Philosophical Transactions, Vol. CLXV. Part 2. 4to. 1876.
Saxon Society of Sciences, Royal—Abhandlungen: Band XV. Nos. 7, 8, 9: 1874. Band XVI. No. 6: 1874. Band XVII. Nos. 2, 3, 4; 1874–6. Band XVIII. Nos. 1–5. 8vo. 1874–5.
Berichte; Phil. Hist. Classe: 1873, 1874: 1875, No. 1. 8vo.
Math. Phys. Classe: 1873, Nos. 3, 4, 5, 6. 1874. 1875, No. 1.
Statistical Society—Journal, Vol. XXXIX. Parts 2, 3. 8vo. 1876.
Stockholm, Royal Academy of Sciences—Handlingar, Bandet XI. and Atlas. 1872–5. Bihang, Bandet III. Häfte 1. 8vo. 1875.
Öfversigt, 1875. 8vo.
St. Petersburg, Académie des Sciences—Tableau Générale des Matières contenues dans ses Publications. Partie 1. 8vo. 1872.
Bulletins, Tome XX. Nos. 3, 4; Tome XXI. Tome XXII. Nos. 1, 2. 4to. 1875–6.
Mémoires. 7^e Série. Tome XXII. Nos. 4–10; Tome XXIII. No. 1. 1875.
Repertorium für Meteorologie. Band V. Heft 1. 4to. 1876.
St. Petersburg Central Physical Observatory—Annalen, 1874. 4to. 1876.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, July–Oct. 1876. 8vo.
Tremaux, M. P. (the Author)—Principe Universel du Mouvement, &c. 3rd ed. 16°. Paris, 1876.
Tyndall, Professor, D.C.L. F.R.S. M.R.I. (the Author)—Lessons in Electricity. 8vo. 1876.
United Service Institution, Royal—Journal, Nos. 84, 86, 87. 8vo. 1876.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen. Marz, April, Mai, Juni, Juli, 1876. 4to.
Victoria Institute—Journal, No. 38. 8vo. 1876.
Canon Birks: On the Uncertainties of Modern Physical Science. 8vo. 1876.
Vincent, B. Libr. R.I. (the Editor)—Haydn's Dictionary of Dates. Fifteenth edition. (Two Copies.) 8vo. 1876.
Yorkshire Archæological and Topographical Association—Journal, Part 15. 8vo. 1876.
Yorkshire Philosophical Society—Report for 1875. 8vo. 1876.
Young, James, Esq. and Dr. R. Angus Smith (Editor)—T. Graham: Chemical and Physical Researches. (Privately printed.) 1876.
Zoological Society—Transactions, Vol. IX. Part 9. 4to. 1876.
Proceedings, 1876, Parts 2, 3. 8vo.

GENERAL MONTHLY MEETING,

Monday, December 4, 1876.

Sir T. Frederick Elliot, K.C.M.G. Vice-President,
in the Chair.

Major J. F. D. Donnelly, R.E.
Mrs. L. E. J. Elwes,
Edward Herries, Esq. C.B.
Frederick John Horniman, Esq. F.L.S. F.R.G.S. F.Z.S. &c.
Samuel Joshua, Esq.
William Cunliffe Pickersgill, Esq.
Evan Wynne Roberts, Esq.
Henry Bowden Smith, Esq.
Sydney Thompson, Esq.
Mrs. Tyndall,

were *elected* Members of the Royal Institution.

The Special Thanks of the Members were given to Mr. William Edward Kilburn for his Presents of a large Thermopile, a Uranium Vacuum Tube, and Optical Apparatus for producing Lissajous' Figures.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Asiatic Society of Bengal—Journal, 1876, Part I. No. 1. Part II. Nos. 1, 2. 8vo.
Proceedings, 1876. Nos. 3–7. 8vo.
Bradlee, Rev. C. D. (the Author)—The Teachings of the Mountains: a Sermon. (K 101) 8vo. 1876.
British Architects, Royal Institute of—Sessional Papers, 1876–7: No. 1. 4to.
British Museum Trustees—Fac Similes of Egyptian Hieratic Papyrus of the Reign of Rameses III. fol. 1876.
Catalogue of Oriental Coins, Vol. II. 8vo. 1876.
Catalogue of Greek Coins (Sicily). 8vo. 1876.
Catalogue of Sanskrit and Pali Books. 4to. 1876.
Fac Similes of Ancient Charters. Part II. fol. 1876.
Catalogue of Fossil Reptilia of South Africa. (Illustrated.) 4to. 1876.
Chemical Society—Journal for Nov. 1876. 8vo.
Clinical Society—Transactions, Vol. IX. 8vo. 1876.
Cornwall Polytechnic Society—Forty-third Report, 1875. 8vo.
Cracroft, Bernard, Esq. M.R.I. (the Author)—Trustees' Guide. 12th ed. 4to. 1876.
Letters. 8vo. 1876.

Editors—American Journal of Science for Nov. 1876. 8vo.
Argonaut for Nov. 1876. 8vo.
Athenæum for Nov. 1876. 4to.
Chemical News for Nov. 1876. 4to.
Electrical News for Nov. 1876.
Engineer for Nov. 1875. fol.
Horological Journal for Nov. 1876. 8vo.
Journal for Applied Science for Nov. 1876. fol.
Nature for Nov. 1875. 4to.
Nautical Magazine for Nov. 1876. 8vo.
Pharmaceutical Journal for Nov. 1876. 8vo.
Quarterly Journal of Sciences for Nov. 1876. 8vo.
Telegraphic Journal for Nov. 1876. 8vo.
Franklin Institute—Journal, No. 611. 8vo. 1876.
Geological Society—Quarterly Journal, No. 128. 8vo. 1876.
Halliwell-Phillipps, J.O. Esq. (the Author)—Illustrations of the Life of Shakespeare, Part I. fol. 1874.
Jablonowski'sche Gesellschaft, Leipsic—Preisschriften, XIX. XX. 4to. 1876.
Leeds Philosophical Society—Report for 1875–6. 8vo.
Liverpool Literary and Philosophical Society—Proceedings, No. 30. 8vo. 1876.
Mechanical Engineers' Institution—Proceedings, July, 1876. 8vo.
Medical and Chirurgical Society, Royal—Transactions, Vol. LIX. 8vo. 1876.
Meteorological Society—Quarterly Journal, No. 20. 8vo. 1876.
Photographic Society—Journal, New Series, Nos. 1, 2. 8vo. 1876.
Preussische Akademie der Wissenschaften—Monatsberichte: Aug. 1876. 8vo.
Royal Society of London—Proceedings, No. 174. 8vo. 1876.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, Nov. 1876. 8vo.
Victoria Institute—Journal, No. 39. 8vo. 1875.
Warden of the Standards—Tenth Annual Report. 8vo. 1876.

The following arrangements of the Lectures before Easter, 1877, were announced:—

Professor John Hall Gladstone, Ph.D. F.R.S.—Six Lectures adapted to a Juvenile Auditory, on the Chemistry of Fire; on Dec. 28 (Thursday), 30, 1876; Jan. 2, 4, 6, 9, 1877.

Professor Alfred H. Garrod, M.A. F.R.S.—Ten Lectures on the Human Form; its Structure in relation to its Contour; on Tuesdays, Jan. 16 to March 20.

Dr. C. R. Alder-Wright, F.C.S.—Four Lectures on Metals and the Chief Industrial Uses of these Bodies and their Compounds; on Thursdays, Jan. 18 to Feb. 8.

William Pole, Esq. F.R.S. Mus.Doc.—Six Lectures on the Theory of Music; on Thursdays, Feb. 15 to March 22.

Ernst Pauer, Esq.—Two Lectures on the Nature of Music: the Italian, French, and German Schools; on Saturdays, Jan. 20, 27.

J. A. Symonds, Esq.—Three Lectures on Florence and the Medici; on Saturdays, Feb. 3 to 17.

Professor Henry Morley.—Five Lectures on Effects of the French Revolution upon English Literature; on Saturdays Feb. 24, to March 24.

Professor Tyndall will give a Course of Lectures after Easter.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 19, 1877.

Sir W. Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Professor Tyndall, D.C.L. LL.D. F.R.S.

A Combat with an Infective Atmosphere.

[Abstract Deferred.]

S OF ENGLAND

WEEKLY EVENING MEETING,

Friday, January 26, 1877.

William Spottiswoode, LL.D. Tr.R.S. Secretary and Vice-President, in the Chair.

Sir John Lubbock, Bart. M.P. D.C.L. F.R.S. *M.R.I.*

The Habits of Ants.

The Anthropoid apes no doubt approach nearer to man in bodily structure than do any other animals; but when we consider the habits of ants, their social organisation, their large communities, elaborate habitations, their roadways, their possession of domestic animals, and even, in some cases, of slaves, it must be admitted that they have a fair claim to rank next to man in the scale of intelligence. They present, moreover, not only a most interesting but also a very extensive field of study. In this country we have nearly thirty species; but ants become more numerous, in species as well as individuals, in warmer countries, and more than seven hundred kinds are known. Even this large number certainly is far short of those actually in existence.

I have kept in captivity nearly half of our British species of ants, and at the present moment have in my room more than thirty nests, belonging to about twenty species, some of which, however, are not English. No two species are identical in habits; and, on various accounts, their mode of life is far from easy to unravel. In the first place, most of their time is passed underground: all the education of the young, for instance, is carried on in the dark. Again, ants are essentially gregarious; it is in some cases difficult to keep a few alive by themselves in captivity, and at any rate their habits under such circumstances are entirely altered. If, on the other hand, a whole community is kept, then the greater number introduces a fresh element of difficulty and complexity. Moreover, within the same species, the individuals seem to differ in character, and even the same individual will behave very differently under different circumstances. Although, then, ants have attracted the attention of many naturalists, Gould, De Geer, Swammerdam, Latreille, Leuwenhoeck, Huber, and have recently been the object of interesting observations by Frederick Smith, Belt, Moggridge, Bates, Mayr, Emery, Forel, and others, they still present one of the most promising fields for observation and experiment.

The larvæ of ants, like those of bees and wasps, are small, white, legless grubs, somewhat conical in form, being narrow towards the head. They are carefully tended and fed, being carried about from chamber to chamber by the workers, probably in order to secure the most suitable amount of warmth and moisture. I have observed also that they are very often sorted according to age. It is sometimes very curious in my nests to see them divided into groups according to size, so that they remind one of a school divided into five or six classes. When full grown they turn into pupæ, sometimes naked, sometimes covered with a silken cocoon, constituting the so-called "ant-eggs." After remaining some days in this state, they emerge as perfect insects. In many cases, however, they would perish in the attempt, if they were not assisted; and it is very pretty to see the older ants helping them to extricate themselves, carefully unfolding their legs and smoothing out the wings, with truly feminine tenderness and delicacy.

Under ordinary circumstances an ants' nest, like a beehive, consists of three kinds of individuals: workers, or imperfect females (which constitute the great majority), males, and perfect females. There are, however, often several females in an ants' nest; while, as we all know, there is never more than one queen in a hive. The queens have wings, but after a single flight they tear off their own wings, and do not again quit the nest. In addition to the ordinary workers there is in some species a second, or rather a third, form of female. In almost any ants' nest we may see that the workers differ more or less in size. The amount of difference, however, depends upon the species. In *Lasius niger*, the small brown garden ant, the workers are, for instance, much more uniform than in the little yellow meadow ant, or in *Atta barbara*, where some of them are more than twice as large as others. But in certain ants there are differences still more remarkable. Thus, in a Mexican species, besides the common workers, which have the form of ordinary neuter ants, there are certain others in which the abdomen is swollen into an immense sub-diaphanous sphere. These individuals are very inactive, and principally occupied in elaborating a kind of honey.* In the genus *Pheidole*, very common in southern Europe, there are also two distinct forms without any intermediate gradations; one with heads of the usual proportion, and a second with immense heads provided with very large jaws. These latter are generally supposed to act as soldiers, and the size of the head enables the muscles which move the jaws to be of unusual dimensions, though the little ones are also very pugnacious. This differentiation of certain individuals so as to adapt them to special functions seems to me very remarkable; for it must be remembered that the difference is not one of age or sex.

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* Westwood, 'Modern Class. of Insects,' vol. ii. p. 225.

The food of ants consists of insects, great numbers of which they destroy; of honey, honeydew, and fruit; indeed, scarcely any animal or sweet substance comes amiss to them. Some species, such, for instance, as the small brown garden ant, ascend bushes in search of aphides. The ant then taps the aphis gently with her antennæ, and the aphis emits a drop of sweet fluid, which the ant drinks. Sometimes the ants even build covered ways up to and over the aphides, which, moreover, they protect from the attacks of other insects. Our English ants do not collect provision for the winter; indeed, their food is not of a nature which would admit of this. Some southern species, however, collect grain, occasionally in considerable quantities. Moreover, though our English ants cannot be said exactly to lay up stores, some at least do take steps to provide themselves with food in the future. The small yellow meadow ant (*Lasius flavus*), for instance, lives principally on the honeydew of certain aphides which suck the roots of grass. The ants collect the aphides in the nest, not only watching over them themselves, but, as I have been able to satisfy myself, even over their eggs, an act which one is much tempted to refer to forethought, and which in such a case implies a degree of prudence superior to that of some savages. Besides these aphides, many other insects live in ants' nests. If they are to be regarded as domestic animals, then ants have more domestic animals than we have. The majority of these ant guests are beetles. Some of them, as, for instance, the curious little *Claviger*, are quite blind, and are only found in ants' nests, the ant taking just as much care of them as of their own young. It is evident, therefore, that in some way they are useful or agreeable to the ants. The subject, however, is one as yet but little understood, and very difficult to study. Grimm and Lespés consider that some of these beetles secrete a sweet fluid like the aphides, and from analogy this seems probable. Other creatures which habitually live in ants' nests, like the little *Beckia albinos*, or the blind woodlouse (*Platyarthrus*), perhaps make themselves useful as scavengers.

Nor are ants without their enemies. In addition to birds and other larger foes, if you disturb a nest of the brown ants at any time during the summer you will probably see some very small flies hovering over them, and every now and then making a dash at some particular ant. These flies belong to the genus *Phora*, and to a species hitherto unnamed, which Mr. Verrall has been good enough to describe for me. They lay their eggs on the ants, inside which the larvæ live. Other species of the genus are in the same way parasitic on bees. On the 14th of October last I observed that one of my ants had a mite attached to the underside of its head. The mite, which is still in the same position, is almost as large as the head. The ant cannot remove it herself. She has never come out of the nest, so that I could not do it for her, and none of her own companions from that day to this have thought of performing this kind office.

In character the different species of ants differ very much from one another. *F. fusca*, the one which is pre-eminently the enslaved ant, is, as might be expected, extremely timid; while the nearly allied *F. cinerea* has, on the contrary, a considerable amount of individual audacity. *F. rufa*, the horse ant, according to M. Forel, is especially characterised by the want of individual initiative, and always moves in troops; he also regards the genus *Formica* as the most brilliant, though some others excel it in other respects, as, for instance, in the sharpness of their senses. *F. pratensis* worries its slain enemies; *F. sanguinea* never does. The slave-making ant (*P. rufescens*) is, perhaps, the bravest of all. If a single individual finds herself surrounded by enemies, she never attempts to fly, as any other ant would, but transfixes her opponents one after another, springing right and left with great agility, till at length she succumbs, overpowered by numbers. *M. scabrinodis* is cowardly and thievish; during wars among the larger species they haunt the battle-fields and devour the dead. *Tetramorium* is said to be very greedy; *Myrmecina* very phlegmatic.

In industry ants are not surpassed even by bees and wasps. They work all day, and in warm weather, if need be, even at night too. I once watched an ant from six in the morning, and she worked without intermission till a quarter to ten at night. I had put her to a saucer containing larvæ, and in this time she carried off no less than a hundred and eighty-seven to the nest. I once had another ant, which I employed in my experiments, under observation several days. When I came up to London in the morning, and went to bed at night, I used to put her in a small bottle, but the moment she was let out she began to work again. On one occasion I was away from home for a week. On my return I let her out of the bottle, placing her on a little heap of larvæ about three feet from the nest. Under these circumstances I certainly did not expect her to return. However, though she had thus been six days in confinement, the brave little creature immediately picked up a larva, carried it off to the nest, and after half an hour's rest returned for another.

We have hitherto very little information as to the length of life in ants. So far, indeed, as the preparatory stages are concerned, there is little difficulty in approximately ascertaining the facts; namely, that while they take only a few weeks in summer, in some species, as our small yellow meadow ants, the autumn larvæ remain with comparatively little change throughout the winter. It is much more difficult to ascertain the length of life of the perfect insect, on account of their gregarious habits, and the difficulty of recognising individual ants. It has, however, generally been supposed that they live about a season, and this is probably the case; but I have still some workers of *F. cinerea*, which I captured at Castellamare, in November, 1875, and some of *F. sanguinea* and *F. fusca* since September in that year. They must now, therefore, be at least a year and a half old. I have also some queens of *F. fusca* which have been with me since December, 1874, and still seem in perfect health. If they lived much longer, and could compare their experiences, ants would, from their immense numbers, even in temperate regions, contend with mankind on no such very unequal terms.

The behaviour of ants to one another differs very much according as they are alone or supported by numerous companions. An ant which would run away in the first case, will fight bravely in the second.

It is hardly necessary to say that, as a general rule, each species lives by itself. There are, however, some interesting exceptions. The little *Stenamma Westwoodii* is found exclusively in the nests of the much larger *F. rufa* and the allied *F. pratensis.* We do not know what the relations between the two species are. The *Stenammas*, however, follow the *Formicas* when they change their nest, running about among them and between their legs, tapping them inquisitively with their antennæ, and even sometimes climbing on to their backs, as if for a ride, while the large ants seem to take little notice of them. They almost seem to be the dogs, or rather cats, of the ants. Another small species, *Solenopsis fugax*, which makes its chambers and galleries in the walls of the nests of larger species, is the bitter enemy of its hosts. The latter cannot get at them, because they are too large to enter the galleries. The little *Solenopsis*, therefore, are quite safe, and, as it appears, make incursions into the nurseries of the larger ant, and carry off the larvæ as food. It is as if we had small dwarfs, about eighteen inches to two feet long, harbouring in the walls of our houses, and every now and then carrying off some of our children into their horrid dens.

Most ants, indeed, will carry off the larvæ and pupæ of others if they get a chance; and this explains, or at any rate throws some light upon, that most remarkable phenomenon, the existence of slavery among ants. If you place a number of larvæ and pupæ in front of a nest of the horse ant, for instance, they are soon carried off; and those which are not immediately required for food remain alive for some days, though I have never been able to satisfy myself whether they are fed by their captors. Both the horse ant and the slave ant (*F. fusca*) are abundant species, and it must not unfrequently occur that the former, being pressed for food, attack the latter and carry off some of their larvæ and pupæ. Under these circumstances it occasionally happens that the pupæ come to maturity in the nests of the horse ant, and nests are sometimes, though rarely, found in which, with the legitimate owners, there are a few *F. fuscas.* With the horse ant this is, however, a very rare and exceptional phenomenon; but with an allied species, *F. sanguinea*, a species which exists in our southern counties and throughout Europe, it has become an established habit. The *F. sanguineas* make periodical expeditions, attack neighbouring nests of *F. fusca*, and carry off the pupæ. When the latter come to maturity, they find themselves in a nest consisting partly of *F. sanguineas*, partly of *F. fuscas*, the results of previous expeditions. They adapt themselves to circumstances, assist in the ordinary household duties, and, having no young of their own species, feed and tend those of the *F. sanguineas*. But though the *F. sanguineas* are thus aided by the *F. fuscas*, they have not themselves lost the instinct of working. It seems not improbable that there is some division of functions between the two species, but we have as yet no distinct knowledge on this point; and at any rate the *F. sanguineas* can "do" for themselves, and carry on a nest, if necessary, without slaves.

In another species, however, *Polyergus rufescens*, which is not British, this is not the case. They present a striking lesson of the degrading tendency of slavery, for they have become entirely dependent on their slaves. Even their bodily structure has undergone a change: their mandibles have lost their teeth, and have become mere nippers, deadly weapons indeed, but useless except in war. They have lost the greater part of their instincts: their art, that is, the power of building; their domestic habits, for they take no care of their own young, all this being done by the slaves; their industry—they take no part in providing the daily supplies; if the colony changes the situation of its nest, the masters are all carried by the slaves to the new one; nay, they have even lost the habit of feeding. Huber placed thirty of them with some larvæ and pupæ and a supply of honey in a box.

> "At first," he says, "they appeared to pay some little attention to the larvæ; they carried them here and there, but presently replaced them. More than one-half of the Amazons died of hunger in less than two days. They had not even traced out a dwelling, and the few ants still in existence were languid and without strength. I commiserated their condition, and gave them one of their black companions. This individual, unassisted, established order, formed a chamber in the earth, gathered together the larvæ, extricated several young ants that were ready to quit the condition of pupæ, and preserved the life of the remaining Amazons." *

This observation has been fully confirmed by other naturalists. However small the prison, however large the quantity of food, these stupid creatures will starve in the midst of plenty rather than feed themselves. I have had a nest of this species under observation for a long time, but never saw one of the masters feeding. I have kept isolated specimens for weeks by giving them a slave for an hour or two a day to clean and feed them, and under these circumstances they remained in perfect health, while, but for the slaves, they would have perished in two or three days. I know no other case in nature of a species having lost the instinct of feeding.

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* Huber, 'Natural History of Ants.'

In *P. rufescens*, the so-called workers, though thus helpless and stupid, are numerous, energetic, and in some respects even brilliant. In another slave-making species, however, *Strongylognathus*, the workers are much less numerous, and so weak that it is an unsolved problem how they contrive to make slaves.

Lastly, in a fourth species, *Anergates atratulus*, the workers are absent, the males and females living in nests with workers belonging to another ant, *Tetramorium cæspitum*. In these cases the *Tetramoriums*, having no queen, and consequently no young of their own, tend the young of the *Anergates*. It is therefore a case analogous to that of *Polyergus*, but it is one in which slave-owning has almost degenerated into parasitism. It is not, however, a case of true parasitism, because the *Tetramoriums* take great care of the *Anergates*, and if the nest is disturbed, carry them off to a place of safety.

M. Forel, in his excellent work on ants, has pointed out that very young ants devote themselves at first to the care of the larvæ and pupæ, and that they take no share in the defence of the nest or other out-of-door work until they are some days old. This seems natural, because at first their skin is comparatively soft; and it would clearly be undesirable to undertake rough work or run into danger until their armour had had time to harden. There are, however, reasons for thinking that the division of labour is carried still further. I do not allude merely to those cases in which there are completely different kinds of workers, but even to the ordinary workers. In *L. flavus*, for instance, it seems probable that the duties of the small workers are somewhat different from those of the large ones, though no such division of labour has yet been detected. In *F. fusca* I made an observation which surprised me very much. In the autumn of 1875 I noticed an ant out feeding alone. The next day the same ant was out by herself, and I could easily recognise her because by some accident she had lost the claws of one of her hind feet. My attention being roused, I watched the nest for some weeks, and saw this same ant out repeatedly, but no other. This winter I have kept two nests under close observation, that is, I arranged with my daughters and their governess, Miss Wendland, most conscientious observers, that we should look at the nest once every hour throughout the day, and this has been done since the middle of November, with a few exceptions not enough to affect the conclusion. The former nest contains about two hundred, the second about four hundred individuals; but as they are somewhat torpid, and there are no larvæ to be fed, much food is not required. In each case only two or three individuals came out for food, each about twice a day, though some days they did not come out at all. Thinking that possibly these specimens were unusually voracious, or in some other way abnormal, I imprisoned the foragers belonging to one of the nests. The following day two others came out for food, and continued coming for several days. I then imprisoned them also, when two others came out, showing, I think, that the community requires food, and that it was the function of certain individuals to obtain it.

One of the most interesting problems about ants is, of course, to determine the amount of their intelligence. In order to test this, it seemed to me that one way would be to ascertain some object which they would clearly desire, and then to interpose some obstacle which a little ingenuity would enable them to overcome. With this object in view, I placed food in a porcelain cup on a slip of glass surrounded by water, but accessible to the ants by a bridge, consisting of a strip of paper two-thirds of an inch long and one-third wide. Having then put a *F. nigra* from one of my nests to this food, she began carrying it off, and by degrees a number of friends came to help her. I then, when about twenty-five ants were so engaged, moved the little paper bridge slightly, so as to leave a chasm just so wide that the ants could not reach across. They came to the edge and tried hard to get over, but it did not occur to them to push the paper bridge, though the distance was only about one-third of an inch, and they might easily have done so. After trying for about a quarter of an hour they gave up the attempt, and returned home. This I repeated several times. Then, thinking that paper was a substance to which they were not accustomed, I tried the same with a bit of straw one inch long and one-eighth of an inch wide. The result was the same. I repeated this twice. Again I placed particles of food close to and directly over the nest, but connected with it only by a passage several feet in length. Under these circumstances it would be obviously a saving of time and labour to drop the food on to the nest, or at any rate to spring down with it, so as to save one journey. But though I have frequently tried the experiment, my ants never adopted either of these courses. I arranged matters so that the glass on which the food was placed was only raised one-third of an inch above the nest. The ants tried to reach down, and the distance was so small that occasionally, if another ant passed underneath just as one was reaching down, the upper one could step on to its back, and so descend; but this only happened accidentally, and they did not think of throwing the particles down, nor, which surprised me very much, would they jump down themselves. I then placed a heap of fine mould close to the glass, but just so far that they could still not reach across. It would have been of course quite easy for any ant, by moving a particle of earth for a quarter of an inch, to have made a bridge by which the food might have been reached, but this simple expedient did not occur to them. On the other hand, I then put some provisions in a shallow box with a glass top, and a single hole on one side, and put some specimens of *Lasius niger* to the food. As soon as a stream of ants was at work, busily carrying supplies off to the nest, and when they had got to know the way thoroughly, I poured some fine mould in front of the hole so as to cover it up to a depth of about half an inch. I then took out the ants which were actually in the box. As soon as they had recovered from the shock of this unexpected proceeding on my part, they began to run all round and about the box, looking for some other place of entrance. Finding none, however, they began digging down into the earth just over the hole, carrying off the grains of earth one by one, and depositing them, without any order, all round at a distance of from half an inch to six inches until they had excavated down to the doorway, when they again began carrying off the food as before. This experiment I repeated on following days three or four times, always with the same result.

As evidence both of their intelligence and of their affection for their friends, it has been said by various observers that when ants have been accidentally buried they have been very soon dug out and rescued by their companions. Without for a moment doubting the facts as stated, we must remember the habit which ants have of burrowing in loose fresh soil, and especially their practice of digging out fresh galleries when their nests are disturbed. It seemed to me, however, that it would not be difficult to test whether the excavations made by ants under the circumstances, were the result of this general habit, or really due to a desire to extricate their friends. With this view I tried (20th August) the following experiments. I placed some honey near a nest of *Lasius niger* on a glass surrounded with water, and so arranged that in reaching it the ants passed over another glass covered with a layer of sifted earth about one third of an inch in thickness. I then put some ant to the honey, and by degrees a considerable number collected round it. Then, at 1.30 P.M., I buried an ant from the same nest under the earth, and left her there till 5 P.M., when I uncovered her. She was none the worse, but during the whole time not one of her friends had taken the least notice of her.

Again, September 1st, I arranged some honey in the same way. At 5 P.M. about fifty ants were at the honey, and a considerable number were passing to and fro. I then buried an ant as before, of course taking one from the same nest. At 7 P.M. the number of ants at the honey had nearly doubled. At 10 P.M. they were still more numerous, and had carried off about two-thirds of the honey. At 7 A.M. the next morning the honey was all gone; two or three ants were still wandering about, but no notice had been taken of the prisoner, whom I then let out. In this case I allowed the honey to be finished, because I thought it might perhaps be alleged that the excitement produced by such a treasure distracted their attention; or even, on the principle of doing the greatest good to the greatest number, that they were intelligently wise in securing a treasure of food before they rescued their comrade, who, though in confinement, was neither in pain nor danger. So far as the above ants, however, are concerned, this cannot be urged. I may add that I repeated the same experiment several times, in some cases with another species, *Myrmica ruginodis*, and always with the same results.

Ants have been much praised on account of their affection for their friends. In this respect, however, they seem to vary greatly.

At any rate, anyone who has watched them much must have met with very contradictory facts. I have often put ants which were smeared with a sticky substance on the boards attached to my nests, and very rarely indeed did their companions take any notice of, or seek to disentangle them.

I then tried the following experiment. A number of the small yellow ants (*L. flavus*) were out feeding on some honey. I took five of them, and also five others of the same species, but from a different nest, chloroformed them, and put them close to the honey, and on the path which the ants took in going to and from the nest, so that these could not but see them. The glass on which the honey was placed was surrounded by a moat of water. This, then, gave me an opportunity of testing both how far they would be disposed to assist a helpless fellow-creature, and what difference they would make between their nest companions and strangers from a different community. The chloroformed ants were put down at ten in the morning. For more than an hour, though many ants came up and touched them with their antennæ, none of them did more. At length one of the strangers was picked up, carried to the edge of the glass, and quietly thrown, or rather dropped, into the water. Shortly afterwards a friend was taken up and treated in the same way. By degrees they were all picked up and thrown into the water. One of the strangers was, indeed, taken into the nest, but in about half an hour she was brought out again and thrown into the water like the rest. I repeated this experiment with fifty ants, half friends and half strangers. In each case twenty out of the twenty-five ants were thrown into the water as described. A few were left lying where they were placed, and these also, if we had watched longer, would no doubt have been also treated in the same way. One out of the twenty-five friends, and three out of the twenty-five strangers, were carried into the nest, but they were all brought out again and thrown away like the rest. Under such circumstances, then, it seems that ants make no difference between friends and strangers.

It may, however, be said in this experiment, that as ants do not recover from chloroform, and these ants were therefore to all intents and purposes dead, we should not expect that much difference would be made between friends and strangers. I therefore tried the same experiment, only, instead of chloroforming the ants, I made them intoxicated. This was a rather more difficult experiment. No ant would voluntarily degrade herself by getting drunk, and it was not easy in all cases to hit off the requisite degree of this compulsory intoxication. In all cases they were made quite drunk, so that they lay helplessly on their backs. The sober ants seemed much puzzled at finding their friends in this helpless and discreditable condition. They took them up and carried them about for awhile in a sort of aimless way, as if they did not know what to do with their drunkards, any more than we do. Ultimately, however, the results were as follows. The ants removed twenty-five friends and thirty strangers.

Of the friends, twenty were carried into the nest, where no doubt they slept off the effect of the spirit—at least we saw no more of them—and five were thrown into the water. Of the strangers, on the contrary, twenty-four were thrown into the water; only six were taken into the nest, and four of these were shortly afterwards brought out again and thrown away.

The difference in the treatment of friends and strangers was, therefore, most marked.

Dead ants, I may add, are always brought out of the nest, and I have more than once found a little heap on one spot, giving it almost the appearance of a burial ground.

I have also made some experiments on the power possessed by ants of remembering their friends. It will be recollected that Huber gives a most interesting account of the behaviour of some ants, which, after being separated for four months, when brought together again, immediately recognised one another, and "fell to mutual caresses with their antennæ." Forel, however, regards these movements as having indicated fear and surprise rather than affection, though he also is quite inclined to believe, from his own observation, that ants would recognise one another after a separation of some months. The observation recorded by Huber was made casually; and neither he nor anyone else seems to have taken any steps to test it by subsequent experiments. The fact is one, however, of so much interest, that it seemed to me desirable to make further experiments on the subject. On the 4th of August, 1875, therefore, I separated one of my nests of *F. fusca* into two halves, which I kept entirely apart.

I then from time to time put an ant from one of these nests into the other, introducing also a stranger at the same time. The stranger was driven out, or sometimes even killed. The friend, on the contrary, was never attacked, though I am bound to say that I could see no signs of any general welcome, or that she was taken any particular notice of.

I will not trouble you with all the evidence, but will content myself with one case.

On the 12th November last, that is to say, after the ant had been separated for a year and three months, I put a friend and a stranger into one of the divisions. The friend seemed quite at home. One of the ants at once seized the stranger by an antenna, and began dragging her about. At

11.45. The friend is quite at home with the rest. The stranger is being dragged about.

12.0. The friend is all right. Three ants now have hold of the stranger by her legs and an antenna.

12.15. Do. do.

12.30. Do. do.

12.45. Do. do.

1.0. Do. do.

1.30. Do. One now took hold of the friend, but soon seemed to find out her mistake and left go again.
1.45. The friend is all right. The stranger is being attacked. The friend also has been almost cleaned; while on the stranger the colour has been scarcely touched.
2.15. Two ants are licking the friend, while another pair is holding the stranger by her legs.
2.30. The friend is now almost clean, so that I could only just perceive any colour. The stranger, on the contrary, is almost as much coloured as ever. She is now near the door, and I think would have come out, but two ants met her and seized her.
3.0. Two ants are attacking the stranger. The friend was no longer distinguishable from the rest.
3.30. Do.
4.0. Do.
5.0. Do.
6.0. The stranger now escaped from the nest, and I put her back among her own friends.

The difference of behaviour to these two ants was most marked. The friend was gradually licked clean, and except for a few moments, and that evidently by mistake, was never attacked. The stranger, on the contrary, was not cleaned, was at once seized, was dragged about for hours with only a few minutes' interval, by one, two, or three assailants, and at length made her escape from the nest at a time when no other ant was out.

In most species of ants the power of smell is very keen. I placed ants on a strip of paper, each end of which was supported on a pin, the foot of which was immersed in water. They then ran backwards and forwards along the paper, trying to escape. If a camel's-hair pencil be suspended just over the paper, they pass under it without taking any notice of it; but if it be scented, say with lavender-water, they at once stop when they come near it, showing in the most unmistakable manner that they perceive the odour. This sense appears to reside, though not perhaps exclusively, in the antennæ. I tethered, for instance, a large specimen of *Formica ligniperda* with a fine thread to a board, and when she was quite quiet I approached a scented camel's-hair pencil slowly to the tip of the antenna, which was at once withdrawn, though the antenna took no notice of a similar pencil, if not scented.

On the other hand, as regards their sense of hearing, the case is very different. Approaching an ant which was standing quietly, I have over and over again made the loudest and most shrill noises I could, using a penny pipe, a dog-whistle, a violin, as well as the most piercing and startling sounds I could produce with my own voice, without effect. At the same time I by no means would infer from this that they are really deaf, though it certainly seems that their range of hearing is very different from ours. We know that certain allied insects produce a noise by rubbing one of their abdominal rings against another Landois is of opinion that ants also make sounds in the same way, though these sounds are inaudible to us. Our range is, however, after all, very limited, and the universe is probably full of music which we cannot perceive. There are, moreover, in the antennæ of ants certain curious organs which may perhaps be of an auditory character. There are from ten to a dozen in the terminal segment of *Lasius flavus*, the small meadow ant, and indeed in most of the species which I have examined, and one or two in each of the short intermediate segments. These organs consist of three parts: a small spherical cup opening to the outside, a long narrow tube, and a hollow body shaped like an elongated clock-weight. They may serve to increase the resonance of sounds, acting, in fact, to use the words of Professor Tyndall, who was good enough to look at them with me, like microscopic stethoscopes.

The organs of vision are in most ants very complex and conspicuous. There are generally three eyes arranged in a triangle on the top of their heads, and on each side a large compound eye containing sometimes more than two thousand facets between them. Nevertheless the sight of ants does not seem to be very good. In order to test how far ants are guided by vision, I made the following experiments. I placed a common lead pencil on a board, fastening it upright, so as to serve as a landmark. At the base I then placed a glass containing food, and then put a *L. niger* to the food; when she knew her way from the glass to the nest and back again perfectly well, she went quite straight backwards and forwards. I then took an opportunity when the ant was on the glass, and moved the glass with the ant on it about three inches. Now, under such circumstances, if she had been much guided by sight, she could not of course have had any difficulty in finding her way to the nest. As a matter of fact, however, she was entirely at sea, and after wandering about for some time, got back to the nest by another and very roundabout route. I then again varied the experiment as follows. I placed the food in a small china cup on the top of the pencil, which thus formed a column seven and a half inches high. When the ant once knew her way, she went very straight to and from the nest. This puzzled her very much: she went over and over the spot where the pencil had previously stood, retraced her steps several times almost to the nest, and then returned along the whole line, showing great perseverance, if not much power of vision. I then moved the pencil six inches. She found the pencil at last, but only after many meanderings.

I then repeated the observation on three other ants with the same result: the second was seven minutes before she found the pencil, and at last seemed to do so accidentally; the third actually wandered about for no less than half an hour, returning up the paper bridge several times.

Let us compare this relatively to man. An ant measuring say one-sixth of an inch, and the pencil being seven inches high, is consequently forty-two times as long as the ant. It bears, therefore, somewhat the same relation to the ant as a column two hundred and fifty feet high does to a man. The pencil having been moved six inches, it is as if a man in a country he knew well would be puzzled at being moved a few hundred feet, or if put down in a square containing less than an acre, could not find a column two hundred and fifty feet high, that is to say, higher than the Duke of York's column.

Another evidence of this consists in the fact, that if, when my *L. nigers* were carrying off food placed in a cup on a piece of board, I turned the board round so that the side which had been turned towards the nest was away from it, and *vice versâ*, the ants always returned over the same track on the board, and consequently directly away from home. If I moved the board to the other side of my artificial nest, the result was the same. Evidently they followed the road, not the direction.

It is remarkable that we do not even now know exactly how an ants' nest is begun. Whether they always commence as a colony from some older establishment; whether wandering workers who chance to find a queen, under certain circumstances remain with her and begin a new nest; or whether the queen ant, like the queen wasp, forms a cell for herself, and then brings up a few workers, who afterwards take upon themselves the labours of the family, as yet we know not. When once started, the communities last for years, being kept up by a succession of individuals. The queens themselves rarely or never quit the nest, but receive their food from the workers, and indeed appear to do nothing except lay eggs.

A nest of ants must not be confused with an ant hill in the ordinary sense. Very often indeed a nest has only one dwelling, and in most species seldom more than three or four. Some, however, form numerous colonies. M. Forel even found a case in which one nest of *F. exsecta* had no less than two hundred colonies, and occupied a circular space with a radius of nearly two hundred yards. Within this area they had exterminated all the other ants, except a few nests of *Tapinoma erraticum*, which survived, thanks to their great agility. In these cases the number of ants thus associated together must have been enormous. Even in single nests Forel estimates the numbers at from five thousand to half a million.

In their modes of fighting, different species of ants have their several peculiarities. Some also are much less military than others. *Myrmecina Latreillii*, for instance, never attack, and scarcely even defend themselves. Their skin is very hard, and they roll themselves into a ball, not defending themselves even if their nest is invaded, to prevent which, however, they make the entrances small, and often station at each a worker, who uses her head to stop the way. The smell of this species is also, perhaps, a protection. *Tetramorium* *cæspitum* has the habit of feigning death. This species, however, does not roll itself up, but merely applies its legs and antennæ closely to the body.

Formica rufa, the common horse ant, attacks in serried masses, seldom sending out detachments, while single ants scarcely ever make individual attacks. They rarely pursue a flying foe, but give no quarter, killing as many enemies as possible, and never hesitating, with this object, to sacrifice themselves for the common good.

Formica sanguinea, on the contrary, at least in their slave-making expeditions, attempt rather to terrify than to kill. Indeed, when they are invading a nest, they do not attack the flying inhabitants unless they are attempting to carry off pupæ, in which case they are forced to abandon the pupæ. When fighting, they attempt to crush their enemies with their mandibles.

Formica exsecta is a delicate, but very active species. They also advance in serried masses, but in close quarters they bite right and left, dancing about to avoid being bitten themselves. When fighting with larger species they spring on to their backs, and then seize them by the neck or by an antenna. They also have the instinct of combining in small parties, three or four seizing an enemy at once, and then pulling different ways, so that she on her part cannot get at any one of her foes. One of them then jumps on her back and cuts, or rather saws, off her head. In battles between this ant and the much larger *F. pratensis*, many of the latter may be seen each with a little *F. exsecta* on her back, sawing off her head from behind.

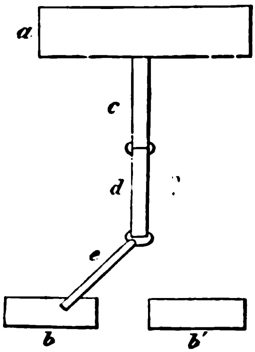
One might, at first sight, be disposed to consider that the ants with stings must have a great advantage over those with none. In some cases, however, the poison is so strong that it is sufficient for it to touch the foes to place them *hors de combat*, or at least to render them incapacitated, with every appearance of extreme pain. Such species have the abdomen unusually mobile.

The species of *Lasius* make up in numbers what they want in strength. Several of them seize an enemy at once, one by each of her legs or antennæ, and when they have once taken hold they will suffer themselves to be cut in pieces rather than leave go.

Polyergus rufescens, the celebrated slave-making or Amazon ant, has a mode of combat almost peculiar to herself. The jaws are very powerful, and pointed. If attacked—if, for instance, another ant seizes her by a leg—she at once takes her enemy's head into her jaws, which generally makes her quit her hold. If she does not, the *Polyergus* closes her mandibles, so that the points pierce the brain of her enemy, paralysing the nervous system. The victim falls in convulsions, setting free her terrible foe. In this manner a comparatively small force of *Polyergus* will fearlessly attack much larger armies of other species, and suffer themselves scarcely any loss.

Much of what has been said as to the powers of communication possessed by bees and ants depends on the fact that if one of them in the course of her rambles has discovered a supply of food, a number of others soon find their way to the store. This, however, does not necessarily imply any power of describing localities. If the bees or ants merely follow their more fortunate companion, or if they hunt her by scent, the matter is comparatively simple; if, on the contrary, the others have the route described to them, the case becomes very different. To determine this, therefore, I have made a great number of experiments, of which, however, I will here only mention a few. Under ordinary circumstances, if an ant discovers a stock of food she carries as much as possible away to the nest, and then returns for more, accompanied generally by several friends. On their return these bring others, and in this way a string of ants is soon established. Unless, therefore, various precautions are taken, and this, so far as I know, has never been done in any of the previous observations, the experiment really tells very little.

I therefore made the following arrangement. One of my nests of the small brown garden ant, *Lasius niger*, was connected with a board, on which I was in the habit of placing a supply of food and water. At a short distance from the board I placed two glasses (*b b′*), and on *b* I placed some food. I then connected the glass *b* with the board *a* by three slips of paper, *c*, *d*, *e*, and put an ant to the food. She carried off a supply to the nest, returning for more, and so on. Several friends came with her, and I imprisoned them till the experiment was over. When she had passed several times over the paper bridges, I proceeded as follows. Any friends who came with her were excluded from the bridges when she was on them. If she was not there, as soon as a friend arrived at the bridge *c*, I took up *e* in my fingers and rubbed it lightly, with a view of removing or blurring the scent; and as soon as the ant arrived on *d* I took up the bridge *c*, and put it across the chasm from *d* to *b′*. Now if the ant went by description, she would of course cross *e* to *b*. If, on the other hand, she went by scent, then she would be at the least as likely to go over *c* to *b′*. The results were that out of about one hundred and twenty friends who passed over *d*, only twenty went to the food, while nearly one hundred passed over *c* to the empty glass. In this case the friends generally came more or less in sight of one another to the bridge *c*, and once there, could hardly avoid arriving either at *b* or *b′*. I therefore modified the experiment as follows. I established and endowed an ant as before, imprisoning the friends who came with her. When she got to know her way thoroughly, I allowed her to return to the nest on her own legs, but as soon as she emerged again I took her up and transferred her to the food.



Under these circumstances, as will be seen, very few ants indeed ever found their way to the food. I began this at 5.30, when she returned to the nest. At 5.34 she came out with no less than ten friends, and was then transferred to the food. The others wandered about a little, but by degrees returned to the nest, not one of them finding her way to the food. The first ant took some food, returned, and again came out of the nest at 5.39 with eight friends, when exactly the same happened. She again came out

At 5.44	with	4	friends.	At 6.44	with	0	friends.
„ 5.47	„	4	„	„ 6.46	„	3	„
„ 5.49	„	1	„	„ 6.49	„	2	„
„ 5.52				„ 6.56			
„ 5.54	„	5	„	„ 6.59			
„ 5.58	„	2	„	„ 7.2	„	2	„
„ 5.59	„	2	„	„ 7.4			
„ 6.1	„	5	„	„ 7.6	„	3	„
„ 6.4	„	1	„	„ 7.8	„	3	„
„ 6.7				„ 7.10	„	5	„
„ 6.11	„	3	„	„ 7.13			
„ 6.14	„	4	„	„ 7.17	„	3	„
„ 6.17	„	6	„	„ 7.19	„	7	„
„ 6.20				„ 7.21	„	5	„
„ 6.23	„	5	„	„ 7.24			
„ 6.25	„	6	„	„ 7.26	„	3	„
„ 6.29	„	8	„	„ 7.29	„	1	„
„ 6.32	„	2	„	„ 7.31	„	2	„
„ 6.35				„ 7.35			
„ 6.42	„	4	„				

(39 journeys; 11 alone, 28 with 120 friends.)

Thus, during these two hours more than one hundred and twenty ants came out of the nest, in company with the one under observation. She knew her way perfectly, and it is clear that if she had been left alone all these ants would have accompanied her to the store of food. Three of them were accidentally allowed to do so, but of the remainder only five found their way to the food; all the others, after wandering about awhile, returned empty-handed to the nest.

I conclude, then, that when large numbers of ants come to food they follow one another, being also to a certain extent guided by scent. The fact, therefore, does not imply any considerable power of intercommunication. There are, moreover, some circumstances which seem to point in an opposite direction. For instance, I have already mentioned that if a colony of *Polyergus* changes the situation of its nest, the masters are all carried to the new one by the slaves. Again, if a number of *F. fusca* are put in a box, and in one corner a dark place of retreat is provided for them with some earth, one soon finds her way to it. She then comes out again, and going up to one of the others, takes her by the jaws. The second ant then rolls herself into a heap, and is carried off to the place of shelter. They then both repeat the same manœuvre with other ants, and so on until all their companions are collected together. Now it seems to me difficult to imagine that so slow a course would be adopted if they possessed any power of communicating description.

On the other hand, they certainly can, I think, transmit simpler ideas. In support of this I may adduce the following experiment. Two strips of paper were attached to the board just mentioned (p. 268), and parallel to one another, and at the other end of each I placed a piece of glass. In the glass at the end of one tape I placed a considerable number (three to six hundred) of larvæ. In the second I put two or three larvæ only. I then took two ants, and placed one of them to the glass with many larvæ, the other to that with two or three. Each of them took a larva and carried it to the nest, returning for another, and so on. After each journey I put another larva in the glass with only two or three larvæ to replace that which had been removed. Now, if other ants came under the above circumstances as a mere matter of accident, or accompanying one another by chance, or if they simply saw the larvæ which were being brought, and consequently concluded that they might themselves also find larvæ in the same place, then the numbers going to the two glasses ought to be approximately equal. In each case the number of journeys made by the ants would be nearly the same; consequently, if it were a matter of smell, the two routes would be in the same condition. It would be impossible for an ant, seeing another in the act of bringing a larva, to judge for itself whether there were few or many larvæ left behind. On the other hand, if the strangers were brought, then it would be curious to see whether more were brought to the glass with many larvæ than to that which only contained two or three. I should also mention that every stranger was imprisoned until the end of the experiment. I will select a few of the results:

Exp. 1. Time occupied, one hour. The ant with few larvæ made 6 visits, and brought no friends. The one with many larvæ made 7, and brought 11 friends.

Exp. 3. Time occupied, three hours. The ant with few larvæ made 24 journeys, and brought 5 friends. The one with many larvæ made 38 journeys, and brought 22 friends.

Exp. 5. Time occupied, one hour. The ant with few larvæ made 10 journeys, and brought 3 friends. The other made 5 journeys, and brought 16 friends.

Exp. 9. Time occupied, one hour. The ant with few larvæ made 11 journeys, and brought one friend. The one with many larvæ made 15 journeys, and brought 13 friends.

Exp. 10. I now reversed the glasses, the same two ants being under observation; but the ant which in the previous observation had few larvæ to carry off now consequently had many, and *vice versâ*. Time occupied, two hours. The ant with few larvæ made 21 journeys, and brought 1 friend. The one with many larvæ made 22 journeys, and brought 20 friends. These two experiments are, I think, especially striking.

Taken as a whole, I found that in about fifty hours the ants which had access to many larvæ brought 257 friends, while those visiting a glass with few larvæ only brought 82. The result will appear still more striking if we remember that a certain number, say perhaps 25, would have come to the larvæ anyhow, which would make the numbers 232 as against 57, a very striking difference.

I have elsewhere discussed the relations of flowers to insects, and especially with bees, and particularly the mode in which the flowers were modified so that the bees might transfer the pollen from one flower to another. Ants are also of considerable importance to plants, especially in keeping down the number of insects which feed on them. So far as I know, however, there are no plants which are specially modified in order to be fertilised by ants; and, indeed, even to those small flowers which any little insect might fertilise, the visits of winged insects are much more advantageous, because, as Mr. Darwin has shown in his excellent work on cross and self-fertilisation of plants, it is important that the pollen should be brought, not only from a different flower, but also from a different plant, while creeping insects, such as ants, would naturally pass from flower to flower of the same plant.

Under these circumstances it is important to plants that ants should not obtain access to the flowers, for they would otherwise rob them of their honey without conferring on them any compensating advantage. Accordingly, we not only find in flowers various modes of attracting bees, but also of excluding ants; and in this way ants have exercised more influence on the vegetable kingdom than might be supposed. Sometimes, for instance, the flowers are protected by *chevaux de frise* of spines and fine hairs pointing downwards (*Carlina*, *Lamium*); some have a number of glands secreting a glutinous substance over which the ants cannot pass (*Linnæa*, gooseberry); in others the tube of the flower is itself very narrow, or is almost closed either by hairs or by internal ridges, which just leave space for the proboscis of a bee, but no more. Lastly, some, and especially pendulous flowers (*Cyclamen*, snowdrop), are so smooth and slippery that ants cannot easily enter them, but often slip off in the attempt, and thus are excluded, just as the pendulous nests of the weaver-birds preclude the entrance of snakes. This, however, is a large subject, into which I cannot now enter.

Let me in conclusion once more say, that as it seems to me, notwithstanding the labours of those great naturalists to whom I gratefully referred in commencing, there are in natural history few more promising or extensive fields for research than the habits of ants.

[J. L.]

WEEKLY EVENING MEETING,

Friday, February 2, 1877.

Sir T. Frederick Elliot, K.C.M.G. Vice-President, in the Chair.

Professor Osborne Reynolds,

OWENS COLLEGE, MANCHESTER.

Vortex Motion.

In commencing this discourse the author said, Whatever interest or significance the facts I hope to set before you may have, is in no small degree owing to their having, as it were, eluded the close mathematical search which has been made for them, and to their having in the end been discovered in a simple, not to say commonplace, manner. In this room you are accustomed to have set before you the latest triumphs of mind over matter, the secrets last wrested from nature by gigantic efforts of reason, imagination, and the most skilful manipulation. To-night, however, after you have seen what I shall endeavour to show you, I think you will readily admit that for once the case is reversed, and that the triumph rests with nature, in having for so long concealed what has been so eagerly sought, and what is at last found to have been so thinly covered.

The various motions which may be caused in a homogeneous fluid like water, present one of the most tempting fields for mathematical research. For not only are the conditions of the simplest, but the student or philosopher has on all hands the object of his research, which, whether in the form of the Atlantic waves or of the eddies in his teacup, constantly claims his attention. And, besides this, the exigencies of our existence render a knowledge of these motions of the greatest value to us in overcoming the limitations to which our actions are otherwise subject.

Accordingly we find that the study of fluid motion formed one of the very earliest branches of philosophy, and has ever since held its place, no subject having occupied the attention of mathematicians more closely. The results have been, in one sense, very successful; most important methods of reasoning have been developed, mathematical methods, which have helped to reveal numberless truths in other departments of science, and have taught us many things about fluids which most certainly we should not otherwise have found out, and of which we may some day find the application. But as regards the direct object in view, the revelation of the actual motion of fluids, the research has completely failed. And now that generations of mathematicians have passed away, now that the mysteries of the motions of the heavenly bodies, of the earth itself, and almost of every piece of solid matter on the earth have been explained by mathematicians, the simplest problems of fluid motion are yet unsolved.

If we draw a disc flatwise through the water, we know by a process of unconscious geometrical reasoning that the water must move round the disc; but by no known mathematical process could the motion be ascertained from the laws of motion. If we draw the plate obliquely through the water we experience a greater pressure on the one side than on the other. Now this case, representing as it does the principle of action of the screw propeller, is of the very highest importance to us; and yet, great as has been the research, it has revealed no law by which we may in a given case calculate the resistance to be obtained, or indeed tell from elementary principles in what way the water moves to let the plate pass. Again, the determination of the resistance which solid bodies, such as ships, encounter is of such exceeding economic importance, that theory, as shipbuilders call it, having failed to inform them what to expect, efforts have been, and are still being, made to ascertain the laws by direct experiment. Instances might be multiplied, but one other must suffice. If we send a puff of fluid into other fluid we know that it will travel to a considerable distance, but the manner in which it will travel and the motion it will cause in the surrounding fluid, mathematics have not revealed to us.

Now the reasons why mathematicians have thus been baffled by the internal motions of fluids appear to be very simple. Of the internal motions of water or air we can see nothing. On drawing the disc through the water there is no evidence of the water being in motion at all, so that those who have tried to explain these results have had no clue; they have had not only to determine the degree and direction of the motion, but also its character.

But although the want of a clue to the character of the motion may explain why so little has been done, it is not so easy to understand how it is that no attempts were made to obtain such a clue. It would seem that a certain pride in mathematics has prevented those engaged in these investigations from availing themselves of methods which might reflect on the infallibility of reason.

Suggestions as to the means have been plentiful. In other cases where it has been necessary to trace a particular portion of matter in its wanderings amongst other exactly similar portions, ways have been found to do it. It may be argued that the influences which determine the path of a particular portion of water are slight, subtle, and uncertain, but not so much so as those which determine the path of a sheep. And yet thousands of sheep have been from time immemorial turned loose on the mountains belonging to different owners, and although it probably never occurred to anyone to reason out the paths of his particular sheep, they have been easily identified by the aid of a little colour. And that the same plan might be pursued with fluids, every column of smoke has been evidence.

But these hints appear to have been entirely neglected, and it was left for nature herself, when, as it were, fully satisfied with having maintained her secret so long, and tired of throwing out hints which were not taken, at last to divulge the secret completely in the beautiful phenomenon of the smoke ring. At last; for the smoke ring is probably a phenomenon of modern times. The curls of smoke, as they ascend in an open space, present to the eye a hopeless entanglement; and although, when we know what to look for, we can see as it were imperfect rings in almost every smoke cloud, it is rarely that anything sufficiently definite is formed to attract attention, or suggest anything more important than an accidental curl. The accidental rings, when they are formed in a systematic manner, come either from the mouth of a gun, the puff of a steam engine, or the mouth of a smoker, none of which circumstances existed in ancient times.

Although, however, mathematicians can in no sense be said to have discovered the smoke ring, or the form of motion which it reveals, they were undoubtedly the first to invest it with importance. Had not Professor Helmholtz some twenty years ago called attention to the smoke ring by the beautiful mathematical explanation which he gave of its motion, it would in all probability still be regarded as a casual phenomenon, chiefly interesting from its beauty and rarity. Following close on Helmholtz came Sir William Thomson, who invested these rings with a transcendental interest by his suggestions that they are the type after which the molecules of solid matter are constituted.

The next thing to enhance the interest which these rings excited, was Professor Tait's simple and perfect process of producing them at will, and thus rendering them subjects for lecture-room experiments. Considering that this method will probably play a great part in perfecting our notions of fluid motion, it is an interesting question how Professor Tait came to hit upon it. There is only one of the accidental sources of these rings which bears even a faint resemblance to this box, and that is the mouth of a smoker as he produces these rings. This might have suggested the box to Professor Tait. But since this supposition involves the assumption that Professor Tait sometimes indulges in a bad habit, and as we all know that Professor Tait is an eminent mathematician, perhaps we ought rather to suppose that he was led to his discovery by some occult process of reasoning which his modesty has hitherto kept him from propounding.

But however this may be, his discovery was a most important one, and by its means the study of the actual motion of these rings has been carried far beyond what would otherwise have been possible.

But it has been for their own sake, and for such light as they might throw on the constitution of matter, that these rings were studied.

The most important lesson which they were capable of teaching still remained unlearned. It does not appear to have occurred to anyone that they were evidence of a general form of fluid motion, or that the means by which these had been revealed, would reveal other forms of motion.

There was, however, at least one exception, which will not be forgotten in this room: the use of smoke to show the effect of sound upon jets of air.

Also, the late Mr. Henry Deacon, in 1871, showed that minute vortex rings might be produced in water by projecting a drop of coloured water from a small tube. And his experiments, in spite of their small scale, excited considerable interest.

Four years ago, being engaged in investigating the action of the screw propeller, and being very much struck by the difference between some of the results he obtained and what he had been led to expect, the author made use of colour to try and explain the anomalies when he found that the vortex played a part in fluid motion which he had never dreamt of; that, in fact, it was the key to almost all the problems of internal fluid motion. That these results were equally new to those who had considered the subject much more deeply than he had, did not occur to him until after some conversation with Mr. Froude and Sir William Thomson.

Having noticed that the action of the screw propeller was greatly affected when air was allowed to descend to the blades, he was trying what influence air would have on the action of a simple oblique vane, when a very singular phenomenon presented itself. The air, instead of rising in bubbles to the surface, ranged itself in two long horizontal columns behind the vane. There was evidence of rotational motion about these air lines. It was evident, in fact, that they were the central lines of two systematic eddies.

That there should be eddies was not surprising, but eddies had always been looked upon as a necessary evil which besets fluid motion as sources of disturbance, whereas here they appeared to be the very means of systematic motion.

Here then was the explanation of the nature of the motion caused by the oblique vane, a cylindrical band of vortices continually produced at the front of the plate, and falling away behind it in an oblique direction.

The recognition of the vortex action caused behind the oblique vane, suggested that there might be similar vortices behind a disc moving flatwise through the water, such as are the eddies caused by a teaspoon.

There was one consideration, however, which at first seemed to render this improbable. It was obvious that the resistance of the oblique vane was caused in producing the vortices at its forward part; so that if a vortex were formed behind a flat plate, as this vortex would remain permanently behind, and not have to be continually elongated, the resistance should diminish after the plate was once set in motion;

whereas experience appeared to show that this was by no means the case. It appeared probable, therefore, that from some disturbing cause the vortex would not form, or would only form imperfectly, behind the plate.

This view was strengthened when, on trying the resistance of a flat plate, it did not appear to diminish after the plate had been started.

Accidentally, however, it was found that if the float to which the plate was attached was started suddenly and then released, the float and plate would move on apparently without any resistance. And more than this, for if the float were suddenly arrested and released, it would take up its motion again, showing that it was the water behind that was carrying it on.

There was evidence therefore of a vortex behind the disc. In the hope of rendering this motion visible, coloured water was injected in the neighbourhood of the disc, and then a beautiful vortex ring, exactly resembling the smoke ring, was seen to form behind the disc. If the float were released in time, this ring would carry the disc on with it; but if the speed of the disc were maintained uniform, the ring gradually dropped behind and broke up. Here then was another part played by the vortex previously undreamt of.

That the vortex takes a systematic part in almost every form of fluid motion was now evident. Any irregular solid moving through the water must from its angles send off lines of vortices such as those behind the oblique vane. As we move about we must be continually causing vortex rings and vortex bands in the air. Most of these will probably be irregular, and resemble more the curls in a smoke cloud than systematic rings. But from our mouths as we talk we must produce numberless rings.

One way in which rings are produced in perhaps as great numbers as from our mouths is by drops falling into the sea. If we colour the surface of a glass vessel full of water, and then let drops fall into it, rings are produced, which descend sometimes as much as two or three feet.

But the most striking rings are those produced in water, in a manner similar to that in which the smoke rings are produced, using coloured water instead of smoky air.

These rings are much more definite than smoke rings, and although they cannot move with higher velocities, since that of the smoke ring is unlimited, the speed at which they move is much more surprising.

In the air we are accustomed to see objects in rapid motion, and so far as our own notions are concerned, we are unaware of any resistance; but this is quite otherwise in water. Every swimmer knows what resistance water offers to his motions, so that when we see these rings flash through the water we cannot but be surprised. Yet a still more striking spectacle may be shown, if, instead of coloured water, a few bubbles of air be injected into the box from which the puff is sent; a beautiful ring of air is seen to shoot along through the water, showing, like the lines of air behind the oblique vane, little or no tendency to rise to the surface.

Such is the ease with which these vortex rings in water move, and so slight is the disturbance which they cause in the water behind them, as to lead to the conclusion that they experience no resistance whatever, except perhaps a little caused by slight irregularities in their construction. Their velocity gradually diminishes; but this would appear to be accounted for by their growth in size, for they are thus continually taking up fresh water into their constitution, with which they have to share their velocity. Careful experiments have confirmed this view. It is found that the force of the blow they will strike is nearly independent of the distance of the object struck from the orifice.

The discovery of the ring behind the disc afforded the opportunity of observing the characteristics of these rings much better than was afforded by the smoke rings; and also suggested facts which had previously been overlooked. The manner of motion of the water which formed the ring and of the surrounding water was very clearly seen. It was at once seen that the visible ring, whether of coloured water or air, was merely the central line of the vortex; that it was surrounded by a mass of coloured water, bearing something the same proportion to the visible ring as a ball made by wrapping string (in and out) round a curtain ring until the aperture was entirely filled up. The disc, when it was there, formed the front of this ball or spheroid of water, but the rest of the surface of the ball had nothing to separate it from the surrounding water but its own integrity. Yet when the motion was very steady the surface of the ball was definite, and the entire moving mass might be rendered visible by colour. The water within the ball was everywhere gyrating round the central ring, as if the coils of string were each spinning round the curtain ring as an axis, the water moving forwards through the interior of the ring and backwards round the outside, the velocity of gyration gradually diminishing as the distance from the central ring is increased.

The way in which the water moves to let the ball pass can also be seen, either by streaking the water with colour or suspending small balls in it. In moving to get out of the way and let the ball of water pass, the surrounding water partakes as it were of the gyrating motion of the water within the *ball*, the particles moving in a horseshoe fashion, so that at the actual surface of the *ball* the motion of the water outside is identical with that within, and there was no rubbing at the surface, and consequently no friction.

The maintenance of the shape of the moving mass of water against the unequal pressure of the surrounding water as it is pushed out of the way is what renders the internal gyratory motion essential to a mass of fluid moving through a fluid. The centrifugal force of this gyratory motion is what balances the excess of pressure of the surrounding water in the front and rear of the ball, compared with what it is at the sides.

It is impossible to have a ring in which the gyratory motion is great, and the velocity of progression slow. As the one motion dies out so does the other, and any attempt to accelerate the velocity of the ring by urging forward the disc, invariably destroyed it.

The striking ease with which the vortex ring, or the disc with the vortex ring behind it, moves through the water, naturally raised the question as to why a solid should experience resistance. Could it be that there was something in the particular spheroidal shape of these balls of water which allowed them to move freely. To try this, a solid of the same shape as the fluid ball was constructed and floated after the same manner as the disc. But when this was set in motion, it stopped directly—it would not move at all. What was the cause of this resistance? Here were two objects of the same shape and weight, the one of which moved freely through the water, and the other experienced very great resistance. The only difference was in the nature of the surface. As already explained, there is no friction at the surface of the water, whereas there must be friction between the water and the solid. But it could be easily shown that the resistance of the solid is much greater than what is accounted for by its surface friction or skin resistance. The only other respect in which these two surfaces differ is that the one is flexible, while the other is rigid, and this seems to be the cause of the difference in resistance.

If ribbons be attached to the edge of the disc, these ribbons will envelope the ball of water which follows it, presenting a surface which may be much greater than that of the solid; and yet this, being a flexible surface, the resistance of the disc with the vortex behind it is not very much greater than it would be without the ribbons—nothing to be compared to that of the solid.

Colouring the water behind the solid shows, that instead of passing through the water without disturbing it, there is very great disturbance in its wake. An interesting question is as to whether this disturbance originates with the motion of the solid, or only after the solid is in motion. This is settled by colouring the water immediately in front of the solid before it is started. Then on starting it the colour is seen to spread out in a film entirely over the surface of the solid, at first without the least disturbance, but this follows almost immediately.

Among the most striking features of the vortex rings, is their apparent elasticity. When disturbed they not only recover their shape, but vibrate about their mean position like an elastic solid. So much so, as to lead Sir William Thomson to the idea that the elasticity of solid matter must be due to its being composed of vortex rings.

But apart from such considerations, this vibration is interesting as showing that the only form of ring which can progress steadily is the circular. Two parallel bands, such as those which follow the oblique vane, could progress if they were infinitely long, but if not, they must be continually destroyed from the ends. Those which follow the oblique vane are continually dying out at one end, and being formed again at the other.

If an oval ring be formed behind an oval plate, the more sharply curved parts travel faster than the flatter parts; and hence, unless the plate be removed, the ring breaks up. It is possible, however, to withdraw the plate, so as to leave the oval ring, which proceeds wriggling along each portion moving in a direction perpendicular to that in which it is curved, and with a velocity proportional to the sharpness of the curvature. So that not only does the ring continually change its shape, but one part is continually falling behind, and then overtaking the other.

These were some of the forms of fluid motion which imagination or reason had failed to show us, but which had been revealed by the simple process of colouring the water.

Now that we can see what we are about, mathematics can be most usefully applied; and it is expected that when these facts come to be considered by those best able to do so, the theory of fluid motion will be placed on the same footing as the other branches of applied mechanics.

[O. R.]

GENERAL MONTHLY MEETING,

Monday, February 5, 1877.

Sir T. **Frederick Elliot**, K.C.M.G. Vice-President, in the Chair.

Sir Thomas Douglas Forsyth, K.C.S.I.
Miss Gladstone.

were *elected* Members of the Royal Institution.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Antiquaries, Society of—Proceedings, Vol. VII. No. 1. 8vo. 1876.
  Archæologia, Vol. XLIV. Part 2. 4to. 1877.
Asiatic Society, Royal—Journal, Vol. IX. Part 1. 8vo. 1876.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVII. Nos. 1, 2. 8vo.
Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1876: Heft 2. 8vo. 1876.
Board of Admiralty—Catalogue of the Admiralty Library. By R. Thorburn. 4to. 1875.
British Architects, Royal Institute of—Sessional Papers, 1876–7, Nos. 2–4. 4to.
British Museum Trustees—Catalogue of British Hymenoptera, Part I. 8vo. 1876.
Bucknill, J. C. M.D. (the Author)—Notes on Asylums for the Insane in America. 8vo. 1876.
Carrère, D.—Mémoire de Géométrie. 4to. 1876.
Chemical Society—Journal for Dec. 1876, Jan. 1877. 8vo.
Dawson, Dr. J. W. (the Author)—On a Recent Discovery of Carboniferous Batrachians in Nova Scotia. (From American Journal of Science, 1876. 8vo.)
Dobell, Horace, M.D. (the Editor)—Annual Reports on Diseases of the Chest. 2 vols. 8vo. 1875–6.
Editors—American Journal of Science for Dec. 1876 and Jan. 1877. 8vo.
  Argonaut for Dec. 1876 and Jan. 1877. 8vo.
  Athenæum for Dec. 1876 and Jan. 1877. 4to.
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WEEKLY EVENING MEETING,

Friday, February 9, 1877.

Sir W. Frederick Pollock, Bart. M.A. Vice-President,
in the Chair.

Francis Galton, Esq. F.R.S. F.G.S. *M.R.I.*

Typical Laws of Heredity.

We are far too apt to regard common events as matters of course, and to accept many things as obvious truths which are not obvious truths at all, but present problems of much interest. The problem to which I am about to direct attention is one of these.

Why is it, when we compare two groups of persons selected at random from the same race, but belonging to different generations of it, we find them to be closely alike? Such statistical differences as there may be, are always to be ascribed to differences in the general conditions of their lives; with these I am not concerned at present; but so far as regards the processes of heredity alone, the resemblance of consecutive generations is a fact common to all forms of life.

In each generation there will be tall and short individuals, heavy and light, strong and weak, dark and pale; yet the proportions of the innumerable grades in which these several characteristics occur tend to be constant. The records of geological history afford striking evidences of this statistical similarity. Fossil remains ot plants and animals may be dug out of strata at such different levels, that thousands of generations must have intervened between the periods in which they lived; yet in large samples of such fossils we seek in vain for peculiarities that will distinguish one generation taken as a whole from another, the different sizes, marks, and variations of every kind, occurring with equal frequency in both. The processes of heredity are found to be so wonderfully balanced, and their equilibrium to be so stable, that they concur in maintaining a perfect statistical resemblance so long as the external conditions remain unaltered.

If there be any who are inclined to say there is no wonder in the matter, because each individual tends to leave his like behind him, and therefore each generation must resemble the one preceding, I can assure them that they utterly misunderstand the case. Individuals do *not* equally tend to leave their like behind them, as will be seen best from an extreme illustration.

Let us then consider the family history of widely different groups, say of 100 men, the most gigantic of their race and time, and the same number of medium men. Giants marry much more rarely than medium men, and when they do marry they have but few children. It is a matter of history that the more remarkable giants have left no issue at all. Consequently the offspring of the 100 giants would be much fewer in number than those of the medium men. Again, these few would, on the average, be of lower stature than their fathers, for two reasons. First, their breed is almost sure to be diluted by marriage. Secondly, the progeny of all exceptional individuals tends to "revert" towards mediocrity. Consequently the children of the giant group would not only be very few, but they would also be comparatively short. Even of these the taller ones would be the least likely to live. It is by no means the tallest men who best survive hardships; their circulation is apt to be languid and their constitution consumptive.

It is obvious from this that the 100 giants will not leave behind them their quota in the next generation. The 100 medium men, on the other hand, being more fertile, breeding more truly to their like, being better fitted to survive hardships, &c., will leave more than their proportionate share of progeny. This being so, it might be expected that there would be fewer giants and more medium-sized men in the second generation than in the first. Yet, as a matter of fact, the giants and medium-sized men will, in the second generation, be found in the same proportions as before. The question, then, is this: How is it, that although each individual does *not* as a rule leave his like behind him, yet successive generations resemble each other with great exactitude in all their general features?

It has, I believe, become more generally known than formerly, that although the characteristics of height, weight, strength, and fleetness are very different in themselves, and though different species of plants and animals exhibit every kind of diversity, yet the differences in height, weight, and every other characteristic, among members of the same species, are universally distributed in fair conformity with a single law.

The phenomena with which that law deals are like those perspectives spoken of by Shakespeare, which, when viewed awry, show nothing but confusion.

Our ordinary way of looking at individual differences is awry: thus we naturally, but wrongly, judge of differences in stature by differences in heights measured from the ground, whereas on changing our point of view to that whence the law of deviation regards them, by taking the average height of the race, and not the ground, as the point of reference, all confusion disappears, and uniformity prevails.

It was to Quetelet that we were first indebted for a knowledge of the fact, that the amount and frequency of deviation from the average among members of the same race, in respect to each and every characteristic, tends to conform to the mathematical law of deviation.

The diagram contains extracts from some of the tables by which he corroborates his assertion. Three of the series in them refer to the heights of Americans, French, and Belgians respectively, and the fourth to the strength of Belgians. In each series there are two parallel columns, one entitled "observed," and the other "calculated," and the close conformity between each of the pairs is very striking.

Scale of Heights.	American Soldiers (25,878 Observations).		France (Hargenvilliers).		Belgium, Quetelet. 20 years' Observations.	
	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
mètres.						
1·90	1	3				
1·90	7	5	..	..	1	1
·87	14	13	..	1		
·84	25	28	25	3	2	3
·81	45	52		7	7	7
·79	99	84		16	14	14
·76	112	117	32	32	34	28
·73	138	142	55	55	48	53
·70	148	150	88	87	102	107
·68	137	137	114	118	138	136
·65	93	109	144	140	129	150
·62	109	75	140	145	162	150
·60	49	45	116	132	106	136
·57	14	24	..	105	110	107
·54	8	11	286	73	147	53
·51	1	4		44		28
·48	..	1		24		14
·45	..	..		11		7
·42	..	..		4		3
·39	..	..		2		1
·36	..	..		1		
	1000	1000	1000	1000	1000	1000

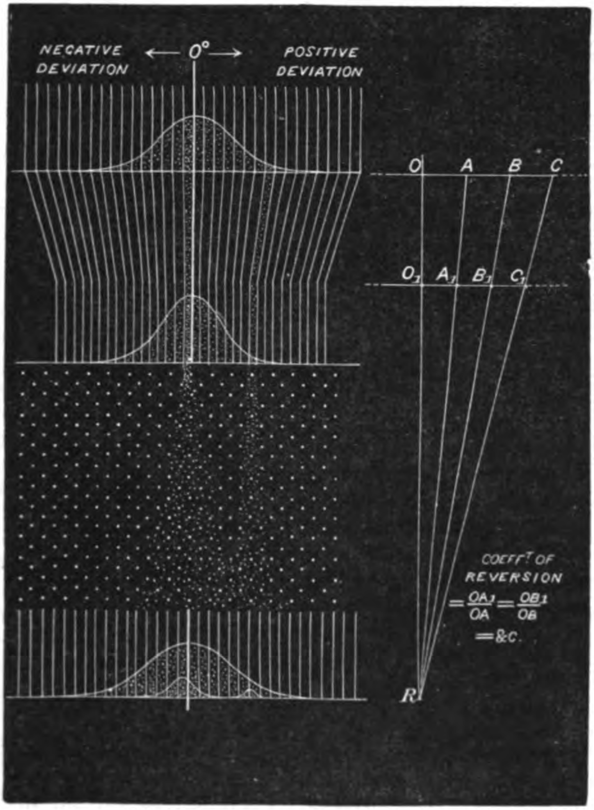
Degrees of Dynamometer.	Lifting Power of Belgian Men.	
	Observed.	Calculated.
200	1	1
190	9	10
180		
170	23	23
160		
150	32	32
140		
130	22	23
120		
110	12	10
100		
90	1	1
	100	100

These tables serve another purpose; they enable those who have not had experience of such statistics to appreciate the beautiful balance of the processes of heredity in ensuring the repetition of such finely graduated proportions as those that the tables record.

The outline of my problem of this evening is, that since the characteristics of all plants and animals tend to conform to the law of deviation, let us suppose a typical case, in which the conformity shall be exact, and which shall admit of discussion as a mathematical problem, and find what the laws of heredity must then be to enable successive generations to maintain statistical identity.

I shall have to speak so much about the law of deviation, that it is absolutely necessary to tax your attention for a few minutes to explain the principle upon which it is based, what it is that it professes to

FIG. 1.


NEGATIVE DEVIATION
← 0° →
POSITIVE DEVIATION
O
A
B
C
O_1
A_1
B_1
C_1
COEFF^T OF REVERSION
$= \frac{OA_1}{OA} = \frac{OB_1}{OB}$
=&c.
R

show, and what the two numbers are, which enable long series to be calculated like those in the tables just referred to. The simplest way of explaining the law is to begin by showing it in action. For this purpose I will use an apparatus that I employed three years ago in this very theatre, to illustrate other points connected with the law of deviation. An extension of its performance will prove of great service to us to-night; but I will begin by working the instrument as I did on the previous occasion. The portion of it that then existed, and to which I desire now to confine your attention, is shown in the lower part of Fig. 1, where I wish to direct your notice to the stream issuing from either of the divisions just above the dots, to its dispersion among them, and to the little heap that it forms on the bottom line. This part of the apparatus is like a harrow with its spikes facing us; below these are vertical compartments; the whole is faced with a glass plate. I will pour pellets from either of these divisions or from any other point above the spikes; they will fall against the spikes, tumble about among them, and after pursuing devious paths, each will finally sink to rest in the compartment that lies beneath the place whence it emerges from its troubles.

The courses of the pellets are extremely irregular; it rarely happens that any two starting from the same point will pursue the same path from beginning to end; yet, notwithstanding this, you will observe the regularity of the outline of the heap formed by the accumulation of pellets.

This outline is the geometrical representation of the curve of deviation. If the rows of spikes had been few, the deviation would have been slight, almost all the pellets would have lodged in the compartment immediately below the point whence they were dropped, and would then have resembled a column; if they had been very numerous, they would have been scattered so widely that the part of the curve for a long distance to the right and left of the point whence they were dropped would have been of uniform width, like an horizontal bar. With intermediate numbers of rows of teeth, the curved contour of the heap would assume different shapes, all having a strong family resemblance. I have cut some of these out of cardboard; they are represented in the diagrams 2, 3, 4 and 5, below. Theoretically speaking, every possible curve of deviation may be formed by an apparatus of this sort, using extremely numerous and delicate spikes and minute pellets, and by varying the length of the harrow and the number of pellets poured in. Or if I draw a curve on an elastic sheet of indiarubber, by stretching it laterally I produce the effects of increased dispersion; by stretching it vertically I produce that of increased numbers. The latter variation is shown by the three curves in each of the four diagrams; but it does not concern us to-night, as we are dealing with internal proportions, which are not affected by the absolute number of the sample employed. To specify the variety of curve so far as dispersion is concerned, we must measure the amount of lateral stretch of the indiarubber sheet. The curve has no

Fig. 2.

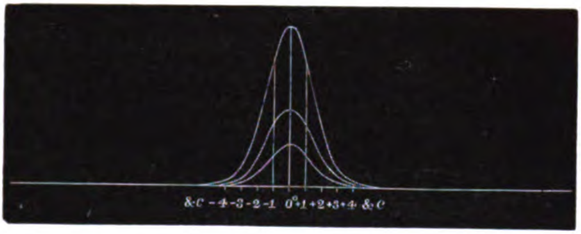


Fig. 3.

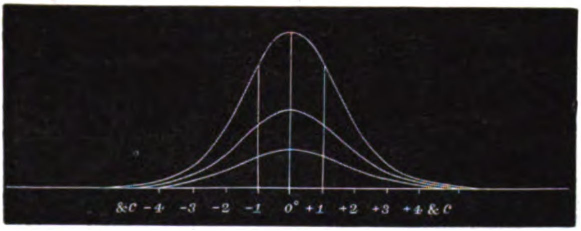


Fig. 4.

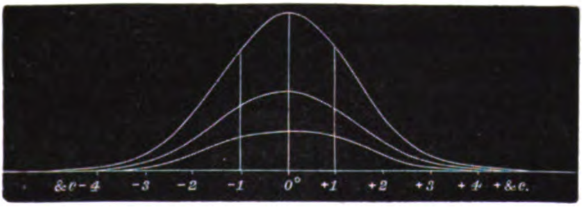
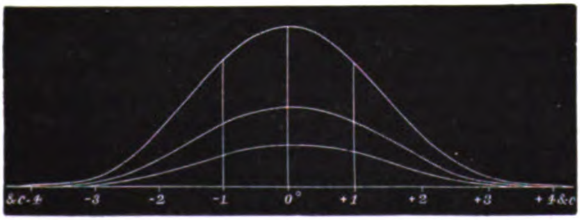
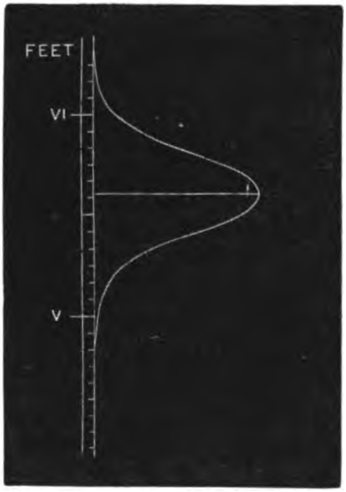


Fig. 5.



definite ends, so we have to select and define two points in its base, between which the stretch may be measured. One of these points is always taken directly below the place whence the pellets were poured in. This is the point of no deviation, and represents the mean position of all the pellets, or the average of a race. It is marked as 0°. The other point is conveniently taken at the foot of the vertical line that divides either half of the symmetrical figure into two equal areas. I take a half curve in cardboard that I have again divided into two portions along this line; the weight of the two portions is equal. This distance is the value of 1° of deviation, appropriate to each curve. We extend the scale on either side of 0° to as many degrees as we like, and we reckon deviation as positive, or to be added to the average, on one side of the centre, say to the right, and negative on the other, as shown on the diagrams. Owing to the construction, one-quarter or 25 per cent. of the pellets will lie between 0° and 1°, and the law shows that 16 per cent. will lie between + 1° and + 2°, 6 per cent. between + 2° and + 3° and so on. It is unnecessary to go more minutely into the figures, for it will be easily understood that a formula is capable of giving results to any minuteness and to any fraction of a degree.

Fig. 6.



Let us, for example, deal with the case of the American soldiers. I find, on referring to Gould's Book, that 1° of deviation was in their case 1·676 inches. The curve I hold in my hand, Fig. 6, has been drawn to that scale. I also find that their average height was 67·24 inches. I have here a standard marked with feet and inches. I apply the curve to the standard, and immediately we have a geometrical representation of the statistics of height of all those soldiers. The lengths of the ordinates show the proportion of men at and about their heights, and the area between any pairs of ordinates gives the proportionate number of men between those limits. It is indeed a strange fact, that any one of us sitting quietly at his table could, on being told the two numbers just mentioned, draw out a curve on ruled paper, from which thousands of vertical lines might be chalked side by side on a wall, at the distance apart that is taken up by each man in a rank of American soldiers, and know that if the same number of these American soldiers, taken indiscriminately, had been sorted according to their stature and marched up to the wall, each man of them would find the chalked line which he saw opposite to him to be of exactly his own height. So far as I can judge from the run of the figures in the table, the error would never exceed a quarter of an inch, except at either extremity of the series.

The principle of the law of deviation is very simple. The important influences that acted upon each pellet were the same; namely, the position of the point whence it was dropped, and the force of gravity. So far as these are concerned, every pellet would have pursued an identical path. But in addition to these, there were a host of petty disturbing influences, represented by the spikes among which the pellets tumbled in all sorts of ways. The theory of combination shows that the commonest case is that where a pellet falls equally often to the right of a spike as to the left of it, and therefore drops into the compartment vertically below the point where it entered the harrow. It also shows that the cases are very rare of runs of luck carrying the pellet much oftener to one side than the other of the successive spikes. The law of deviation is purely numerical; it does not regard the fact whether the objects treated of are pellets in an apparatus like this, or shots at a target, or games of chance, or any other of the numerous groups of occurrences to which it is or may be applied.*

I have now done with my description of the law. I know it has been tedious, but it is an extremely difficult topic to handle on an occasion like this. I trust the application of it will prove of more interest.

First, let me point out a fact which Quetelet and all writers who have followed in his path have unaccountably overlooked, and which has an intimate bearing on our work to-night. It is that, although characteristics of plants and animals conform to the law, the reason of their doing so is as yet totally unexplained. The essence of the law is that differences should be wholly due to the collective actions of a host of independent *petty* influences in various combinations, which were represented by the teeth of the harrow, among which the pellets tumbled in various ways. Now the processes of heredity that limit the number of the children of one class, such as giants, that diminish their resemblance to their fathers, and kill many of them, are not petty influences, but very important ones. Any selective tendency is ruin to the law of deviation, yet among the processes of heredity there is the large influence of natural selection. The conclusion is of the greatest importance to our problem. It is, that the processes of heredity must work harmoniously with the law of deviation, and be themselves in some sense conformable to it. Each of the processes must show this conformity separately, quite irrespectively of the rest. It is not an admissible hypothesis that any two or more of them, such as reversion and natural selection, should follow laws so exactly inverse to one another that the one should reform what the other had deformed; because characteristics, in which the relative importance of the various processes is very different, are none the less capable of conforming closely to the typical condition.

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* Quetelet, apparently from habit rather than theory, always adopted the binomial law of error, basing his tables on a binomial of high power. It is absolutely necessary to the theory of the present paper to get rid of binomial limitations and to consider the law of deviation or error in its exponential form.

When the idea first occurred to me, it became evident that the problem might be solved by the aid of a very moderate amount of experiment. The properties of the law of deviation are not numerous, and they are very peculiar. All, therefore, that was needed from experiment was suggestion. I did not want proof, because the theoretical exigencies of the problem would afford that. What I wanted was to be started in the right direction.

I will now allude to my experiments. I cast about for some time to find a population possessed of some measurable characteristic that conformed fairly well to the law, and that was suitable for investigation. I determined to take seeds and their weights, and after many preparatory inquiries, fixed upon those of sweet-peas. They were particularly well suited to my purposes; they do not cross-fertilise, which is a very exceptional condition; they are hardy, prolific, of a convenient size to handle, and their weight does not alter when the air is damp or dry. The little pea at the end of the pod, so characteristic of ordinary peas, is absent in sweet-peas. I weighed seeds individually, by thousands, and treated them as a census officer would treat a large population. Then I selected with great pains several sets for planting. Each set contained seven little packets, and in each packet were ten seeds, precisely of the same weight. Number one of the packets contained giant seeds, all as nearly as might be of $+ 3^\circ$ of deviation. Number seven contained very small seeds, all of $- 3^\circ$ of deviation. The intermediate packets corresponded severally to the intermediate degrees $\pm 2^\circ \pm 1^\circ$ and 0°. As the seeds are too small to exhibit, I have cut out discs of paper in strict proportion to their sizes, and strips in strict proportion to their weights, and have hung below them the foliage produced by one complete set. Many friends and acquaintances each undertook the planting and culture of a complete set, so that I had simultaneous experiments going on in various parts of the United Kingdom. Two proved failures, but the final result was this: that I obtained the more or less complete produce of seven sets, that is, of $7 \times 7 \times 10$, or 490 carefully weighed seeds.

It would be wholly out of place if I were to enter into the details of the experiments themselves, the numerous little difficulties and imperfections in them, or how I balanced doubtful cases, how I divided returns into groups, to see if they confirmed one another, or how I conducted any other of the well-known statistical operations. Suffice it to say that I took immense pains, which, if I had understood the general conditions of the problem as clearly as I do now, I should not perhaps have cared to bestow. The results were most satisfactory. They gave me two data, which were all that I required in order to understand the simplest form of descent, and so I got at the heart of the problem at once.

Simple descent means this. The parentage must be single, as in the case of the sweet-peas which are not cross-fertilised, and the rate of production and the incidence of natural selection must both be independent of the characteristic. The only processes concerned in simple descent that can affect the characteristics of a sample of a population are those of Family Variability and Reversion. It is well to define these words clearly. By family variability is meant the departure of the children of the same or similarly descended families, from the ideal mean type of all of them. Reversion is the tendency of that ideal mean filial type to depart from the parent type, "reverting" towards what may be roughly and perhaps fairly described as the average ancestral type. If family variability had been the only process in simple descent that affected the characteristics of a sample, the dispersion of the race from its mean ideal type would indefinitely increase with the number of the generations; but reversion checks this increase, and brings it to a standstill, under conditions which will now be explained.

On weighing and sorting large samples of the produce of each of the seven different classes of the peas, I found in every case the law of deviation to prevail, and in every case the value of 1° of deviation to be the same. I was certainly astonished to find the family variability of the produce of the little seeds to be equal to that of the big ones; but so it was, and I thankfully accept the fact; for if it had been otherwise, I cannot imagine, from theoretical considerations, how the typical problem could be solved.

The next great fact was that reversion followed the simplest possible law; the proportion being constant between the deviation of the mean weight of the produce generally and the deviation of the parent seed, reckoning in every case from one standard point. In a typical case, that standard must be the mean of the race, otherwise the deviation would become unsymmetrical, and cease to conform to the law.

I have adjusted an apparatus (Fig. 1) to exhibit the action of these two processes. We may consider them to act not simultaneously, but in succession, and it is purely a matter of convenience which of the two we suppose to act the first. I suppose first Reversion, then Family Variability. That is to say, I suppose the parent first to revert, and then to *tend* to breed his like. So there are three stages: (1) the population of parents, (2) that of reverted parents, (3) that of their offspring; or, what comes to the same thing, (1) the population of parents, (2) that of the *mean* produce of each parent, (3) that of their actual produce. In arranging the apparatus I have supposed the population to continue uniform in numbers. This is a matter of no theoretical concern, as the whole of this memoir relates to the distinguishing peculiarities of samples irrespectively of the absolute number of individuals in those samples. The apparatus consists of a row of vertical compartments, with trap-doors below them, to hold pellets which serve as representatives of a population of seeds. I will begin with showing how it expresses Reversion. In the upper stage of the apparatus the number of pellets in each compartment represents the relative number in a population of seeds, whose weight deviates from the average, within the limits expressed by the distances of the sides of that compartment from the middle point. The correct shape of the heap has been ensured by a slit of the proper curvature in the board that forms the back of the apparatus. As the apparatus is glazed in front, I have only to pour pellets from above until they reach the level of the slit. Such overplus as may have been poured in will run through the slit, to waste, at the back. The pellets to the right of the heap represent the heaviest seeds, those to the left the lightest. I shall shortly open the trap-door on which the few representatives of the giant seeds rest. They will run downwards through an inclined shoot, and fall into another compartment nearer the centre than before. I shall repeat the process on a second compartment in the upper stage, and successively on all the others. Every shoot converges towards one standard point in the middle vertical line; therefore the present shape of the heap of pellets is more contracted in width than it was before, and is of course more humped up in the middle. We need not regard the humping up; what we have to observe is, that each degree of deviation is simultaneously lessened. The effect is as though the curve of the first heap had been copied on a stretched sheet of indiarubber that was subsequently released. It is obvious from this that the process of reversion co-operates with the general law of deviation. The diagram that I annexed to Fig. 1, shows the principle of the process of reversion in a way that will be readily understood by many of those who are present.

I have now to exhibit the effects of variability among members of the same family. It will be recollected that the produce of peas of the same class deviated normally on either side of their own mean weight; consequently, I must cause the pellets which were in each of the upper compartments to deviate on either side of the compartment in which they now lie, which corresponds to that of the medium weight of their produce. I open the trap-door below one of the compartments in the second stage, the pellets run downwards through the harrow, dispersing as they run, and form a little heap in the lowest compartments, the centre of which heap lies vertically below the trap-door through which they fell. This is the contribution to the succeeding generation of all the individuals belonging to the compartment in the upper stage from which they came. They first reverted and then dispersed. I open another trap-door, and a similar process is gone through; a few extreme pellets in this case add themselves to the first formed heap. Again I continue the process; heap adds itself to heap, and when all the pellets have fallen through, we see that the aggregate contributions bear an exact resemblance to the heap from which we originally started. A formula (see Appendix) expresses the conditions of equilibrium. I attended to these conditions, when I cut out the slit in the backboard of the upper compartment, by which the shape of the original heap was regulated. As an example of the results that follow from the formula, I may mention that if deviation after reversion is to deviation before reversion as 4 to 5, and if 1° of family variability is six units, then the value of 1° in the population must be ten units.

It is easy to prove that the bottom heap is strictly a curve of deviation, and that its scale tends invariably to become the same as that of the upper one. It will be recollected that I showed that every variety of curve of deviation was producible by variations in the length of the harrow, and that if the pellets were intercepted at successive stages of their descent they would form a succession of curves of increasing scales of deviation. The curve in the second stage may therefore be looked upon as one of these intercepts; all that it receives in sinking to the third stage being an additional dose of dispersion.

As regards the precise scale of deviation that characterises each population, let us trace, in imagination, the history of the descendants of a single medium-sized seed. In the first generation the differences are merely those due to family variability; in the second generation the tendency to wider dispersion is somewhat restrained by the effect of reversion; in the third, the dispersion again increases, but is more largely restrained, and the same process continues in successive generations, until the step-by-step progress of dispersion has been overtaken and exactly checked by the growing antagonism of reversion. Reversion acts precisely after the law of an elastic spring, as was well shown by the illustration of the indiarubber sheet. Its tendency to recoil increases the more it is stretched, hence equilibrium must at length ensue between reversion and family variability, and therefore the scale of deviation of the lower heap must after many generations always become identical with that of the upper one.

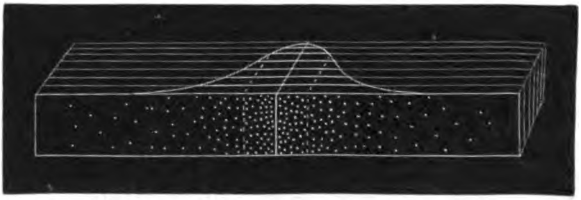
We have now surmounted the greatest difficulty of our problem; what remains will be shortly disposed of. This refers to sexual selection, productiveness, and natural selection. Let us henceforth suppose the heights and every other characteristic of all members of a population to be reduced to a uniform adult male standard so that we may treat it as a single group. Suppose, for example, a female whose height was equal to the average female height + 3° of female deviation, the equivalent in terms of male stature is the average male height + 3° of male deviation. Hence the female in question must be registered not in the feet and inches of her actual height, but in those of the equivalent male stature.

On this supposition we may take the numerical mean of the stature of each couple as the equivalent of a single hermaphrodite parent, so that a male parent plant having 1° deviation, and of a female parent plant having 2° of deviation, would together rank as a single self-fertilised plant of + 1½°.

In order that the law of sexual selection should co-operate with the conditions of a typical population, it is necessary that selection should be *nil;* that is, that there should not be the least tendency for tall men to marry tall women rather than short ones. Each strictly typical quality taken by itself must go for nothing in sexual selection. Under these circumstances, one of the best known properties of the law of deviation (technically called that of "two fallible measures") shows that the population of sums of couples would conform truly to the law, and the value of 1° would be that of the original population multiplied by $\sqrt{2}$. Consequently the population of *means of couples* would equally conform to the law; but in this case, as the deviations of means of couples are half those of sums of couples, the 1° of original deviation would have to be divided by $\sqrt{2}$.

The two remaining processes are Productiveness and Survival. Physiologically they are alike, and it is reasonable to expect the same general law to govern both. Natural selection is measured by the percentage of survival among individuals born with like characteristics. Productiveness is measured by the average number of children from all parents who have like characteristics, but it may physiologically be looked upon as the percentage of survival of a vast and unknown number of possible embryos, producible by such parents. The number being unknown creates no difficulty, if there may be considered to be, on an average, the same in every class. Experiment could tell me little about either natural selection or productiveness. What I have to say is based on plain theory. I can explain this best by the process of natural selection. In each species, the height, &c., the most favoured by natural selection, is the one in which the demerits of excess or deficiency are the most frequently balanced. It is therefore not unreasonable to look at nature as a marksman, her aim being subject to the same law of deviation as that which causes the shot on a target to be dispersed on either side of the point aimed at. It would not be difficult, but it would be tedious, to justify the analogy; however, it is unnecessary to do so, as I propose to base the analogy on the exigencies of the typical formula, no other supposition being capable of fulfilling its requirements. Suppose for a moment that nature aims, as a marksman, at the medium class, on purpose to destroy and not to save it. Let a block of stone, as is Fig. 7, represent a rampart, and let a gun be directed at a vertical line

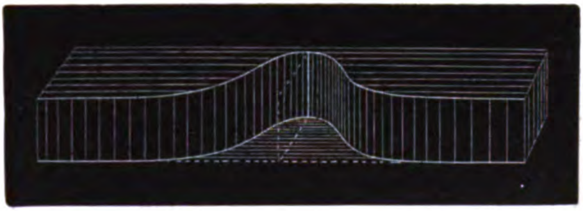
Fig. 7.



on its side on purpose to breach it, the shots would fall with the greatest frequency in the neighbourhood of the vertical line, and their marks would diminish in frequency as the distance increased, in conformity with the law of deviation. Each shot would batter away a bit of stone, and the shape of the breach would be such that its horizontal outline will be the well-known curve. This would be the action of nature were she to aim at the destruction of medium sizes. Her action as preserver of them is the exact converse, and would be represented by a cast that filled the gap and exactly replaced the material that had been battered away. The percentage of thickness of wall that had been destroyed at each degree of deviation is represented by the ordinate of the curve, therefore the percentage of survival is also an ordinate of the same curve of deviation. Its scale has a special value in each instance, subject to the general condition in every typical case, that its 0° shall correspond to the 0° of deviation of height, or whatever the characteristic may be.

In Fig. 8, the thickness of wall that has been destroyed at each degree of deviation is represented by the corresponding ordinate of the horizontal outline of the portion which remains. Similarly, in the case of an imaginary population, in which each class was *equally* numerous, the amount of survivors at each degree of deviation will be represented by the corresponding ordinate of this or a similar curve.

FIG. 8.



But in the original population at which we are supposing nature to aim, the representatives of each class are not equally numerous, but are arranged according to the law of deviation; the middle class being most numerous, while the extreme classes are but scantily represented. The ordinate of the above-mentioned outline will in this case represent, not the *absolute number*, but the *percentage* of survivors at each degree of deviation.

If a graphic representation is desired, that shall give the absolute number of survivors at each degree, we must shape the rampart which forms nature's target so as to be highest in the middle and to slope away at each side according to the law of deviation. Thus Fig. 9 represents the curved rampart before the battered part has been removed; Fig. 10, afterwards.

I have taken a block of wood similar to Fig. 7, to represent the rampart; it is of equal height throughout. A cut has been made at right angles to its base with a fret-saw, to divide it into two portions —that which would remain after it had been breached, Fig. 8, and the cast of the breach. Then a second cut with the fret-saw has been made at right angles to its face, to cut out of the rampart an equivalent to the heap of pellets that represents the original population. The gap that would be made in the heap and the cast that would fill the gap are curved on two faces, as in the model. This is sufficiently represented in Fig. 10.

FIG. 9.

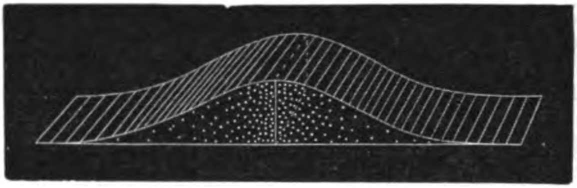
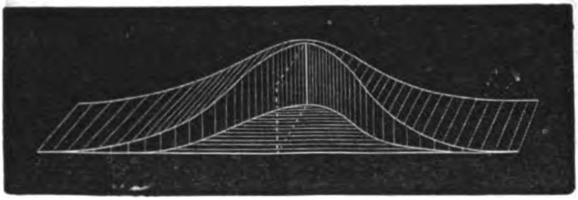


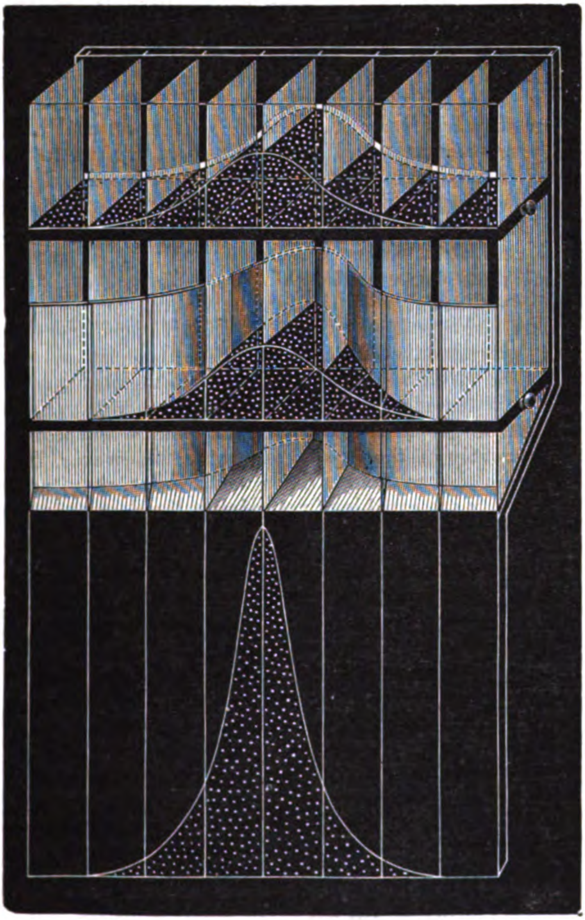
FIG. 10.



The operation of natural selection on a population already arranged according to the law of deviation is represented more completely in an apparatus, Fig. 11, which I will set to work immediately.

It is faced with a sheet of glass. The heap, as shown in the upper compartment of the apparatus, is 3 inches in thickness, and the pellets rest on slides. Directly below the slides, and running from side to side of the apparatus, is a curved partition, which will separate the pellets as they fall upon it, into two portions, one that runs to waste at the back, and another that falls to the front, and forms a new heap. The curve of the partition is a curve of deviation. The shape of this heap is identical with the cast of the gap in Fig. 10. It is highest and thickest in the middle, and it fines away towards either extremity. When the slide upon which it rests is removed, the pellets run down an inclined plane that directs them into a frame of uniform and shallow depth. The pellets from the deep central compartments (it has been impossible to represent in the diagram as many of these as there were in the apparatus) will stand very high from the bottom of the shallow frame, while those that came from the distant compartments will stand even lower than they did before. It follows that the selected pellets form, in the lower compartment, a heap of which the scale of deviation is much more contracted than that of the heap from which it was derived. It is perfectly normal in shape, owing to an interesting theoretical property of the law of deviation (see formula at end of this memoir).

FIG. 11.



Productiveness follows the same general law as survival, being a percentage of possible production, though it is usual to look on it as a simple multiple, without first multiplying and then dividing by the 100. Looking upon it as a simple multiple, the front face of each compartment in the upper heap represents the number of the parents of the same class, and the depth of the partition below compartment represents the average number that each individual of that class produces.

To sum up. We now see clearly the way in which the resemblance of a population is maintained. In the purely typical case, all the processes of heredity and selection are subject to well-defined and simple laws, which I have formulated in the appendix. Family variability, productiveness, and survival are all subject to the law of deviation, and reversion is expressed by a simple fractional coefficient. It follows that when we know in respect to any characteristic, the values of 1° in the several curves of family variability, productiveness and survival, and when we know the coefficient of reversion, we know absolutely all about the ways in which the characteristic in question will be distributed among the population at large.

I have confined myself in this explanation to purely typical cases, but it is easy to understand how the actions of the processes would be modified in those that were not typical. Reversion might not be directed towards the mean of the race; neither productiveness nor survival might be greatest in the medium classes, and none of their laws may be strictly of the typical character. However, in all cases the general principles would be the same, and the same actions that restrain variability are capable of restraining the departure of average values beyond certain limits in cases where any of the above-mentioned processes are unsymmetrical in their actions. The typical laws are those which most nearly express what takes place in nature generally; they may never be exactly correct in any one case, but at the same time they will always be approximately true and always serviceable for explanation. We estimate through their means the effects of the laws of sexual selection, of productiveness, and of survival, in aiding that of reversion in bridling the dispersive effect of family variability. They show us that natural selection does not act by carving out each new generation according to a definite pattern on a Procrustean bed, irrespective of waste. They also explain how small a contribution is made to future generations by those who deviate widely from the mean, either in excess or deficiency, and they enable us to discover the precise sources whence the deficiencies in the produce of exceptional types are supplied, and their relative contributions. We see by them that the ordinary genealogical course of a race consists in a constant outgrowth from its centre, a constant dying away at its margins, and a tendency of the scanty remnants of all exceptional stock to revert to that mediocrity, whence the majority of their ancestors originally sprang.

APPENDIX.

I will now proceed to formulate the typical laws. In what has been said, 1° of deviation has been taken equal to the "probable error" = C × 0·4769 in the well-known formula

$$y = \frac{1}{c\sqrt{\pi}} \cdot e^{-\frac{x^2}{c^2}}.$$

According to this, if x = amount of deviation in feet, inches, or any other external unit of measurement, then the number of individuals in any sample who deviate between x and $x + \delta x$ will vary as $e^{-\frac{x^2}{c^2}} \delta x$ (it will be borne in mind that we are for the most part not concerned with the coefficient in the above formula).

Let the modulus of deviation (c) in the original population, after the process has been gone through, of converting the measurements of all its members (in respect to the characteristic in question) to the adult male standard, be written c_0.

1. Sexual selection has been taken as *nil*, therefore the population of "parentages" is a population of which each unit consists of the mean of a couple taken indiscriminately. This, as well known, will conform to the law of deviation, and its modulus, which we will write c_1, has already been shown to be equal to $\frac{1}{\sqrt{2}} \cdot c_0$.

2. Reversion is expressed by a simple fractional coefficient of the deviation, which we will write r. In the "reverted" parentages (a phrase whose meaning and purport have already been explained),

$$y = \frac{1}{rc\sqrt{\pi}} \cdot e^{-\frac{x^2}{r^2 c^2}}.$$

In short, the population of which each unit is a reverted parentage follows the law of deviation, and has its modulus, which we will write c_2, equal to $r c_1$.

3. Productiveness. We saw that it followed the law of deviation; let its modulus be written f. Then the number of children to each parentage that differs by the amount of x from the mean of the parentages generally (i.e. from the mean of the race) will vary as $e^{-\frac{x^2}{f^2}}$; but the number of such parentages varies as $e^{-\frac{x^2}{c_2^2}}$, therefore if each child absolutely resembled his parent, the number of children who deviated x would vary as $e^{-\frac{x^2}{f^2}} \times e^{-\frac{x^2}{c_2^2}}$, or as $e^{-x^2\left\{\frac{1}{f^2}+\frac{1}{c_2^2}\right\}}$. Hence the deviations of such children in their amount and frequency would conform to the law, and the modulus of the population of

children in the supposed case of absolute resemblance to their parents, which we will write c_3, is such that

$$\frac{1}{c_3} = \sqrt{\left(\frac{1}{f^2} + \frac{1}{c^2}\right)}.$$

We may, however, consider the parents to be multiplied, and the productivity of each of them to be uniform; it is more convenient than the converse supposition, and it comes to the same thing. So we will suppose the reverted parentages to be more numerous but equally prolific, in which case their modulus will be c_3, as above.

4. Family variability was shown by experiment to follow the law of deviation, its modulus, which we will write v, being the same for all classes. Therefore the amount of deviation of any one of the offspring from the mean of his race is due to the combination of two influences—the deviation of his "reverted" parentage and his own family variability; both of which follow the law of deviation. This is obviously an instance of the well-known law of the "sum of two fallible measures."* Therefore the modulus of the population in the present stage, which we will write c_4, is equal to $\sqrt{(v^2 + c_3^2)}$.

5. Natural selection follows, as has been explained, the same general law as productiveness. Let its modulus be written s, then the percentage of survivals among children, who deviate x from the mean, varies as $e^{-\frac{x^2}{s^2}}$; and for the same reasons as those already given, its effect will be to leave the population still in conformity with the law of deviation, but with an altered modulus, which we will write c_5, and

$$\frac{1}{c^2} = \sqrt{\left(\frac{1}{s^2} + \frac{1}{c_4^2}\right)}.$$

Putting these together, we have, starting with the original population having a modulus $= c_0$,

$$1.\quad c_1 = \frac{1}{\sqrt{2}} c_0.$$

$$2.\quad c_2 = r c_1.$$

$$3.\quad c_3 = \sqrt{\left\{\frac{f^2 c^2_2}{f^2 + c_2^2}\right\}}.$$

$$4.\quad c_4 = \sqrt{\{v^2 + c_3^2\}}.$$

$$5.\quad c_5 = \sqrt{\left\{\frac{s^2 c_4^2}{s^2 + c_4^2}\right\}}.$$

And lastly, as the condition of maintenance of statistical resemblance in consecutive generations,

$$6.\quad c_5 = c_0.$$

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* Airy, 'Theory of Errors,' § 43.

Hence, given the coefficient r and the moduli v, f, s, the value of c_0 (or c_5) can be easily calculated.

In the case of simple descent, which was the one first considered, we have nothing to do with c_0, but begin from c_1. Again, as both fertility and natural selection are in this case uniform, the values of f and s are infinite. Consequently our equations are reduced to

$$c_3 = r\,c_1\,; \quad c_4 = \surd\{v^2 + c_3^2\}\,; \quad c_4 = c_1,$$

whence

$$c_1^2 = \frac{v^2}{1 - r^2}.$$

Suppose, for example, that $r = \frac{4}{5}$ and $v = 6$, then

$$c_1^2 = \frac{36}{1 - \frac{16}{25}} = \frac{36 \times 25}{9} = 100,$$

or

$$c_1 = 10,$$

as was mentioned in the course of the foregoing remarks.

[F. G.]

WEEKLY EVENING MEETING,

Friday, February 16, 1877.

SIR T. FREDERICK ELLIOT, K.C. M.G. Vice-President, in the Chair.

PROFESSOR FREDERICK GUTHRIE, F.R.S.

Solid Water.

WHEN, some months ago, I received, through your excellent Secretary, a notification of your wish that I should undertake one of these Friday evening discourses, I looked upon the invitation as a command: as a command, if for no other reason, because, for some years past, I have, through your courtesy, been privileged to participate between these walls in many a rare intellectual treat.

As I cast about for a suitable subject, I felt sure that, at your hands, I should not incur the charge of egoism if I brought before you, as briefly and simply as I could, the results of some experimental researches which for the last few years have engrossed my leisure time. And whatever hesitation I had in selecting a subject of this nature, has been obliterated by the feeling that what I have to speak of to-night illustrates a generalization which, I think, has not hitherto been enunciated with sufficient distinctness, but which may be maintained with great show of reason. I will not call it a law of nature, for they who use the term law in connection with natural phenomena are, I believe, neither sufficiently alive to the insignificance of law, nor to the omnipotence of nature. Let me therefore call what I mean by the more expressive, because more ambiguous, term "Generalization." It is this: "Substances which are most abundant are in their nature most exceptional." At once, among the elements we find that great trinity, oxygen, hydrogen, and nitrogen, standing as a group far removed from other elements, only to differ from one another by immeasurable intervals. The metal sodium, than which there is none more remarkable, is perhaps the most abundant; while the rarer metals, such as gold, platinum, osmium, iridium, &c., have many characteristics in common.

It will not do to push this proposition too far: for even as I spoke, instances have occurred to you, as they have to me, where the generalization appears at least to fail. If, however, I wished to adduce the strongest testimony in its favour, I should speak of water, a compound body, and of all compound bodies as simple as any, and perhaps as abundant. Let me glance at the properties which render it pre-eminent. Of all compound liquids, it has by far the greatest hardness or cohesion. Gather under like circumstances drops of water and of all other known liquids, and you will find those of water the largest. Endeavour to pass radiant heat through it, and you find it resists such passage with the greatest stubbornness: it is eminently athermanous. But compare its power of allowing heat of contact to pass through, and you find it to be by far the best liquid heat conductor. Examine its capacity for heat, and you find that it here exceeds all known liquids, with one exception, and that exception is a mixture containing a large proportion of water. Cast light on it, and it bends that light less than any substance, excepting its own offspring, ice. Finally, it enjoys that rare and almost unique property of having a state of maximum density at a temperature above its freezing point.

On all these grounds, the expression of the old Greek,

ἀριστον μεν ὑδωρ,

seems to have been prophetic, if we understand *αριστος* in its simple sense of pre-eminence.

I propose this evening to consider mainly one faculty of water, namely, its power of dissolving that innumerable class of bodies called salts, which may, I suppose, without much danger of a misunderstanding, be considered as bodies soluble in water, and which contain metals. I wish to consider the relationship between the water and the salt it has dissolved, and I desire to direct especial attention to the circumstances under which water becomes solid in presence of and in combination with other bodies.

Water may become solid in a variety of ways. First, by itself, losing heat, it becomes ice; secondly, when thrown upon quick lime and similar bodies, by the loss of heat it becomes along with the lime a solid, slaked lime; thirdly, if you pour water upon dried alum and similar bodies, and let the solution stand, you get dry crystals, I dare scarcely say containing water, but from which water can be readily got. Again, if you boil glue or isinglass with water, you get a more or less solid jelly, according to the quantity of water you employ.

These various ways by which water may be solidified result in the more or less complete obliteration of its character as water. Chemists tell us that in the slaking of lime we do not witness the mere juxtaposing of a molecule of water alongside a molecule of lime, but rather a double decomposition whereby the molecules of each constituent are decomposed and one or two new molecules are formed; [*]

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[*] Not $CaO + HO = CaOHO$, but $Ca_2O + H_2O = 2CaHO$,
$(Ca = 20, H = 1, O = 8)$ $(Ca = 20, H = 1, O = 16)$
or $CaO + H_2O = Ca2HO$.
$(Ca = 40, H = 1, O = 16)$

and that when by heat the water from slaked lime is recovered, recomposition of quick lime and of water ensues. Far less violent is the action when burnt alum dissolves in water, and far more readily are the two again separated: while from the ordinary "washing soda" in moderately dry air the water gradually escapes by diffusion into the air. Yet in all these cases the water is held by the solid with some strength. Lastly, when glue or gum dissolves in water there appears to be no energy expended in the act of solution, they mix even as two gases mix.

Strictly speaking, when a grain of salt dissolves in a gallon of water both are destroyed; the salt ceases to be salt and the water ceases to be water: the two form a salt solution. This fact must never be forgotten, but its strict observance would land us here in the cumbersome restriction of denying the name of water to the liquids of our springs, rivers, and seas.

On the table you see examples of the various kinds of solid water. And first stands ice. In this room it is unnecessary for me to speak especially of this substance, which has formed for you the basis of so many an eloquent discourse. Again, there is water of combination, of constitution, of gelatinization, and of crystallization.

It is to be remarked, and is indeed for our present purpose most noteworthy, that whole classes of salts are known which solidify with water of crystallization, while others of no noticeable chemical difference reject water as they build themselves together. Nay, more, that salts most closely allied to one another in their chemical nature combine with water of crystallization in very different proportions. What is there peculiar in saltpeter, in lunar-caustic, in sal-ammoniac, that they should hitherto have refused to associate themselves as solids with water; while alum, soda, and the "vitriols," white, green, and blue, combine with water and form crystals of such beauty; to me of such extreme interest because in their faces and edges, as in those of other and anhydrous crystals, nature for once makes use of planes and straight lines?

It has been my good fortune to have been able, to some extent, to wipe out this line of demarcation, to establish continuity; to prove, in short, that all salts whatever, which are soluble in water, are able to combine with it in definite weight-ratio to form solid crystalline bodies. I do not doubt but that we may consider the number of known definite compounds to have been thereby at least doubled.

The formation of these new solid water-compounds may perhaps be best approached by studying the phenomena which take place when any salt solution is cooled. Let us consider a boiling saturated solution of saltpeter. Take it from over the lamp, and let it cool. A certain quantity of the salt separates out, but the crystals are free from water. Cool it down to 0° C., more anhydrous saltpeter separates; but at 0° C. it is still rich in saltpeter, and is, of course, saturated at that temperature. What takes place if we go on cooling below 0° C.? If pure anhydrous saltpeter were to go on separating out until it were all out, there would be left at some temperature below 0° C. pure liquid water—an impossibility. If, on the other hand, only pure ice were to separate out, we should get at last anhydrous saltpeter liquid below 0° C.: this is equally impossible. What actually does occur is this—anhydrous saltpeter goes on separating out until the solution has acquired a certain degree of weakness (11·20 per cent.), and this stage is reached at a certain temperature below 0° C. (− 2·6). When still more heat is withdrawn, the temperature refuses to sink further, and the remainder of the solution begins to solidify, and continues to solidify at the temperature and with the composition it has reached until the last drop is solidified.

If, again, we begin with a very dilute solution of saltpeter, say one containing 2 oz. of saltpeter in 98 oz. of water, it is well known that such a solution requires to be cooled below the freezing point of water before solidification begins; and the differences of opinion which have prevailed as to whether pure ice or "impure" ice is separated in such cases arise apparently from the circumstance that solutions of different strength have been examined. From our solution, pure ice is separated at a little below 0° C. And as the temperature falls, more and more ice separates out, thus enriching the remaining solution. But this cannot go on indefinitely, for if it did so we should have at last anhydrous saltpeter liquid at some temperature below 0° C. The enrichment goes on as the temperature falls, until the same temperature and the same composition are reached as were reached in the case of the impoverishment of the saturated solution by the withdrawal of the saltpeter. And now again the temperature ceases to fall, the salt solution ceases to change its composition, the water and saltpeter solidify together at the same temperature and in the same ratio as they did before, until the last drop is solid.

What manner of body is this which is thus formed? A solid, of crystalline form, consisting of water and saltpeter in fixed ratio, and of constant freezing and melting points. A hydrate obviously, and because it can only exist in the solid form below the freezing point of water, we may call it a cryohydrate.

Thirty or forty of the most familiar soluble salts have been examined in a similar manner and with similar results. Each combine with a certain proportion of water at a certain temperature below zero C. The proportions are different with different salts, and so are the temperatures of solidification, and at present I can only see indications in a few cases of generalizations connecting the chemical compositions with the temperatures.

The Table A and Diagram B now nearly completely explain themselves.

TABLE A.—Shows (1) The CHEMICAL FORMULA of the SALT; (2) The LOWEST TEMPERATURE to be got by mixing the SALT with ICE; (3) TEMPERATURE of SOLIDIFICATION of the CRYOHYDRATE; (4) MOLECULAR RATIO between ANHYDROUS SALT and WATER of its CRYOHYDRATE (Water-worth); (5) PERCENTAGE of ANHYDROUS SALT in CRYOHYDRATE.

(1) Formula of Salt.	(2) Temperature of Cryogen.	(3) Temperature of Solidification of Cryohydrate.	(4) Molecular Ratio or Water-worth.	(5) Perc[illegible] Anhydr[illegible] in Cryohydrate.
	°	°		
$CaCl_2$	− 33	− 37	11·8	36·45
NaBr	− 28	− 24	8·1	41·33
NH_4I	− 27	− 27·5	6·4	55·49
NaI	− 26·5	− 28	5·8	59·45
KI	− 22	− 22	8·5	52·07
NaCl	− 22	− 22	10·5	23·60
$SrCl_2 + 6H_2O$	− 18	− 17	22·9	27·57
$NH_4{}^2SO_4$	− 17·5	− 17	10·2	41·70
NH_4Br	− 17	− 17	11·1	32·12
NH_4NO_3	− 17	− 17·2	5·72	43·71
$NaNO_3$	− 16·5	− 17·5	8·13	40·80
NH_4Cl	− 16	− 15	12·4	19·27
KBr	− 13	− 13	13·94	32·15
KCl	− 10·5	− 11·4	16·61	20·03
K_2CrO_4	− 10·2	− 12	18·8	36·27
$BaCl_2 + 2H_2O$	− 7·2	− 8	37·8	23·2
$AgNO_3$	− 6·5	− 6·5	10·09	48·38
Sr_2NO_3	− 6	− 6	33·5	25·99
$MgSO_4 + 7H_2O$ ..	− 5·3	− 5	23·8	21·86
$ZnSO_4 + 7H_2O$ ..	− 5	− 7	20·0	30·84
KNO_3	− 3	− 2·6	44·6	11·20
Na_2CO_3	− 2·2	− 2	92·75	5·97
$CuSO_4 + 5H_2O$ ..	− 2	− 2	43·7	16·89
$FeSO_4 + 7H_2O$ ..	− 1·7	− 2·2	41·41	16·92
K_2SO_4	− 1·5	− 1·2	114·2	7·80
$K_2Cr_2O_7$	− 1	− 1	292·0	5·30
$Ba2NO_3$	− 0·9	− 0·8	259·0	5·30
$Na_2SO_4 + 10H_2O$ ..	− 0·7	− 0·7	165·6	4·55
$KClO_3$	− 0·7	− 0·5	222·0	2·93
$Al_2NH_42SO_4 + 12H_2O$	− 0·4	− 0·2	261·4	4·7
$HgCl_2$	− 0·2	− 0·2	450·0	3·24

In Diagram B are shown, as well as the cryohydrates (which are the points of reflexure, or lowest points of each curve), the temperatures at which various salts of various strengths (*a* left-hand branches) give up ice and (*b* right-hand branches) give up salt.

Let me only remark concerning these ratios of Table A, that those bodies at the bottom of the list are nearly pure ice, and yet by the generalization of continuity they are bodies of similar composition to those at the top of the list which are well within the limits of chemical ratio. One is tempted to look upon these ratios as being brought about not by the same saturating capacity as determines

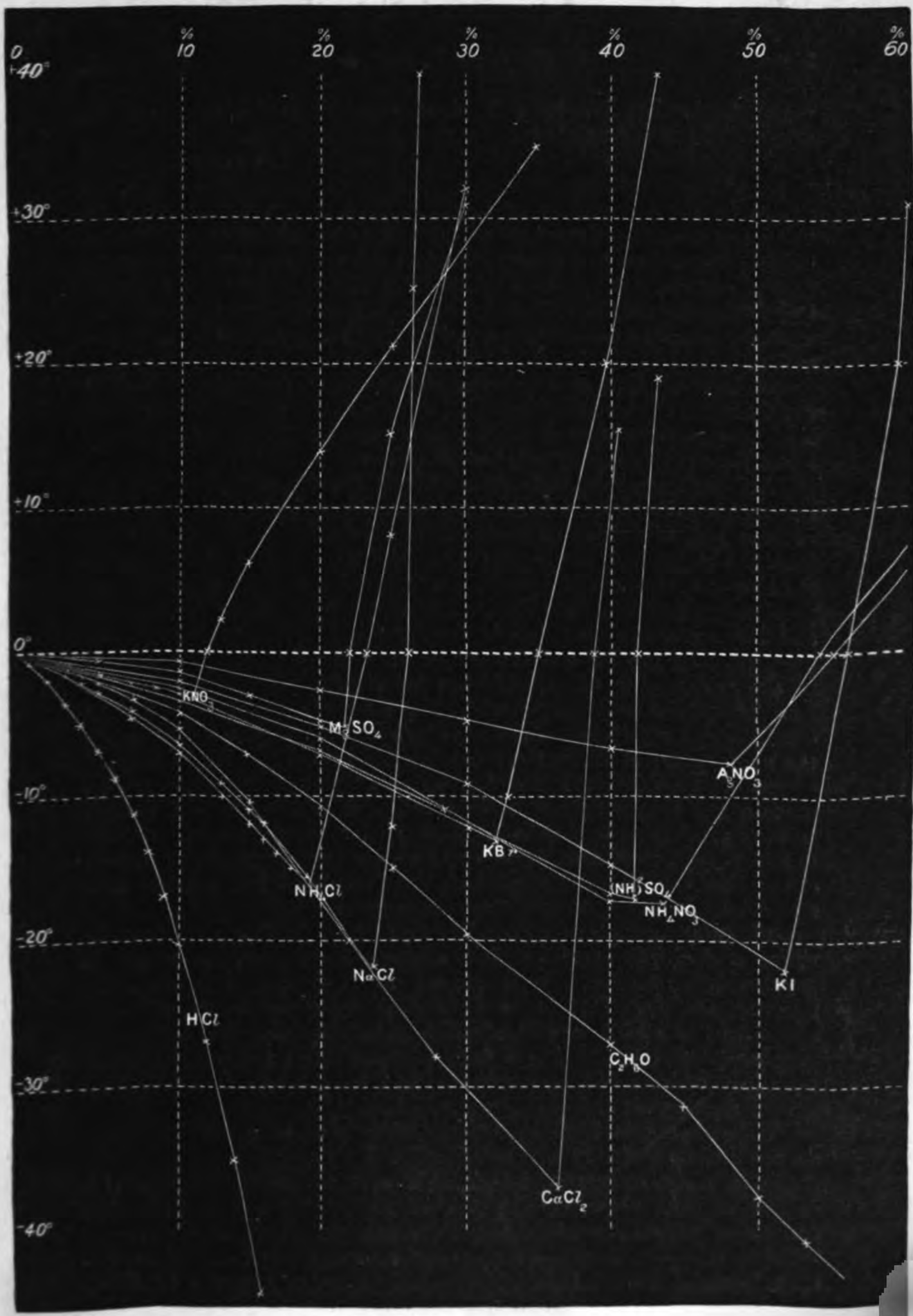
0
% 10
% 20
% 30
% 40
% 50
% 60
+40°
+30°
+20°
+10°
0°
-10°
-20°
-30°
-40°
KNO3
MgSO4
AgNO3
KBr
NH4Cl
(NH4)2SO4
NH4NO3
NaCl
KI
HCl
C2H6O
CaCl2

Diagram B.

elementary union, but by a kind of crystallographic relationship, of which I can say no more because no more is known.

Very palpable evidence of what we may in this generation call physical relationship as distinguished from chemical relationship, is shown in the composition of innumerable minerals, and notably in the water quantity of various silicates, the composition of which can only be reconciled to ordinary chemical ratio by the greatest licence in arithmetical manipulation. I say, "in this generation," for haply the time practically predicted by Berthollet at the beginning of the century cannot be remote when the barrier between the two sciences will vanish.

Is it likely that so numerous and interesting a class of bodies as these cryohydrates should be mere creatures of the physical laboratory, and be without a function in nature? I trow not. They affect the composition of polar ice as follows.

Although sea water has no maximum density, and therefore ice can be formed anywhere in its mass, yet it loses heat mainly from the surface, and it is there that ice is chiefly formed. If the fall of temperature be gradual, the crystals of pure water which solidify are for the moment surrounded by cold sea water deprived of a portion of pure water, that is enriched sea water—a stronger brine. This sinks and diffuses, and gives place to fresher sea, which in its turn yields pure ice. By gravity and osmose, in time, the pure sea is renewed in the region of gelation, and pure ice results. But if the loss of heat be sudden and considerable, the salts of the sea are fixed as cryohydrates with the water, and so perpetuated *in situ* according to the temperature at which their cryohydrates solidify. And I should expect that paleocrystic ice formed from the freezing sea would contain the metals sodium, magnesium, and calcium in a different relative proportion to that in which they exist in the sea water. Be this, however, as it may, it is clear to me that no theory of ocean circulation can be complete which does not take into account the formation of cryohydrates in the polar regions.

These compounds of salts with water have both a great use in many a familiar operation, and a great significance in many a familiar phenomenon. With regard to their use, I need only mention the fact, that it is clear that by their means we can attain to and maintain with absolute constancy many definite temperatures below 0° C. For a body plunged into a melting or solidifying cryohydrate will be maintained at a temperature as constant as that of melting ice or freezing water.

With regard to their significance, they give us a complete key to the hitherto closely concealed *rationale* of freezing mixtures or cryogens made by mixing ice or snow with various salts. For the degree of cold which can be reached on mixing a salt with ice can never exceed in lowness the temperature at which the cryohydrate solidifies, because the consequent solidification of the cryohydrate would furnish heat. Nay, more. Since of all ratios between the salt and the water that of the cryohydrate demands the lowest temperature for solidification, the liquid portion of a freezing mixture can neither be stronger nor weaker than the cryohydrate, it is the cryohydrate and the slightest further loss of heat causes solidification. Look now at columns (2) and (3) of Table A, and you see how closely parallel, how, indeed, within the limits of experimental error, identical are the two series of numbers.

By cooling a thick slab of glass in a freezing mixture, and dropping upon it various salt solutions, I show you now a few cryohydrates in the act of solidification.

I may mention that not only metallic salts, but crystalline solids, of organic and inorganic origin form similar cryohydrates. Amongst the most interesting of these are the cryohydrates of alcohol and ether. The latter contains a large proportion of water, and its temperature of solidification is so little below zero C. that I can show it you. Solidified in a test tube, and removed therefrom, it forms a white crystalline rod, like a candle. The ether burns away when a light is applied to one end, and, by its non-luminosity, clearly illustrates the generalization of Dr. Frankland, that, other things being the same, a cold combustible burns with less luminosity than a hot one.

Let me here turn for a moment to the comparison between the effects of heat and cold upon salt solutions, and notice how closely parallel are the two series of phenomena.

Compare the decomposition of a salt solution by the loss of heat with the decomposition by gain of heat when such a solution boils. And in instituting this comparison, we must bear in mind how much more sensitive to variation in pressure is the boiling than the solidifying point.

And before quitting this part of the subject, I may call to mind

(1) A solution poorer than the cryohydrate loses heat; ice is formed.	(1) A solution poorer than that saturated at a given temperature receives heat; vapour is formed.
(2) This goes on until the proportion of the cryohydrate is reached, the temperature falling.	(2) This goes on until saturation is reached, the temperature rising.
(3) The cryohydrate may be reached by freezing out ice from a weaker solution, or by any other withdrawal of water.	(3) Saturation may be reached by evaporation, boiling, or any other withdrawal of water.
(4) When ice separates from a liquid, it remains in contact with the liquid, and endeavours to redissolve therein.	(4) Vapour separated from a liquid is removed from the field of contention, unless the liquid be enclosed with the vapour.
(5) When by the separation of ice the proportion of the cryohydrate is reached (nearly independent of pressure), ice and the salt separate simultaneously.	(5) When by the separation of vapour the proportion of saturation is reached (very dependent upon pressure), vapour and the salt separate simultaneously.

(6) The two bodies (ice and the salt) being crystallizable solids, unite to form a crystallizable cryohydrate which exhibits a constant gravimetric composition.	(6) One being a solid and the other a vapour, they do not unite, but in their separation preserve a constant gravimetric ratio under like conditions of pressure.
(7) A cryohydrate in the act of solidification shows identity of composition between the solid and liquid portions. The temperature of solidification is constant.	(7) A saturated solution, when boiling, shows the same ratio between the vapour formed and the salt precipitated as exists between the liquid water present and the salt it holds in solution. The temperature of boiling is (under like pressure) constant.

the fact that we find amongst the phenomena of igneous fusion facts perfectly analogous with those which we have been studying. Thus, in Pattinson's process for the desilverizing of lead, the mass containing a small percentage of silver is melted and gradually cooled, lead (ice) separates out in almost perfect purity, the temperature sinks, and more and more lead is separated, until, as Dr. Percy informs me, a two and a quarter per cent. of silver lead alloy, a pyro-plumbide (a cryohydrate), is reached.

Lastly, let us reflect upon the solidification of water in jellies, and its attitude towards colloid bodies.

My illustrious teacher Graham, in a series of researches which have perhaps never been surpassed for philosophical insight, showed how all matter is divisible into two great classes, crystalloid and colloid. He proved that a crystalloid body penetrates through a colloid without essential obstruction. The colloid and crystalloid have no grip on one another. And quite in accordance with Graham's views I find that, although gum-arabic is far more soluble in water than table salt, we totally fail to make a freezing mixture with gum and ice or snow. And inversely, when a strong solution of gum or glue is cooled, the temperature cannot be brought below 0° C. until the whole of the water has been separated as ice. This being so, we ought also to find that the presence of gum or glue in water does not raise its boiling point, as do the presence of crystalline salts. Experiment shows us that the boiling point of water is really and considerably lowered, so that if, for instance, I remove the atmospheric pressure from two vessels slightly warmed, the one containing 50 per cent. of gum and the other pure water, the gum solution is the first to boil.

In the series of barometer tubes on the table, whose images I throw on the screen, you have (1) an ordinary barometer, (2) a similar barometer with a crystal of alum in the vacuum, (3) with a saturated solution of rock salt, (4) with water, (5) with a morsel of size, and (6) with a 50 per cent. solution of gum-arabic. It is observed that the depression of the mercury, which of course measures the vapour tension, is in the order mentioned. The solid water of crystallization leaves the alum, but is partly restrained by the affinity of the residue. The saturated salt solution again, but more feebly, restrains the water, while the gum and size do not restrain the vapour tension of the water at all; they have no grip on the water.

Even through the colloid caoutchouc, of which the toy air-balls are made, water penetrates with marvellous facility. The one on the table, which weighs now about 700 grams, weighed 750 grams only a few weeks ago. It has been losing 0·06 grams per hour with remarkable regularity. And I suppose, if we could see the structure of a jelly, we should find it to consist of a network of solid fibres hampering the motion of the crystalloid liquid among them; or of cells of solid elastic matter, containing liquid such as the mass of cells on the table.

The hour is up, and I must end. My task will not have been essayed in vain if I have brought forward evidence that, in the philosophy of the simplest things, yet much remains to be disclosed. Far from me the thought of undervaluing the labours of those who from time to time enrich science with hundreds of new substances of complex composition. On the contrary, I admire, I am grateful to them for their successful efforts; for I am thoroughly assured that it is by the comparison of terms of series, which terms stand apart from one another by almost imperceptible differentials, that the great integral curves of natural generalizations can be traced out. And yet I say, and earnestly maintain, that in the philosophy of the most familiar properties of the most familiar things, there are, as it were, lying at our very doors, vast and unexplored regions; meads brightly blooming with the unplucked flowers, and orchards mellow with the ungarnered fruits of natural truth.

[F. G.]

WEEKLY EVENING MEETING,

Friday, February 23, 1877.

SIR W. FREDERICK POLLOCK, Bart. M.A. Vice-President, in the Chair.

JOHN FLETCHER MOULTON, Esq.

Matter and Ether.

[Abstract Deferred.]

WEEKLY EVENING MEETING,

Friday, March 2, 1877.

JOSEPH DALTON HOOKER, C.B. M.D. D.C.L. Pres.R.I. Vice-President, in the Chair.

PROFESSOR HUXLEY, LL.D. Sec.R.S.

The Natural History of Birds.

[Abstract Deferred.]

GENERAL MONTHLY MEETING,

Monday, March 5, 1877.

GEORGE BUSK, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

John Laidlay Bashford, Esq.
Miss Anna Frances Busk,
James Farmer, Esq.
John Harris, Esq.
Rev. Francis Fraser Hird,
James Knowles, Esq.
John Fletcher Moulton, Esq.
Capt. Hugh Gilliat Oldham,
Francis Richard Philp, M.D.
Carl H. Siemens, Esq.
Mrs. C. F. Stovin,
William George Stuart, Esq.
Augustus Weiss, Esq.
George F. White, Esq.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Asiatic Society of Bengal—Journal, 1876, Part I. No. 1. Part II. No. 3. 8vo.
Proceedings, 1876. No. 8. 8vo.

Blackie, W. G. Esq. (Secretary of Local Committee, British Association, Glasgow)—
Western Scottish Fossils, Catalogue. 16to. 1876.
West of Scotland Fauna and Flora. 16to. 1876.
West of Scotland Principal Manufactures. 16to. 1876.

British Architects, Royal Institute of—Sessional Papers, 1876–7. Nos. 5, 6. 4to.
Chemical Society—Journal for Feb. 1877. 8vo.
Christiania University—Memoirs, &c. 4to and 8vo. 1876.
Civil Engineers' Institution—Minutes of Proceedings, Vol. XLVII. 8vo. 1877.
Editors—American Journal of Science for Feb. 1877. 8vo.
  Athenæum for Feb. 1877. 4to.
  Chemical News for Feb. 1877. 4to.
  Engineer for Feb. 1877. fol.
  Horological Journal for Feb. 1877. 8vo.
  Journal for Applied Science for Feb. 1877. fol.
  Nature for Feb. 1877. 4to.
  Nautical Magazine for Feb. 1877. 8vo.
  Pharmaceutical Journal for Feb. 1877. 8vo.
  Telegraphic Journal for Feb. 1877. 8vo.
Franklin Institute—Journal, No. 614. 8vo. 1877.
Genève, Société de Physique—Mémoires, Tome XXIV. Partie 2. 4to. 1875–6.
Geological Institute, Imperial, Vienna—Jahrbuch, 1876. No. 3. 8vo. 1876.
  Verhandlungen, 1876. Nos. 11, 13. 8vo. 1876.
Geological Society—Quarterly Journal, No. 129. 8vo. 1876.
New South Wales Government—G. H. Reid: Essay on New South Wales. 8vo. Sydney, 1876.
Photographic Society—Journal, New Series, Nos. 3, 4, 5. 8vo. 1876.
Royal Society of London—Proceedings, No. 177. 8vo. 1876.
St. Petersburg, Académie des Sciences—Bulletins, Tome XXIII. No. 1. 4to. 1876.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, Feb. 1877. 8vo.
Twining, Thomas, Esq. M.R.I.—Public Libraries in the United States. 2 Parts. 8vo. 1876.
University of London Senate—Catalogue of the Library. 8vo. 1876.
Vaux, W. S. W. Esq. M.A. F.R.S. M.R.I. (the Author)—Persia: from the Earliest Period to the Arab Conquest. 16to.
Victoria Institute—Journal, No. 40. 8vo. 1876.
Zoological Society—Transactions, Vol. IX. Part 10. 4to. 1876.

WEEKLY EVENING MEETING,

Friday, March 9, 1877.

Admiral Sir Henry John Codrington, K.C.B. Manager, in the Chair.

F. J. Bramwell, Esq. M. Inst. C.E. *M.R.I.*

PAST PRESIDENT INST. MECHANICAL ENGINEERS.

The Future of Steel.

With a few exceptions, construction (whether of building, of bridges, or of ships) was, not so very long ago, carried out by the employment of stone, of brick, or of timber. Metals played but an insignificant part in any kind of construction. Lead probably was the most used of all, being employed in roofing. If sheathing may be taken as part of the structure of a ship, then it would seem that lead was thus structurally employed, occasionally at all events, as far back as the reign of Trajan; while the metal copper, now so largely used as sheathing to wooden ships, was first applied in the Royal Navy in 1758, in the 'Alarm' frigate, and had been adopted for ships of every class in that navy by the year 1783. But the metal "iron," with which and with its offspring, steel, we are to-night concerned, was practically not used as a portion of the structure, either of houses, of bridges, or of ships, except, indeed, in the way of fastenings, bolts and nuts, screws and nails, to unite other materials together. Gradually, however, cast iron began to enter into structures. As far back as the year 1779 the celebrated bridge at Coalbrook Dale was built, not long after followed by the Sunderland bridge, and early in the present century by several others, including, in 1819, that magnificent structure Southwark Bridge, the work of the elder Rennie, which bridge still adorns the metropolis. Cast iron also was used in buildings to replace stone columns and wooden story-posts for direct pressure, and also as a substitute for wood in beams. In Burwash church, and in other churches in Sussex, may still be seen cast-iron tombs, dated as long back as the fourteenth century.

Wrought iron took its first important place as a structural material in the building of vessels. In 1787 an iron canal-boat was made in Staffordshire; and, indeed, the first iron steamboat, the 'Aaron Manby,' was produced in 1821 in that most inland of inland places, the Horseley Works.

By the year 1830, however, iron ship-building had gone to its appropriate home, that of a large port; for by that date Mr. Laird's works at Birkenhead, which were founded in 1824, were in full operation, and the first iron vessel had been built there, followed shortly by iron steam-vessels, the precursor of the large fleet which owes its origin to these justly celebrated works. From the small beginnings in Staffordshire and at Birkenhead sprang that wonderful wrought-iron navy now to be found on every sea, a navy which, whatever may be the ownership of the vessels, has in most cases Great Britain for its birthplace.

But while both cast and wrought iron were being thus extensively used, steel was not employed as a structural material at all. Steel was a luxury; it was made in small portions, it was sold at high prices, as much as a shilling or eighteenpence per pound, or even more; and it was employed, as we know, for swords, cutlery, surgical instruments, watch-springs, mechanics' tools, needles, and other purposes such as these, where the quantity used was but trifling, and where the importance of superior material was such as to justify the large expenditure incurred. It was felt in these days, as, indeed, it had been for ages past, that steel was worth paying for because it was to be trusted; indeed its trustworthiness had passed into a proverb, "As true as steel."

Iron we nowadays use in three great divisions, *cast iron*, *wrought iron*, and *steel*. Cast iron is, again, divisible into ordinary cast-iron, chilled cast-iron, and malleable cast-iron. Wrought iron is also divisible into ordinary wrought-iron and case-hardened wrought-iron. Steel, so far as I know, is not thus divisible; but it may be of very varied composition, producing qualities ranging from those which possess great flexibility, even when quenched in cold water, to those which exhibit intense brittleness when so treated.

Cast iron, or *pig iron* is, as you know, produced by putting iron ore with coke or raw coal and suitable fluxes into a blast furnace, a structure which has now, in the Cleveland district, reached to 100 feet in height by as much as 30 feet in diameter, having a capacity of over 40,000 cubic feet. Up through this enormous pile of materials a powerful blowing engine sends air heated, in the most approved furnaces, to as much as 1250°. The ore, as it descends through the furnace, is first deoxidized; and then the sponge of iron so produced is impregnated with such a dose of carbon as to render the iron fusible, so that it trickles down into the hearth of the furnace, from which, at intervals varying in different districts from four to as much as twelve hours, it is tapped out into the pig bed, the earthy matter of the ore, of the fuel, and of the flux flowing away as a coarse glass or cinder. In these days, in the best furnaces the unconsumed carbonic oxide is no longer allowed to blaze away from the top of the furnace, but is conveyed therefrom by suitable pipes, and is employed to heat the blast, and to generate steam for the blast engine. According to the proportion of fuel employed, and to the treatment, so will the issuing pig iron be more or less charged with carbon.

Pig iron is in common distinguished by numbers. I have on the table before me, due to the courtesy of the authorities at the War Office, two sets of samples of pig iron ranging from Nos. 1 to 8, No. 1 being the iron which will the most readily melt, and will become the most fluid, and make the sharpest but weakest castings, while the higher numbers are never used for ordinary foundry purposes, but are taken as proper irons to be converted into wrought iron. Anybody who examines these specimens closely will see quite clearly in the No. 1 iron the flakes of graphite separated out, and lying between the crystals of the cast iron. These flakes become less and less obvious as the numbers get higher and higher, until with the No. 5 pig the graphite ceases to be easily visible, the whole of the carbon appearing to be combined.

Leaving impurities out of consideration, pig iron or cast iron is the element iron accompanied with a very large proportion of carbon. In the case of No. 1 as much as (taking the total of combined and uncombined carbon together) from 4 to 4½, or even more, per cent., while in the case of No. 8 the carbon is only probably from 2 to 2½ per cent.; and although no portion of this is visible as graphite, the carbon, even here, is only partially combined with the iron; complete combination with the carbon must be looked for when the cast iron is in another condition, "chilled," such as is one end of the sample I have here. You will see that one end of this bar has been chilled, and that the other is not chilled. The chilling has been effected by pouring the fluid cast-iron into a mould, one portion of which was made of metal which conducted the heat away rapidly, and thus caused the iron to set quickly and to chill; while the other end of the mould was made of sand, and there the metal cooled slowly, and did not chill. You will also observe that the chilling, as it is called, has penetrated about one-eighth of an inch; and on trying it with the finest file, you will find it impossible to cut it, while the other end of the bar may be filed with ease. The hardness is due, I believe, to the iron being chilled by the metal mould, and being thus set into a solid before the carbon has had time to separate out of it, in the form of graphite, by liquation. As a striking instance of chilling, I would call your attention to a split-open chilled Palliser shell now on the table.

I have here a sample bar of *malleable cast-iron*. This bar has been made malleable at one end, but has been left in the ordinary state of cast iron at the other. The endowment with the malleable property has been due to the continued heating of the bar when placed in a close vessel, and surrounded by iron ore. The oxygen of the ore has partially decarbonized the metal, and has thereby caused it to approach to the condition of wrought iron, rendering it, as you will see, malleable. I have here a sample bar of malleable iron, which has been forged.

Wrought iron is nowadays made by taking pig iron and putting it into the hearth or bath of a puddling furnace, where it is melted, and then is stirred about, commonly by tools called "rabbles" worked by hand, but occasionally lately by "rabbles" worked by mechanism; or, as the latest improvement, it is agitated in a furnace of a special construction, where the body part is put into revolution like a barrel-churn. In whatever way the agitation may be effected, the object is to cause the oxygen of the material (iron ore or other oxide of iron) with which the furnace is lined (the "fettling," to use the technical term) to unite with the carbon of the pig, and form carbonic oxide, which, rising to the surface, keeps the whole bath of metal in a state of violent ebullition or "boil," as it is called, the gas igniting on the surface and producing a multitude of lively blue flames. The submerging by the "rabble" of any oxidized portions of the surface, assuming that there may be at times free air in the furnace, also perhaps aids in the decarbonizing process. This process goes on until, practically, the whole of the carbon is driven off, about one and a half tenths to two-tenths of 1 per cent. only remaining; and then the iron becomes pasty, it being no longer possible for it, when deprived of the carbon, to remain fluid in the heat of an ordinary puddling furnace, and the workman is enabled to gather it up into balls, making probably four or five balls out of a 4 cwt. charge. For the purposes of this evening it would suffice to speak of cast iron as though it contained no foreign ingredient but carbon; but I need hardly say that this is not so, and that in practice the presence in the puddling furnace of silicon, phosphorus, sulphur, and occasionally other matters, has to be considered; and I will venture to occupy a few minutes by asking your attention to diagrams representing the nature and the extent of the impurities which in practice have to be dealt with in the puddling processes.

As I shall have occasion to remark later on, when speaking of the due appreciation of such expressions as 0·001 or one-tenth of 1 per cent., I have a great belief in the value of models or diagrams that the eye can readily appreciate. Such contrivances I find to be great aids to ready comprehension and also to steadfast retention of facts and of proportions. Mr. Head, of Middlesborough, who has done so much to apply science to the industry of iron, has kindly lent me the diagrams with which he illustrated, last year at the Mechanical Engineers' Institution, his observations upon that which takes place in the act of puddling.

You will see that the tall diagram to which I am now pointing indicates by its height, 100 parts, by weight, of the charge from the introduction of the solid pig-iron into the puddling furnace down to the puddled bar itself, after it has been hammered and rolled. The width of the diagram represents time from 10h. 23m., when the pig iron was put in, to 12h. 42m., when the puddling was finished; 2 hours and 19 minutes. The element iron is represented by the blue tint, while the carbon, silicon, phosphorus, manganese, and sulphur are shown by various colours. A near inspection of the diagram will enable you to ascertain not only how these matters diminish in puddling, but the rate and the order of their diminution. The fellow large diagram represents the changes of the fluid cinder in the furnace taken at various times; and you will see that while the impurities have been leaving the iron, they have been entering to a large extent into the cinder. The cinder being composed of oxides, each of the colours indicating the oxide of iron and the other oxide constituents has been made of a dark shade and of a light shade, the dark representing the base, and the light shade the accompanying oxygen. Besides the two principal figures, there are drawings to a large scale of the impurities both in their contorted form, due to their being drawn as superposed, and also as each constituent would appear when laid off on a level datum-line.

Just one caution in respect to these diagrams. They do not represent the variation in the total quantity of charge and of cinder (as time goes on, this it is almost impossible to exhibit), but they represent this, that if at the respective times 100 parts be taken from the charge, they will be found to contain the percentages stated. If this be not borne in mind, it will be difficult to understand why it is that there appears to be, during the first hour and more, an increase of carbon. This is not an actual increase, but a relative increase, due to the decrease of other matter. I think you will concur with me in thanking Mr. Head for this excellent mode of graphically exhibiting the changes and the sequence of the changes which take place in the act of puddling.

To revert to that which I was saying about *puddling*. During the balling process a very considerable waste of the iron ensues by oxidation; the spongy mass is permeated by cinder, oxide of iron, and earthy matter, and in this condition it is put under a hammer called a shingling hammer, where, by dexterous manipulation, it is sought to drive out the cinder from the mass, and to weld together the particles of wrought iron into a bloom. This bloom, commonly at the same heat, but sometimes after being reheated, is passed through rollers, and is then known as puddle bar. These bars are cut up, are piled into piles of the size desired for the intended production, are reheated to a welding heat, and are then either hammered and subsequently passed through rolls, or else are passed through rolls direct, making No. 2 iron, the ordinary iron of commerce. Occasionally, for special purposes, the piling and reheating and rolling are repeated. The material produced, when uncontaminated with phosphorus or sulphur or silicon, is the iron which, as I have said, has about one and a half tenths to two-tenths of 1 per cent. of carbon remaining in it, and it is this material that for so many years past we have trusted to form the hulls of our ships, to form the shells of our boilers, to bear their 140 lb. pressure per square inch, as they do on every railway, to form our 81-ton and 100-ton guns, to form the rails of our railways, and to form bridges and viaducts, and, in fact, to be applied to those purposes where toughness, ductility, flexibility, conductivity, cheapness, and trustworthiness were the desiderata. Wrought iron has the great advantage that it can be worked at almost any temperature between that of the atmosphere (in the best kinds of iron) and that of a high welding heat. It is a very fair conductor of heat where it is necessary to transmit heat, as through the sides of boilers, and it possesses that excellent property of union by welding. But the best of welds, or all but the very best of welds, are treacherous; the manufacturer never can be sure that in the interior of the weld some foreign substance, oxide of iron or sulphide of iron, is not interposed between the two surfaces, and thus he never knows whether the weld which looks so sound upon the outside is really sound throughout. To show you how difficult it is to ensure soundness in welding, I have on the table before me a sample of a wrought-iron rail, which, I doubt not, when it came out of the rolling mill, appeared to be perfectly sound metal, metal of uniform structure from side to side. But what is it now that it has been subjected to the action of engine and carriage wheels? a bundle of fibres, a mere faggot. These severances have not taken place through the solid metal; they have taken place through what were supposed to be sound welds in the pile.

I have here another sample of separation at the weld. This is a piece of "shelly" boiler-plate. The layers of the pile from which this plate was made have, under the action of the fire, separated at the weld, and the result has been that the heat-conducting power of the plate has become impaired, and it has blistered and has been rendered useless, and, indeed, unsafe.

For certain purposes, when it was desired to have the hardness of hard steel combined with the toughness of wrought iron, *case-hardening* was resorted to. This was effected by surrounding wrought iron with some suitable substance, commonly animal charcoal or bones, enclosing the whole in an air-tight vessel, and heating for a considerable time. The result was to carbonize and thereby steelify or case-harden the surface. The axle-trees of all our carriages are thus made at the present day.

I have on the table a wrought-iron bar, one end of which is case-hardened, while the other is left in its natural condition; and you will find that the case-hardened portion is unattackable, even by a fine file, while the other parts may be readily cut.

Steel, in those days when steel was a luxury, was made, and, indeed, is still made, as, again, no doubt you know, by taking the wrought iron from which the workman had laboriously ejected the carbon, putting that wrought iron into air-tight firebrick boxes containing charcoal, and heating the iron there during many hours. Under these circumstances the iron, as in the instance of the case-hardening, took up carbon, and then, the whole being suffered to get cool, the bars were withdrawn and were found to be covered with vesicles, and thereupon were called *blistered steel.* I have a sample of such a bar before me. Until the middle of the last century these bars were heated and forged, and were made into the articles required, and were called shear steel; and if heated and welded together and re-forged they were double shear-steel. With such a process of manufacture as this the product was of necessity very variable in quality, it was irregularly carbonized, it was subject to the bad welds that I have mentioned, and it was only due to the experience and dexterity of the manipulator that useful steel was obtained. About the middle of the last century, however, Huntsman made the great invention of *cast steel.* I have already told you that the heat of an ordinary puddling furnace, or, indeed, any of the heats of those days, was insufficient to keep iron, when deprived of nearly all its carbon, in fusion, it being the carbon that gives the ready fusibility to cast iron. But Huntsman discovered that the carbon in steel was sufficient, if a very high heat were got, to admit of fusion. By his process the blister-bars were broken up, they were examined to judge by their fracture as to the amount of carbon they had taken in, and they were then put into covered crucibles heated by coke fires, where, after many hours' intense application of heat, and with a large consumption of fuel, the pieces were melted and a charge of 50 or 60 lb. was obtained. The pots were then laboriously lifted out by tongs, the legs of the workman being enveloped in wet bandages to prevent him from being scorched or from having his clothes set on fire.

The cover being removed from the pot, the fluid steel was poured out into an ingot mould, producing an ingot of cast steel. These ingots were drawn out under the tilt-hammer or were rolled, and in that way the steel of luxury, of which I have spoken, was made. This cast steel had the great advantage over the shear steel of being, at all events so far as each ingot was concerned, a substance of homogeneity; but the expense was very great; there was the expense of the crucibles, of the labour, and above all of the fuel. As much as three tons of the highest-priced coke were required to melt a single ton of steel, even if it were harsh steel, which, from its containing the greatest amount of carbon, is the most fusible; and still more was required if it were desired to melt the milder qualities of cast steel. For many years, indeed, these mild qualities were not made, and it was long held that cast steel was not weldable, the fact being that the cast steel of those days was all too highly carbonized to admit of its bearing, when at a welding heat, the stroke of the hammer. After a time, however, the furnaces and pots were improved, and steel so mild as with great care to admit of welding was produced, and was looked upon as a great wonder,—weldable cast steel. I may say that pot steel contained probably in the very harshest, such as that employed for making files, 1·0 to 1·2 per cent. of carbon, and in the very mildest or less fusible steel, five-tenths to six-tenths of 1 per cent. Not only was steel thus obtained expensive, but it was also limited to small pieces of 50 or 60 lb., the contents of a crucible; this objection, however, was overcome by, I believe, in the first instance, Krupp, who, by taking care to have a sufficient number of pots hot at once, and by drilling his men, succeeded, in 1851, in pouring the pots, pot after pot, into the desired ingot, so as to form from these petty spoonfuls, as it were, an ingot weighing as much as 4500 lb. This was exhibited in the Exhibition of 1851, and was the object of great admiration; by 1862, however, Mr. Krupp was enabled to exhibit an ingot perfectly sound, and of a weight of 20 tons.

But others had been attempting to improve the manufacture of steel; and among these I must bring to your notice the ingenious French chemist, Chenot, who essayed to get rid of the blast furnace and the puddling furnace altogether in the preparatory stages, and to make steel direct from the ore; and he not only essayed it, but he effected it. His plan consisted in putting a pure ore into an air-tight vertical chamber surrounded by a heated central zone, and so constructed that the ore could be put in at the top and could be drawn off at the bottom at a considerable distance below this zone of heat, and in a cool state. The ore, while in the chamber, was in company with powdered charcoal, which deoxidized the ore, so that when it was withdrawn from the bottom of the furnace it was a sponge of iron, that is to say, wrought iron in minute particles accompanied by the earthy matter of the ore. This sponge was ground, and a portion of it was treated while cold with resin or fish-oil, or some other carbonaceous matter; then the portion thus treated was mingled in any desired quantity with the untreated powder, the mixture was put into a machine which, in order to consolidate the powder and make it occupy less space, formed it into pellets of about the size and shape of wine corks, these were then placed in ordinary steel melting-crucibles and were fused. The earthy matter separated and floated on the surface as a viscous glass, while the fluid steel which remained below was poured into an ingot. Chenot called this process "cémentation à froid." The result was some of the very best steel I have ever seen. But although it would seem that the process of making steel direct from the ore should have been a cheap one, it was not so; the cost of the melting remained, and the process has not been pursued to any extent in practice.

About the year 1850 a German, Riepe, patented (as a communication) another mode of making steel, which was used to a very considerable extent in England; the product was known as puddled steel.

I have mentioned that in converting cast iron into wrought iron in the puddling furnace, the large quantity of carbon present in pig iron is practically all driven out; you will see, therefore, that there must be a time in the puddling process when the mass still retains just so much carbon as is required for steel. Riepe's patent was for stopping the puddling process at this point, and for bringing out the product as steel. The patent provided an ingenious plan for enabling the workman to make (as was thought) sure of the result; but the product was an uncertain one, and you will not wonder at this when I tell you that nowadays the steel manufacturer is careful of his proportions down to the twentieth of 1 per cent. of carbon.

We drift into the habit of considering 1 per cent. of anything as rather a small matter; 1 per cent. of phosphorus for example. But is 1 per cent. such a small matter, even when we compare bodies of equal specific gravities? I think, as I have already said, we can best appreciate such facts by the aid of diagrams or of models; and in this case, following a suggestion of my friend, Mr. Hawksley, past President of the Institution of Civil Engineers, and President of the Institution of Mechanical Engineers, I will in this instance use models.

You see I have here several coloured cubes. I will ask you to suppose that the large one (the blue one) represents wrought iron; it is exactly 1 cubic foot, 1728 in. I have a yellow cube and a black cube, each of them is 2·58 on the side, and contains therefore 17·28 in., or 1 per cent. of the cubic foot; and I have two smaller cubes, yellow and black, each of which is 1·2 on the side, and contains therefore 1·728 cubic in., or one-tenth of 1 per cent. of a cubic foot. Assuming the yellow and black cubes to represent respectively phosphorus and carbon, and that phosphorus and carbon had each the same specific gravity as iron, the eye tells us that 1 per cent. is a very appreciable amount, and that even one-tenth of 1 per cent. is by no means to be despised; but phosphorus and carbon are both lighter than iron. Phosphorus I have taken as occupying 4·28 times the bulk of iron, weight for weight, and carbon in the state of graphite 3·48 times the bulk of iron. Let me now, therefore, substitute cubes representing the bulk that 1 per cent. and one-tenth of 1 per cent. by weight of phosphorus and carbon would occupy; you will find the 1 per cent. of carbon is now shown by a cube of 3·92 on a side, and that one-tenth of 1 per cent. is represented by a cube of as much as 1·82 linear dimensions, while the cubes of phosphorus, which has a somewhat less specific gravity than graphite, being practically about one-fourth that of wrought iron, have attained to 4·19 in. for the 1 per cent., and to 1·95 for the one-tenth of 1 per cent.

Looking at these models, I think we more easily appreciate the influence that may be exercised by even only one-tenth of 1 per cent.

To revert to Riepe. He patented (also as a communication) another improvement in the steel industry, and an improvement which, unlike his puddling process, remains. Before his time any article in steel had to be made by forging, casting being confined to the forming of the mere ingot from which the forgings were produced. The difficulty in casting steel was this: if it were run into moulds of iron, such as those in which the ingots were made, it could not be a casting of any curved form that would embrace the mould, as that embrace would inevitably break the casting in the act of contraction; moreover, the moulds would be costly to make, while, on the other hand, steel could not be poured into moulds made of sand or loam such as are used for iron castings, because the excessive heat partially fused the material of the mould, and that material was also penetrated by spiculæ of steel, and the casting was therefore useless. I have experimented upon this subject, to try whether it were possible to cast steel in large masses (30 cwt. in a piece) in the most carefully prepared mould, such as is used for cast iron. I tried to make a toothed pinion for a rolling mill, and I thought I had succeeded; but when I endeavoured to clear out what I supposed was the sand from between the teeth, I found a mixture of sand and steel that was impossible to remove, and the casting was totally useless. Riepe's patent got over the difficulty in this simple way: the inventor said, "If your mould were faced with some material which has already been subjected to a heat equal to that of melted steel, then melted steel would not have much effect upon it; one of the best materials you can use is that of the old crucibles in which steel has been melted; grind these into powder." The objection to this was that such powder would not adhere, and the walls of the mould would tumble in. This objection was overcome by mixing with the ground crucible material an extremely small quantity of fireclay, just enough to cement the powder together. With these linings, moulds are competent to receive melted steel just as well as ordinary loam moulds are to receive fluid iron, and the casting comes out as clean; and in this manner we nowadays produce toothed wheels, screw propellers, and all sorts of castings in steel. This process was first taken up by Messrs. Vickers and Co., of Sheffield, and is now largely used by them and by other firms. I have samples here of such castings.

While efforts were being made to perfect the Puddled Steel process, Bessemer came with his great invention. Leaving out the early attempts, you know, of course, all of you in this Institution, that the present apparatus of Bessemer and his process are (briefly stated) as follows: An egg-shaped vessel, the convertor, with a curved spout at the top, is mounted upon hollow trunnions through which air is blown, and passing down suitable conduits at the sides of the vessel, is received into a chamber below a false bottom, containing numerous small holes, up through which the air issues into the egg-shaped vessel. The vessel, being heated, is laid down on its side; in this condition it receives molten pig-iron, the blast is put on and then the vessel is turned vertically, so that the molten iron is supported upon the perforated floor, if I may so call it, through which the air is issuing. The iron cannot flow down through the holes on account of the violence of the upward blast, and thus the molten iron stands on the top of the holes in the same way as the liquid in the upper part of a gazogene stands upon the perforated bottom of that vessel. The air in its passage through the molten iron combines with and burns out the carbon and silicon, generating heat in so doing, and thereby increasing the temperature of the fluid mass, a most necessary thing, as, from the loss of the carbon, that mass would set or become pasty, if it were subjected to no higher heat than that which it had when it was poured into the convertor. Further, the air burns a portion of the iron, and in this way there is produced the final intense heat requisite for retaining in the fluid state iron practically deprived of its carbon. As soon as the carbon has been driven off, and the metal has thus been brought into the condition of fluid wrought iron, the convertor is again laid down on its side, the blast is stopped, and a certain proportion of Spiegeleisen, that is, cast iron containing a large admixture of carbon and of manganese, is poured in. The carbon restores to the iron the quantity requisite to convert it into steel; the manganese plays a part in neutralizing the bad effects of the shut up, or, as I believe the scientific term is, occluded oxygen, and probably, as I shall have occasion to note hereafter, performs other useful offices. But on this obscure subject of the true function of the manganese I dare not at present to venture further.

By the addition of the Spiegeleisen the fluid iron has become fluid steel; this is poured into ingots, and those ingots are forged and rolled into rails, tyres, and other useful forms.

Now, I told you that for years steel was a luxury; I had also told you that it was a luxury people paid for, because in certain cases that which was to be trusted was worth any money; and that steel was to be trusted was a proverb, "As true as steel." But, unhappily, the time came when confidence in steel was lost, and its character was gone. The irregularity in the puddled steel did much to lower the character; and I regret to say that the employment of steel in civil-engineering structures, such as bridges, has been retarded until this very time (for indeed even yet the ban is not removed) by the Board of Trade, because of their experiments in the year 1860 with puddled steel, experiments that did not yield results superior to those obtainable from wrought iron. The Bessemer process, although infinitely better than Riepe's in the matter of accuracy, and a process which may be carried out with very considerable approach to certainty of result if great care be used, did not in the first instance repair the damaged reputation of steel as regarded uniformity. Do not for one moment imagine that I am casting any slur upon the Bessemer process. On the contrary, I wish it to be understood that it has no more ardent admirer than myself, and there is no one more ready to acknowledge the enormous stimulus that it gave to the steel trade, by making it possible to afford to employ steel for purposes from which the price had previously shut it out. But it was not unnatural, in the earlier days of the manufacture, that the persons engaged therein were not alive to the extreme delicacy required; and that thus steel was produced which, from charge to charge, varied two or three tenths of a per cent. of carbon. I have already called your attention to the fact that this, which, when stated in words, appears to be but a trifling matter, is seen to be, when judged of by inspection of models, a serious one; and practice proves that it is serious. Rails were made of such steel, and the result was that sometimes the rails were entirely satisfactory, while at other times rails which appeared equally good broke under the weight of a passing engine, and an accident ensued. Boilers were made of steel plates, and it was found that, after punching, the plates were so much weakened that they could not be trusted; while another boiler appeared to be superior to any that had ever been constructed of wrought iron. Tests were made, and great diversity was found. Some steel broke with 28 or 30 tons, others supported 50; and, as the result of these early investigations, steel got so bad a name that the prudent man began to think he must give up the idea of steel, and go back to iron. Steel manufacturers pointed to the fact that the average strength of steel was considerably in excess of wrought iron; but the engineer responsible for the safety of structures retorted, and with great effect: "The average strength won't do for me; an average may be made up of samples that may have a very high breaking resistance, and of others which have a very low one. How can I tell that in the structure I am making I may not have the low samples, or that I may not have them in certain parts which will be as fatal as though I had them all over? It will be a very poor consolation for some passenger maimed in a railway accident to be told that I had made my calculations upon the average, and that these calculations were perfectly right, but that unhappily he had suffered from going over a structure which contained a minimum sample." Obviously the thing needed in structures is not a high average alone, but a high average coupled with a minimum of variation, a certainty. It is only two years ago that Mr. Barnaby, speaking at the Institution of Naval Architects, after stating the precautions taken in the French Government yards in the working of steel for ships, said: "I, for one, should feel very doubtful about a ship built of it for myself, unless I could see every plate worked."

Mr. Barnaby was judging fairly enough from that which had been the case during the infancy of the great wholesale manufacture, but he was not giving credit for the advance which had been made by the able men who had been directing their intellects to the improvement of the manufacture and to the attainment of certainty. Among these men was one of the managers of this Institution, Dr. Siemens. He, as you know, some years before had perfected his excellent invention of the regenerative gas furnace,[*] by means of which he is enabled to attain any heat consistent with the limit of endurance of the furnace materials. Armed with this power, he has no difficulty in keeping pure wrought-iron in fusion, and in keeping it so without waste, because he can always ensure that the flame (being under control) shall be neutral, or even reducing. His process of making steel and the system he employs (as I have had an opportunity of seeing more than once at the Landore Steel Works, Swansea, and at other works) consists, according to one mode, in melting in a regenerative furnace several tons, 7, 9, 10, or 12, according to the size of the furnace, of pig iron. To this bath of fluid metal he adds from time to time about 25 per cent. of iron ore. The ore and the pig iron react one upon the other; the carbon of the pig iron and the oxygen of the ore unite so as to decarbonize the pig iron and to deoxidize the iron ore, the result being fluid wrought-iron, with scarcely a trace of carbon in it. This fluid wrought-iron has added to it an appropriate quantity of Spiegeleisen, and in that way the bath is converted into steel. The process takes about four hours for the melting of the pig iron, about three hours for the addition, by degrees, of the ore, and about one hour for taking out specimens and ensuring that the correct condition has been arrived at, the power of thus taking specimens and adjusting proportions being a great advantage in the Siemens process. Then the Spiegeleisen and the tapping require also about an hour, and another hour is employed in getting the furnace into condition for a further charge, so that about ten to eleven hours elapse from the commencement of one charge to the commencement of the next. Practically, including the short day on Saturday, thirteen charges of 7, 9, or 12 tons are obtained from each furnace per week. Owing to the ability of testing the steel material before it is tapped out, this mode of manufacture can be relied upon with the most absolute certainty to produce the very steel that the manufacturer had intended. If it is to contain two-tenths of 1 per cent. of carbon, he knows that it has got it, because he knows it was deprived of carbon down to say half of one-tenth, and then he is aware of exactly the amount which is put in. A further advantage is that, as regards one-fifth of the material (the iron ore), that fifth has never been contaminated by exposure to impure fuel or to impure fluxes. Moreover, the iron in it, which may be taken at, perhaps, 50 per cent. of the weight of the crude ore, is iron in the very cheapest of all forms. A further advantage is that the labour is reduced to a minimum. With the gas furnace, the furnace-man has nothing to do with fuel, for that is shot out of the railway waggon into the tops of the producers in a very wholesale manner and by unskilled men. The furnace-man's work, as regards heating the furnace, is confined to turning a tap as he would to light the gas in his own room, and to alternate the regenerator valves every half hour or three-quarters of an hour.

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[*] 'Proceedings of the Royal Institution,' June 20, 1862, vol. iii. p. 5[illegible]

The sole labour worth talking about consists in putting into the baths the 25 per cent. of iron ore during the three hours out of the ten. There is no rabbling, and, in fact, no laborious employment at all. But it may be said that the process compares unfavourably with the Bessemer in consequence of the amount of fuel used, for that in the Bessemer process no fuel is employed. This is not the place to go into commercial statistics; but if it were, it could be shown that the coal required to make the coke for melting the Bessemer pig in the first instance, for heating the convertor, for working the blowing engines, and for melting the Spiegeleisen amounts to from 13 to 14 cwt. per ton of ingots, while 10 cwt. of small coal made into gas suffices, by the Siemens process, to make 1 ton of ingots; moreover, the ultimate fuel in the Bessemer process is the combustion of a portion of the iron itself. These, however, are commercial details; but the fact is undoubted, that while by the Bessemer process, when conducted by skilled men thoroughly well acquainted with their business, trustworthy results can be obtained, by the Siemens process it requires very great cleverness indeed to obtain an untrustworthy result.

There is another mode in which the manufacture of steel by the Siemens process is carried out. This consists in melting pig iron as before; but instead of its being four-fifths of the total charge, it is now only two-sevenths. Into this melted pig-iron ore, deprived in a previous process of its oxygen, is introduced, either in the shape of spongy iron, or in a more consolidated form as a ball or bloom, produced direct from the ore in a rotatory furnace. In the absence of this deoxidized ore, and for the purpose of using up old materials, wrought scrap, such as worn rails and matters of that kind, or what are known as the "skulls," that is to say, steel which has set and adhered to the sides of the ladles, may be put into the baths. These readily melt down and dissolve in the high heat of a Siemens furnace when in contact with the carbonaceous pig-iron; then a small percentage of ore is added to get rid of the carbon of the pig. It has long been thought that phosphorus was absolutely destructive of good steel; but this is not so, provided that it be accompanied by an increase in the dose of manganese. Phosphorus appears not only to be harmless for many purposes, but to replace the carbon; and thus it is that if the old iron contain phosphorus, the bath being treated with ferro-manganese instead of the Spiegeleisen yields good steel, although there may be in it as much as one fourth of 1 per cent. of phosphorus.

I have already quoted to you Mr. Barnaby's remarks made at the Naval Architects' meeting in 1875, when he expressed his doubts as to the suitability of steel for the building of ships. In 1876, after he had investigated the matter further, and had seen what could be done, he said, speaking of certain samples of steel, that they were a splendid material, such as shipbuilders might use with a very great deal of confidence. "My remarks," said he, "last year were taken somewhat unkindly, as I thought, by the Bessemer steel makers; and I would ask the gentlemen in the room now, who know most about the matter, whether we have not since last year got a material better suited for ship-building than we had then seen?"

I was going to say steel is getting back its character. I believe I may truly say it *has* got back its character. At all events the Admiralty show their confidence by using it. They demand that when tested, every sample, if 8 inches long, shall elongate at least 20 per cent. before ultimate fracture, and also that every sample, after being heated and quenched in cold water, should be capable of being bent cold (the radius of curvature being not more than three-fourths the thickness of the sample) without any distress whatever being evinced by the metal. I believe I am correct in saying, that every sample out of fourteen thousand which have been tested at Landore has fulfilled these conditions. It has not been a question of averages, but each sample has fulfilled the conditions.

But the fact is, that such material will stand far more severe tests than this. I will ask your attention to these samples, folded and refolded quite flat when cold and after quenching. I have hung on the wall a rough diagram of the testing machine used at the Landore Works. You will see that the sample to be torn asunder is tested in a vertical position, and is held at its lower extremity in a shackle attached to a strong adjusting screw, while the upper part is fast to a similar shackle, which holds down the short end of the main lever. This lever is carried on knife-edges, and has a working end twenty times as long as the short one, so that each hundredweight suspended to the long end gives a ton strain to the sample. At the working end there is an hydraulic ram, which upholds the lever while the weight is being applied; as soon as it is in its place a valve is opened, the water is suffered to run slowly out from below the ram, and thus the weight is brought gradually on the specimen. Fractions of a ton are applied by sliding the small weight along the beam. By such an apparatus as this the experiments can be accurately although rapidly conducted, and the amount of extension, the elastic limit, and the strain required for rupture can all be ascertained.

I wish at this point to make some observations upon testings, which testings, I think, have led the public to believe in higher results than have really been obtained. I have before me on the table the samples 8 inches long I have previously referred to, which samples have been tested for the Admiralty at the Landore Works; and to render more intelligible that which I am about to say, I have hung a diagram upon the wall, of the appearance presented by such samples;

you will see that at the point of fracture the width is diminished to about 80 per cent., and the thickness to about 80 per cent., so that the area of the fracture is rather more than five-eighths of that possessed by the original metal. You will also see that this diminution has not extended throughout the whole 8 inches, but that, on the contrary, it prevails only at the very point of fracture, and that the area greatly increases for about 1 inch each way. The elongation in the whole 8 inches has been $2\frac{1}{8}$, or above 25 per cent., instead of the 20 per cent. contracted for; but if the 2 inches bordering upon the fracture alone were taken, it would be found the percentage of stretch in those 2 inches would be as much as 50 per cent.; and thus it is that by referring the extension at rupture to short samples, very high ratios of extension have been published, ratios which, as I have said, may, if not explained, lead to erroneous impressions.

I have thought it might be interesting to you to see a sample tested; and, thanks to the kindness of the authorities at the Museum of Economic Geology, who have lent me the apparatus, I am able to show this to you. I have added a pointer, to indicate the extension within the elastic limits, and also the extension up to rupture. The pointer exaggerates the movement ten times.

[The image of the specimen was thrown upon the screen, so that the change of shape preceding the moment of rupture was seen.]

I have here another sample of steel which has been torn asunder. This is a material intended for the outside of cannon, where the requirements are, comparatively little extension but great ultimate strength. The elongation here in the 8 inches has only been 14 per cent., while the ultimate tension has been 50 tons per inch.

I need hardly tell this meeting that for all purposes of railway structure, railway axles, tyres, and in other instances where shock or impact have to be resisted by a material, the mere power of withstanding a steady strain is not sufficient. What is required is the ability to demand a considerable amount of mechanical force to be exerted to cause rupture—that is to say, the ability to resist "work done."

Supposing that I had a material which would only bear one ton to the inch, but which would extend 100 per cent. before rupture; then a bridge made of such a material would bear a very light load only without beginning to yield, but no amount of shock would make it break suddenly; one sees that it would continue to stretch and stretch. That would be a useless material. Suppose, on the other hand, I had a material that would bear 100 tons upon the inch, but which would only elongate one thousandth of 1 per cent. before fracture; that would be, again, wholly unfit, because the smallest shock would break it; it would be as brittle as glass. That which is required, therefore, is the union of the two qualities, the great strength and the great elasticity. When the product of these two is a maximum, then the maximum of safety is attained in structures exposed to shock. Steel which will bear 30 tons, and will elongate 20 per cent., would obviously require in a length of 100 a power of $\frac{30}{2}$ by 20 (= 300) to break one square inch. If it would bear 40 tons, and would elongate 15 per cent., it would equally require a power of $\frac{40}{2} \times 15$ (= 300) to effect rupture; or if it would bear 60 tons, and would yield only 10 per cent., it again would require a power of 300. Now, according to the purpose to which the steel has to be applied, it may be desirable to increase the power of resisting the quiescent load or to increase the power of extension; but that which should be aimed at is obviously the faculty of increasing both these in the same sample; and I am sorry that any precaution as regards tests should be taken which deters the manufacturer from improvement in this direction; but such precautions are taken. It is known that it is possible to make a steel which will not bear more than 30 tons per square inch without rupture, but which will certainly extend 20 per cent. in 8 inches before that rupture takes place. There are persons who prescribe tests who are content with this condition of things, and in order to ensure it they absolutely forbid the steel being made to bear more than the 30 tons. To my mind this is to be regretted. I should have thought the right thing to do would be to say, "We will have our 20 per cent. in the 8 inches of extension; we will not have a less ultimate tensile strain than 30 tons; but we shall be very much obliged to you to give us as much greater a tensile strain, so long as it is accompanied by no decrease in extension, or in other qualities, as your knowledge of steel manufacture can enable you with certainty to give."

I told you that the civil engineer had suffered from the Board of Trade having experimented in the year 1860 with puddled steel. In England, therefore, the very birthplace of the great steel industry, in contradistinction to the limited luxurious steel industry, the civil engineer has been unable up to the present time to use steel in railway bridges, because up to the present time steel (cheap as it has become) has always been dearer than wrought iron, weight for weight, although not dearer of late years when considered in relation to its power of supporting strains. But cheap as it has been, the English engineer has not been able to use it, because the Board of Trade will not recognize it as being a more valuable material than wrought iron; and therefore if the engineer were to use steel, he would be compelled to employ just as much weight of steel as he would employ of iron if he used wrought iron.

In Holland the civil engineer is allowed to use steel in his bridges, and it is treated as it deserves to be, as a metal competent to bear a greater strain than wrought iron; but in England the unhappy puddled-steel experiment stands in the way. I am glad to say, however, that by the insistance of Sir John Hawkshaw, in the first case, and then by that of a Committee of the British Association, inaugurated at the Bradford meeting by Mr. William Henry Barlow, President of the Mechanical Section, of which Committee Sir John Hawkshaw is a member, the Board of Trade are, I believe, being brought to a due appreciation of the present state of the steel manufacture; and I do, hope that ere long English engineers may not be debarred from making railway bridges of large spans, which they could well do were they allowed to use steel at its reasonably safe value.

Let me remind you that it is not a mere question of the weight and cost of bridges which is here involved, but it is a question of the maximum span that can practically be attained; and in respect of this question I do not think I can do better than quote from Mr. Barlow's address delivered at Bradford:

"We know from established mechanical laws that the limiting spans of structures vary directly as the strength of the material employed in their construction, when the proportion of depth to span and all other circumstances remain the same. We know also that, taking an ordinary form of open wrought-iron detached girder (as, for example, when the depth is one-fourteenth of the span), the limiting span in iron, with a strain of five tons to the inch upon the metal, is about 600 feet; and it follows that a steel girder of like proportions, capable of bearing eight tons to the inch, would have theoretically a limiting span of 960 feet.

"This theoretical limiting span of 960 feet would, however, be reduced by some practical considerations connected with the minimum thickness of metal employed in certain parts, and it would, in effect, become about 900 feet for a girder of the before-mentioned construction and proportions.

"The knowledge of the limiting span of a structure, as has been explained elsewhere, enables us to estimate very quickly, and with close approximation to the truth, the weight of girders required to carry given loads over given spans; and although the limiting spans vary with every form of structure, we can obtain an idea of the effect of introducing steel by the relative weights of steel and iron required in girders of the kind above mentioned.

"Assuming a load, in addition to the weight of the girder, of one ton to the foot, the relative weights under these conditions would be as follows:

Span.	Weight of Steel Girder.	Weight of Iron Girder.
	tons.	tons.
200	57	100
300	150	300
400	320	800

I must not leave the history of that which has been done to render steel trustworthy without alluding to a particular defect that frequently occurred in the Bessemer ingot, and to the manner in which Sir Joseph Whitworth, that most distinguished amongst English scientific mechanics, has succeeded in curing that defect; I refer to the cavities that were sometimes found in the interior of the Bessemer ingots, cavities which, when they did occur, interfered permanently with the strength of the article provided from such ingots; because, although the action of powerful hammers might close the cavities, it was impossible for their sides, at the temperature at which steel is worked, to be brought into actual metallic union. Sir Joseph Whitworth has devised an apparatus by which the steel in the act of cooling can be subjected to very heavy pressure, and by which therefore the cavities may be prevented from forming, or may be materially diminished in extent. Sir Joseph has obtained some very remarkable results; but I will not pursue this branch of steel improvement further, because it has been brought before the members of this Institution on a previous occasion by Professor Tyndall.*

I think it right, however, to say that many steel manufacturers of large experience hold that with steel made by the pot process or by the Siemens process, sound ingots may be obtained without the application of high pressure; and I would on this subject ask your attention to a slice of the head of an ingot which has been polished and sent here by Messrs. Vickers and Co., of Sheffield.

I think, on inspection after the lecture, you will find it to be absolutely sound; and you must remember that this slice is taken from the upper part, and therefore presumably the least sound part of the large mass, the lower portion of which has been forged into a crank. Messrs. Vickers write to me that they are making at the rate of two such crank-forgings a day, and that they can confidently say every one of the ingots is as free from cavities as is this sample.

The Future of Steel.

I embrace within the title of steel any material which is composed of iron united with a very small proportion of carbon, or of some other alloy, a material which has been in fusion and is malleable.

I believe the application of steel to railways will be universal. For the rails it is now as commonly employed for new rails as is wrought iron itself. For wheel tyres it is used in all cases where excellence is wanted. For the locomotive engine I believe I am right in saying that Mr. Webb, of Crewe, has made every part of a locomotive engine which had been previously made of wrought iron (or of brass in the case of the tubes for the boiler), of steel; steel frame, steel wheels, steel tyres, steel boiler, barrel, fire-box, and tubes; and with respect to the bridges and other works on the line itself, I do hope, as I have said, that the Board of Trade difficulty will be removed, and that we shall see steel girder bridges of every construction. I have mentioned Mr. Webb's name and the extension he has given to the use of steel. He informs me that after years of careful observation he came to the conclusion that steel was so far superior to wrought iron for all the purposes for which the Crewe works supplied that first of our railways, the London and North-Western, that he has entirely abandoned the making of wrought iron at those works, steel having superseded it.

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* 'Proceedings of the Royal Institution,' June 4, 1875, vol. vii. p. 524.

With respect to ships, the Admiralty, as I have told you, are now using steel; its future there is assured. With respect to boilers, it answers for the locomotive with its 140 lb. of steam, and I believe it will succeed for all boilers in regular work; but with respect to the boilers of the Royal Navy, the principal enemy of which is corrosion from want of use, and is not deterioration by ordinary wear, it remains to be seen whether this enemy may not be more prejudicial to mild steel than it is to iron.

Wire ropes for collieries and for steam-ploughing have for years past been made of steel; telegraph wires are made of steel; cannon, when built up in separate hoops, are most advantageously made of steel, because the required variation in elasticity can be given to each hoop. The shot and shells for cannon can advantageously be made of steel. I have here samples on the table from Landore.

The so-called tinplates, the plates from which tin pots and all the other tinman's ware are made, are now made from steel. This material for tinned plates must be such as to admit of severe bending, to enable the joints and seams of such ware to be executed; for this purpose the very best charcoal-iron was used; this is now replaced by steel plates, of which we have samples from Landore before us.

Reverting to that branch of modern steel manufacture, the casting by the Riepe process, we find that screw-propeller blades, church bells, large toothed wheels, and, indeed, every conceivable thing in which great strength is demanded, can be, and is already, to a large extent, made in steel. Looking at these multifarious uses, it appears to me that the future of steel is assured.

To test the extent of this future, let us ask ourselves this question, For what purpose to which wrought iron is now applied may steel not be beneficially substituted? for that is the shortest way of taking it. Well, it may be said that for these purposes, where welding is absolutely necessary, steel welds badly. This is really not the case. It does not weld badly, although I admit that up to the present time it does not weld with the same facility as does wrought iron. But it does weld, and thoroughly. Is it desired to have toughness? We have got it. Twenty per cent. of extension in such samples as these. Is it desired to have enormous tenacity? We have got it. Fifty tons to the inch in such samples as these, over 100 tons in the case of wire. Is it desired to have homogeneous character? and most certainly that is much to be desired. See what has happened to the split-up iron rail. See what has happened to the shelly boiler-plate. Then this material, steel, which has been in fusion, has got that homogeneous character in perfection. Is it desired to have certainty of manufacture? We have now got that. Once more you may say, without fear of contradiction, "As true as steel." You may rely on it for every purpose for which you intend to use it. Is it desired to have cheapness? We have got that. Strength for strength, steel is now as cheap or cheaper than wrought iron. Under these circumstances I cannot see how, for these great and important uses, wrought iron is to hold its own. It demands a more expensive process of manufacture, involving more manual labour, more fuel, and more skill than the manufacture of steel by the Siemens process, and it is far more uncertain in its result. But at present it is possible to use ores for the purpose of making wrought iron, which cannot be employed, so far as chemical knowledge yet guides us, for the making of steel, although even such ores, when passed through a preparatory process, may advantageously be converted into steel. I do not say that the manufacture of wrought iron by the puddling process will absolutely cease, even if chemistry should teach us how to utilise all kinds of ore in the steel manufacture. I think the village blacksmith style of forging (the handicraftsman) will always keep up a petty demand for puddled wrought-iron; for that material can be brought into a very plastic condition, and one much more amenable to treatment by the hand hammer than is the material steel. Moreover, as I have said, wrought iron welds with great facility. But for all the great purposes, in my opinion it will not be many years before the puddled wrought-iron manufacture will be an extinct industry, and, except for the petty purposes I have mentioned, iron will be used in only two forms, cast iron, which will be retained for those purposes where massiveness is required, as in the case of bridges having good foundations, where pressure only has to be exerted, as upon columns, and where complex outlines and ornamentation are required, as in castings for railings and matters of that kind.

I have before me a beautiful specimen of art work, work in cast iron, which is typical of a domain of cast iron proper, that of ornamentation, which, I believe, will never be invaded by steel. Messrs. Barnard, Bishop, and Barnard, of Norwich, the makers of the Sandringham gates, the Norwich gates of the 1862 Exhibition (which gates were, however, all of wrought iron), have at my request sent me samples of foliage executed in malleable cast-iron, wherein is combined the facility of original manufacture due to a fluid metal, coupled with the endurance arising from the toughness of wrought iron, and united also with the power of varying the form of the cast foliage at the will of the artist by bending. For such purposes as these cast iron will remain. Wrought iron, as I have said, except for the village blacksmith's purposes, and except it be to utilise iron ores which at the present time we do not know how to make into steel, is, I believe, doomed. The Future of Steel, therefore, in my view, is practically the occupation of the whole province that was previously filled by steel and by wrought iron; and further, that part of the province of cast iron, such as toothed wheels and castings of that kind, where, to give adequate strength, wrought iron would have been used had not the complexity of the form prohibited its employment, but where now, thanks to Riepe's patent, steel may be melted and made to flow into the desired shapes.

[F. J. B.]

ROYAL·COLLEGE·OF·SURGEONS

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, February 23, 1877.

Sir W. Frederick Pollock, Bart. M.A. Vice-President,
in the Chair.

John Fletcher Moulton, Esq. M.R.I.

Matter and Ether.

All are familiar with the process by which the discovery of the laws that govern phenomena is effected. Close study of various instances of some one phenomenon, with their general likeness and their individual peculiarities, leads, either by the slow process of trial and error or by some happy guess, to the discovery of the law to which the observed variations all conform. When this is once ascertained, the whole group of phenomena fall into harmony, and instead of a mass of separate observations, science seems but the exemplification of a single law. This step in the simplification and unification of nature taken, the human mind is free to seek further conquests. But great as is the advance made by such a step, the mind cannot rest content with it. The dry mathematical expressions which we know as the laws of nature, are soon felt to assuage scientific curiosity to a very imperfect degree. No sooner have we discovered the law that governs any set of phenomena, than we are irresistibly drawn to ask the question, How comes it that this is the governing law? of what set of hidden causes is it the outcome? And thus we are led to seek for the mechanism of nature and to find the physical causes of its laws. Nay, we go further; we cannot be content till we discover the nature and properties of the subject matter of its phenomena, and can deduce from these the previously discovered laws as necessary consequences. And the complete realization of one part, at least, of this search, is not beyond the bounds of possibility. It probably will some day be known to the human race that the whole of nature is but the manifestation of a very few distinct physical existences, possessed of known properties of so simple a kind and so universal in their presence that experience can never enable us to analyze them farther. The possible forms assumed by these elements will be deducible from their properties; and the more complex laws that will regulate the behaviour of the resulting substances, will be problems determinable from their known structure. Thus all nature will be theoretically the resultant of the interaction of known agencies, and the solution will be complete, even though the stupendous difficulties of the analysis necessary to deduce the laws of even the more simple phenomena from the properties of these elements should compel the men of that far distant future to arrive at laws by induction from experiment, just as we do now already in the simplest of all sciences—astronomy. In this science, the phenomena of which are due to only four laws, each of which is capable of statement in most simple terms, the complexity of the analysis well-nigh drives astronomers to abandon calculation for observation, in the more delicate refinements of accuracy in which they indulge. Similar considerations will always operate to keep distinct the sciences that deal with distinct classes of phenomena, long after it has become a recognized truth that they are all the more or less complex manifestations of some few known elemental, physical existences. And certainly no considerations of the limits of possible success can avail to destroy the fascination of researches into the ultimate constitution of the existences of which nature is built up—the minute anatomy of the universe.

But although investigations of this nature are so interesting, and promise not only success, but success of the highest order, and although the interest of the research is common to the devotees of every science, inasmuch as it deals with that which underlies them all, it is most remarkable that so little advance has been made in this direction. And not only is it the case that we are compelled to admit that our ignorance of the ultimate constitution of matter is well-nigh as dense now as it ever was, but if we examine the attempts at solving the problem, hitherto made, they strike us at once as having the strangest characteristics. While in most of the other regions of research the results of all investigators have a certain family likeness, and the theories they propose—though perhaps mutually exclusive—have many points in common, yet here we find that the different solutions proposed have the wildest dissimilarity, and many of them present so fantastic an appearance, that it is difficult to believe that they are the productions of the sober investigators whose names they bear. We are too apt to pass lightly over the lesson taught us by the contemporaneous existence of the corpuscular and the undulatory theories of light. That two theories so diametrically opposite in nature, hypothesizing such utterly different constructions and properties of matter, could at a time not so very long past have been considered as possessing tolerably equal claims to acceptance, opens up a vista of ignorance as to the ultimate constitution of matter which is very humiliating. Nor will it suffice to say that those were the days when true science was in its infancy. Though the dispute between the two theories of light was speedily settled by the complete defeat of the corpuscular theory, and our ignorace of the real mechanism that produces and transmits light was rendered thereby less total—in fact we may say that it was so far dispelled that only those capable of thoroughly understanding the subject, can feel the difficulties and imperfections of the accepted theory—yet similar struggles are still going on in other kindred subjects, and for them no such excuse can be pleaded. Take, for instance, electricity and magnetism. Our knowledge of their phenomena is extraordinarily wide; such a book as Wiedemann's 'Galvanismus' shows an accumulation of observations on one simple branch of the subject that can scarcely be paralleled in any other science. Yet if we look at the rival theories as to the nature of electricity, and its connection with matter, they seem so grotesque and so dissimilar—so like mere guesses in short—that we can scarcely help fancying that we are back in the days of Lucretius, when the best qualification for being a philosopher was to be an imaginative poet. And indeed in fertility of imagination, the authors of such theories as those to which we are referring seem not unworthy rivals of the Roman poet, and they make similar demands on our powers of belief. Weber and Ampère's ideas of magnetism being caused by each molecule of matter having its own special electric current circulating round it everlastingly; Poisson's idea that it is caused by each molecule being permeated by two mutually neutralizing fluids, capable of being separated by external attraction, but incapable of being removed from the molecule; the rival theories of two fluids, of one fluid, and of no fluid, in electricity, the numerous theories as to the nature of the luminiferous ether and its relation to matter, and the vortex theory of atoms, suffice to show that no effective check has as yet been placed on the free use of the imagination in this department of research, and seem to give some colour to the suggestion that the intolerance of credulity, professed by men of science, is easily relaxed by them in favour of their own pet theories.

The chief and immediate cause of this is not far to seek. Any attempt to arrive at the hidden mechanism which causes a phenomenon, must be a direct reflex of the knowledge and the ignorance of the age in which it is made. Let us take the case of some ingenious machine performing some known operations. If a person wholly ignorant of mechanism, save so far as the objects of common life teach it to intelligent observers, were to attempt to solve the problem of its construction, he would seek for some arrangement of levers or other mechanical appliances of the simplest and most elementary nature, which would give the desired result. If a mechanician were to apply himself to the same problem, he would have present to his mind all the refinements of mechanical science, and would probably arrive at a solution, no portion of which would resemble the one composed of the simpler elements, while his might in no respect resemble that which would be arrived at by one who, in addition to possessing a knowledge of mechanism, was also a skilled electrician. Each of the solutions would consist of the elements which the experience and knowledge of the maker enabled him to use, and there would be no probability of any of them representing the actual construction of the machine in question, unless the artifices used in its construction were such as were known to some of the persons who were thus attempting to reconstruct it. Just so is it with the attempts we make to arrive at the mechanism of nature. They are charged to the full with our ignorance. If we happen to be acquainted with structure similar to the actual structure sought for, then success is possible, and some discoverer will in all probability arrive at a close approximation to the real state of things. If we do not, then all our efforts will but lead us to the discovery of a *possible* mechanism—one which might cause the phenomenon, but which is not the one which actually does cause it; and with this we must rest content, till in some way or other our knowledge of possible elements of construction is widened, when we may return again to the problem and find a new solution, which may in its turn have to be replaced by future ones. Take, for example, the question of the structure of matter. Matter was known to move with little or no resistance through the ether, to attract other matter, to be capable of great complexity in its nature, inasmuch as the light proceeding from elements, when in a state of incandescent gas, shows that they are capable of a large number of fundamental vibrations. To explain all this many most ingenious hypotheses were devised, both as to ether and matter; molecules or atoms were viewed as complex arrangements of parts vibrating or revolving under their mutual attractions. Ether was made an imponderable. Space was filled with ultra-mundane corpuscles, which, by their perpetual rain on all masses of solid matter, caused the phenomenon of gravitation. At length Helmholtz discovered vortex-motion. Vortex-rings were found to move with little or no resistance in the medium, whether fluid or gaseous, in which they were formed; they were sensible of the presence of one another though not in contact; they were capable of being made to have the most intricate forms, and to take up the most complicated systems of vibrations. Instantly a vortex-theory of matter was devised, which has very much to recommend it, and which is at present the one in highest favour. But just as it was only rendered possible by Helmholtz's discovery of the new form of motion after which it is named, so it may in its turn have to give way to other theories which enlarged knowledge shall have enabled us to suggest.

The difficulty at once suggests itself, that if we admit that these theories as to the actual mechanism of nature are so intimately dependent on the state of the ignorance or knowledge in which we are when they are framed, how can it be right to attach any credence to them? How can we believe a theory at the same time that we admit that it will be probably displaced by another and a different one which will have at least as high claims to our belief? Science would at once lose all claim to be called the strictest school of belief, if it countenanced any such moral gymnastics as a belief which could thus co-exist with disbelief. And yet so great is the assistance derived from a well-constructed theory as to the mechanism producing phenomena, that she cannot afford to allow all efforts at solving such problems to be delayed until there comes a stage of such perfect knowledge, that the mind might claim to be capable of pronouncing on them with certainty—if indeed such a time could ever come. So she boldly faces the difficulty of which we have spoken—the difficulty of showing any other choice open to scientific men, than either to be credulous or to be timid—by distinctly recognizing a class of scientific fictions, or, as they are usually called, hypotheses. These are theories as to the mechanism of nature which, either completely or to a great extent, account for some set of phenomena, and which therefore, so far as our knowledge goes, may correctly describe the whole or some part of the actual cause of the phenomena, i. e. the mechanism that produces them.

Nothing is more important in scientific thought than to distinguish between these hypotheses and laws. The discoverer of laws has nothing to do with actual causes. He only notices and formulates connections and relations between phenomena, and these formulations are laws. So far as the law is concerned, it is immaterial whether the one of two connected phenomena is the cause or the effect of the other, or whether the relation between them arises from their being both connected with a third phenomenon; the law is equally true in all cases. So long as the induction which has led to it has been duly and carefully performed, it is true and will never be displaced or superseded, however mistaken were the ideas which its framer possessed of the nature or causes of the phenomena to which it relates. Newton was quite right when he said, *hypotheses non fingo*; for the portion of his great achievements to which he was referring consisted in the demonstration of the existence of certain laws, and not in the explanation of their causes. But he, as well as all other great men of science, when the right time came, was ready to frame hypotheses, to start these scientific fictions, which were to be thankfully received, studied, tested, respected, worked from, and in short, everything but actually believed in. And he least of all would have, on the one hand, despised these tentative solutions, or, on the other hand, lightly believed in their truth. The whole of his work shows that he appreciated the full value of hypotheses, a value which it is difficult to express clearly, but which every learner or teacher of science feels only too keenly. For, without some guiding idea as to the nature and causes of phenomena, the mind is very sluggish in devising good methods of investigation; but so soon as a good hypothesis has been formed, it is so suggestive of fields of research and of experiments, that, whether it be true or false, there follows an immediate and rapid increase of knowledge. For a hypothesis may be a good one without being a true one. It may render the greatest assistance to the mind, it may be so well chosen that it accounts for kindred phenomena, not known at the time when it was first suggested, it may lead to the discovery of new laws, and it may enable calculations to be made which are of the highest value, and yet it may turn out to be radically false. Even the theory that heat was an imponderable fluid, might put forth a strong claim to our gratitude for the assistance it gave to early discoveries. To be thus useful for a time, it is not necessary that the assigned mechanism should be the true one, but only that the laws that result from its structure should closely correspond with the more potent of the laws that actually direct the observed phenomena. And just as this is all that a hypothesis need do, so is it all that its success entitles us to believe that it is doing; and the true scientific attitude of mind towards hypotheses is to recognize them as describing causes which would produce results similar to those observed, and which, if not truly representing the mechanism which actually causes the results, at all events would produce results governed by the same laws.

How then are we to take the further step of selecting from amongst those rival hypotheses, each of which professes to give some mechanism able to produce a particular set of phenomena, the one that actually represents the mechanism by which the phenomena are in fact produced? This is the most serious and the most difficult step in discovery. And yet, at first sight, considering how complex are the phenomena that have to be explained, it would seem that any theory that succeeded in accounting for even a portion of them must be very near to the truth; and it is so usual to consider that this is the final and sufficient test of the claims of a hypothesis, viz. that it should suffice to account for the phenomena, that when they are very complex it is often considered a more than sufficient test, and the hypothesis is accepted in spite of its being in many respects undeniably deficient. But, in fact, so soon as we begin to apply ourselves seriously to the problems of the constitution of matter—the hidden mechanism of nature—we are forced to abandon in great measure all such ideas. For we find that we have no true measure of complexity. From one single simple law will follow the most various and complex results, and hence any mechanism so designed as to exemplify in its results the working of that single law would, under such a canon as the one just referred to, be able to claim as evidence of its truth all the complexity which follows from that law. Yet such evidence would equally avail to support the claims of any other mechanism, whose results similarly obeyed that law, and of such mechanisms there might be many. This has been often exemplified in the history of science. The truth of a hypothesis has appeared to be sufficiently demonstrated by the manner in which it fully accounted for complex phenomena, and it has been accepted as a physical truth on such grounds, until some other hypothesis has been shown equally capable of accounting for them; and the success of both has been subsequently traced to their alike leading to results that were obedient to some one fundamental law, to the working of which the whole of the observed complexity was due. In the history of such deep-reaching principles as that of the Conservation of Energy this has been a common occurrence; but other instances are not wanting. After Sir W. R. Hamilton had deduced theoretically from Fresnel's Theory of Light, that in biaxal crystals there must be internal and external conical refraction, and their existence had been thereupon experimentally demonstrated by Dr. Lloyd, one might well have fancied that the accuracy of so remarkable a prognostication was sufficient to establish Fresnel's Theory in all its details. Yet a theory framed by Cauchy, which, though in many respects similar to Fresnel's Theory, is yet in fact wholly irreconcilable with it, was subsequently found to account equally for the phenomena; and it is probable that any theory of undulatory transmission in a non-isotropic medium might be made to do the like. In fact, we can express in abstract language the weakness of the canon which would make the acceptance of a theory follow from its success in explaining complex phenomena, by saying that to render the canon a good one, complexity must be measured, not by the apparent intricacy of the resulting phenomena, or the apparent difficulty of accounting for them, but by the number of independent laws by which the phenomena are governed, and which are successfully accounted for by the proposed mechanism. And as we are seldom in a position to pronounce on the question of the independence of the laws that govern a set of phenomena, i.e. as to whether they are all traceable to a very few fundamental laws or not, we are seldom able to estimate the complexity of those phenomena in the way that would alone justify us in taking it as sufficient warrant for the acceptance of a successful hypothesis.

Nor is this the only one of the well-tried and approved canons of discovery that fails us when we are engaged in researches in the unknown land of the ultimate constitution of that of which the universe is composed. There is no principle which is more constantly present to the mind of the scientific investigator in his search for the causes of phenomena than that of simplicity. Wondrously complex as are the processes that are going on all around us in nature—so apparently complex that the untaught mind has in all ages sought to ascribe them, in a greater or less degree, to consciousness and volition resident in the things themselves, or in beings possessing the power to direct them—science has so often found that these highly complex results come from the very simplest causes, that the investigator expects to find simplicity in his results, and naturally inclines to believe that explanation to be the true one which accounts in the most simple manner for the observed phenomena. This has been advanced by some persons almost to the dignity of a law of thought, and the mind is considered by them to be constrained to believe in the truth of the simplest hypothesis that explains a set of phenomena to the exclusion of all more complex ones.

It is not very easy satisfactorily to account for the undoubted value of this canon, viz. that the simplest hypothesis is probably the true one. That there can be any truth in it in its abstract form (as is generally understood) is not probable. There is no reason to think that nature has any preference for simplicity over complexity, if indeed it is possible to attach any meaning to such phrases. The deeper our knowledge becomes, the greater the complexity that confronts us, and the fainter our hope of finding that the ultimate solution will be a simple one. It is probable that much of the value of the canon arises from the fact that the simpler hypothesis will in general be the one that supposes the concurrent action of the fewest independent causes; and it is, of course, more probable that a smaller number of independent causes should co-operate than that a larger number should do so. But the value of the canon mainly lies in the view which the mind instinctively takes of simplicity. That is to us simple which is the result of means and processes to which we are thoroughly accustomed, and with the results of which we are fully familiar. Operations the most complex in their nature are often felt to be simple, and scarcely to need any explanation, solely because they are so common. It seems, for instance, almost superfluous to invent elaborate mechanism to account for such simple phenomena as evaporation or weight. And thus a hypothesis which is felt to be simple is usually one which traces the phenomena to the action of causes with which we are very familiar, i. e. which are constantly at work around us, and which are therefore just the causes that are the most probable.

From such considerations we at once see how useless must the canon of simplicity be to us when investigating the ultimate constitution of the materials of which the universe is built up. For, in the first place, we have little or nothing to guide us as to the probability of the concurrence of different causes in this unknown region, and secondly, a thing which is of infinitely greater moment, we are absolutely ignorant (save in one or two isolated points) of the types of structure and action that we may expect to find commonly exemplified therein. For in seeking to determine the ultimate constitution of matter and ether, and of all that directly or indirectly acts upon them, we are going beyond all the phenomena with which we have been rendered familiar by observation, whether general or special, and we are occupied in ascertaining the mechanism by which matter and ether are enabled to produce the phenomena we see. Now, of this we have no previous experience, and it cannot be too distinctly kept in mind that where there is no experience there is absolute ignorance. In the world around us we see only the aggregate results of infinitely numerous separate actions, none of which are simply cognizable by our senses or our instruments. Every experience of matter that we have in mechanics is of matter acting in masses. Chemistry and physics give us certain phenomena, caused doubtless by a more intimate action of matter upon matter, but the results are only known to us in gross; and even if we assume that the process is uniform throughout, it is only the result of that process that we see, and its nature is wholly concealed from our view. Similar remarks apply to the other branches of science. Nowhere do we get any direct information as to the nature or details of these processes, or as to the mechanism by which they are rendered possible, and thus we nowhere get any knowledge of the types of mechanism that we may expect to find at work. It is true that in all action of matter upon matter we see that certain laws are universally obeyed. But all that this enables us confidently to enunciate is, that the nature of matter must be such that when matter acts upon matter in appreciable quantities, such and such laws obtain. We are not even justified in asserting that the most universal of these laws must necessarily hold good in the case of the separate actions of which the aggregate is made up.* Still less are we in a position to say that one hypothesis is to be preferred to another, because it hypothesizes only such types of structure or action as we are familiar with in our experience of the world of visible phenomena. It is scarcely too much to say that at present our ignorance of the ultimate constitution of matter is such that no one suggested structure ought to be viewed by us as being in itself more simple or more probable than another.

Unable then to use these canons in their ordinary form, we are driven back to the truths that underlie them. It is true that we cannot rightly judge of the value of success in explaining the complex behaviour of matter as a test of the truth of a hypothesis. And it is also true that we cannot directly judge whether the causes we propose to assign for that behaviour are probable ones. But out of the combined effect of the two there arises a third canon of the highest value, specially adapted to meet the peculiar difficulties of the task. It may, perhaps, be expressed by saying that the probability of the truth of a suggested hypothesis, as to the constitution of matter or the nature and mode of transmission of its actions on other matter, is measured by the dissimilarity of the phenomena explained by it. So long as the hypothesis satisfactorily accounts for but one class of kindred phenomena, no matter how complex they may be and how satisfactorily it may account for them, its truth must still remain in doubt, inasmuch as from our ignorance we are unable to say how much is denoted by that success, in other words, how far such success may be due to these phenomena being necessary consequences of but very few laws, or even of a single one. But if the same hypothesis explains phenomena of a wholly different character to those which suggested it, and which so far as we can judge have no direct connection with them, then we are justified in accepting the hypothesis as a genuine contribution to our knowledge of nature.

This will be rendered clearer by an example. Take as an instance what is probably the most successful attempt that has yet been made to penetrate the darkness that surrounds the constitution of matter. Long ago, in order to explain the phenomena of combining proportions in chemistry, the hypothesis was framed that every element or chemical compound consisted of small atoms or molecules of definite size, constitution, and weight, and that the process of chemical combination consisted in the building up of new compound molecules out of the molecules or atoms of the combining bodies, or the atoms that composed such molecules. The impetus given to the science of chemistry by this felicitous hypothesis can hardly be exaggerated. It permeated the whole of chemical research, and gradually came to be treated as though it represented a demonstrated fact. Against this the more accurate scientific thinkers protested, and were in the right in so doing. They pointed out that the only facts relating to the matter with which we were acquainted were those expressed by the law of combining proportions—that it was true that the atomic theory satisfactorily accounted for this law, but that the mere fact of its accounting for this single law was a very insufficient ground for accepting the absolute truth of the theory. There could be no doubt of the justness of these views, and the strictly hypothetical character of the theory was once more generally recognized. But of late the investigations of physicists into the dynamical theory of gases have shown that when substances are in the gaseous condition (in which alone the separate particles of which they are composed, are capable of free and independent motion, and —so to speak—of manifesting their separate constitution) they are composed of rapidly moving minute particles, of just such size and weight as the atomic theory would lead us to expect. Now this is precisely the type of confirmation which our canon points to as justifying belief. Nothing was farther from the thoughts of the inventor of the atomic theory than the explanation of the relations between temperature and pressure in gases—it was solely to account for a law of chemical combination that he framed his hypothesis—and yet we find that when in the state of gas the substances actually consist of just such particles as the atomic theory requires. No two more dissimilar classes of phenomena could well be imagined; and it is in consequence of this wide dissimilarity of the phenomena which it explains, that, although theories have been started in other branches of molecular physics which have successfully grappled with far more intricate phenomena, there is no theory of the ultimate constitution of matter which has nearly so high a claim to be regarded as an absolute physical truth as the atomic theory.

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* This is no idle refinement. No law would seem to be more absolutely without exception than that heat of itself tends to pass from a hot body to a cold body, i. e. that heat tends towards an equalization of temperature. Yet this has been shown to depend in some instances rather upon the law of averages than on the fundamental laws of energy, and to be inapplicable to the action of single molecules; and it is not impossible that we might be driven to take a similar view of such a law as that of the conservation of energy, though, fortunately, nothing as yet points to this, and it would, therefore, be unscientific to increase the difficulty of investigation by making such a hypothesis until it shall be found that there are good grounds for doing so.

This canon seems to have but little connection with that of simplicity, or, as we may term it, the Law of Parsimony. And the reason of this is, as has been shown, that we are too deeply ignorant of the nature of the ultimate structure of any portion of the universe to be able to tell whether any suggested structure is a probable one, i. e. is one of a type frequently occurring. Slowly as we penetrate the mystery we shall acquire knowledge of particular instances or types of structure, and shall learn what sort of results to expect in our researches; and so soon as this stage is attained we shall rightly give weight to our inclination or repugnance towards any suggested hypothesis. But at present we are scarcely justified in doing so in any degree, and therefore it is of the greatest help to science that different investigators should separately work out theories depending some wholly on actions requiring a continuous medium for their transmission, and others hypothesizing action at a distance. Such as succeed in explaining the phenomena to which they relate must, in the present state of things, be held to be of an equally hypothetical character, and, on the other hand, to be equally good candidates for final acceptance. As we have said, there is good reason to hope that this state of things is but temporary; but at present it exists, and it therefore profits nothing to turn the mind inward upon itself, to ask it to pronounce on the possibility or impossibility of things as to which it knows nothing. Nothing but laborious and prolonged experimental investigations will entitle us to give the preference to one or the other of the rival theories; and this will be due to the experience so gained, and not to any process depending on considerations of what is *à priori* possible or impossible in thought.

It is this utter absence of experience—this total ignorance of what is possible or impossible, probable or improbable—which causes us on the one hand to tolerate so kindly such fantastic hypotheses as those of which we have given instances in Weber's and Ampère's ideas as to molecules, or the collision-theory of gravitation; and on the other hand to view with such doubt and suspicion, and to examine with such a jealous eye, the brilliant theories which have shed so much light on various parts of physics and chemistry. Take, for instance, the hypothesis of a luminiferous ether. In no branch of physics are the phenomena so striking as in physical optics, and no theory has ever fulfilled so difficult a task as has the Undulatory Theory in explaining and accounting for them; and yet if we deliberately consider the claims of this hypothesis to be regarded as a physical truth, we cannot free ourselves from the most serious and perplexing doubt. We must separate in our mind the laws of the Undulatory Theory from the mechanism by which the hypothesis seeks to account for them, and then if we weigh the evidence directly in favour of the existence of such a mechanism apart from such as is derived from its accounting for the laws of the Undulatory Theory, and when we contrast this with the enormous difficulty of reconciling the existence of such a mechanism with other phenomena, we are almost in despair. Remembering that light only makes itself known to us in connection with matter, the necessity of a hypothesis of so serious a character might well be doubted. If it were not for the finite velocity of light, and the great improbability (judging from our knowledge of the law and nature of energy) of there being a store of energy inherent in nothing for the time being, as there must be if the radiant light in the interplanetary spaces is not propagated through a continuous medium, it is very doubtful whether there would be any sufficient justification for the acceptance of the hypothesis of a luminiferous ether as being even a near approximation to a physical fact. At present the existence of a luminiferous ether is generally admitted, though it is very usual to doubt the existence of a medium for the transmission of electric action, and to look upon the latter class of phenomenon as an instance of action at a distance. Yet I doubt whether the evidence in favour of the existence of a luminiferous medium differs at all in kind, or even greatly in degree, from that in favour of a medium for electric action; always supposing that it can be shown satisfactorily that induction across a vacuum occupies a finite time. If then we admit the existence of a luminiferous ether to be satisfactorily demonstrated, we must also admit the existence of an electric ether, and it will probably be found that other types of action have equal claims with these to special media for their transmission. Are we then to crowd space with interpenetrating media, each having as its sole function the transmission of some special kind of action? Without pronouncing dogmatically as to whether this can or cannot be the truth, it is clear that at present we are justified in declining to regard such conceptions as having any much higher rank than that of hypotheses judiciously framed for the purpose of simplifying our analysis, and assisting our thinking powers. If it should turn out that the so-called luminiferous ether accounts for the transmission of electric action, or—as at present seems more likely to be the case—that a medium hypothesized for the purpose of accounting for electric action is capable of satisfying all the needs of the Undulatory Theory of light, then indeed we may begin to think that our hypotheses closely represent physical facts. But such recognition is rightly withheld so long as each new hypothesis suffices only to explain the special type of phenomena for which it was framed.

Many of our best physicists are at work on such subjects as these, and are making good progress. Difficult as is the task, it is still one that occupies itself with what we have reason to believe is the simplest (if such a term can be appropriately used in such a connection), and the most uniform and homogeneous type of ultimate structure. Every glimpse that we get of the nature of matter (such, for instance, as the revelations of the spectroscope or the phenomena of crystallography and chemical change) makes us start back astonished at the well-nigh unimaginable complexity that it reveals. But in the case of light and electricity, although their manifestations must to some degree be bound up with matter, we have the attendant complexities of matter playing but a secondary part, and the main subject matter of the manifestations appears to be the result of some infinitely less complicated mechanism. It is true that we are at present baffled by this very difference from gross matter, which in all probability will ultimately render the problem more simple, inasmuch as our imagination is little rich in suggestions that rise above modified experience. But the fact remains that we are here brought most nearly face to face with the phenomena arising directly out of a comparatively simple type of ultimate constitution, and though the complex behaviour of matter would seem to give us more information as to its structure, and thus more guidance in our remarks, it is, as far as we can yet see, in the domain of light and electricity that we have best reason to expect success in our efforts to arrive at the hidden secrets of the mechanism of the universe.

[J. F. M.]

WEEKLY EVENING MEETING,

Friday, March 2, 1877.

Joseph Hooker, M.D. D.C.L. LL.D. Pres. R.S. Vice-President, in the Chair.

Professor T. H. Huxley, LL.D. Sec. R.S.

The History of Birds.

The speaker commenced by quoting Cuvier's well-known saying, that the geologist is an "antiquaire d'une espèce nouvelle," and illustrating the identity of the methods pursued by the archæological and the palæontological inquirer.

He then commented upon the contrast presented by the phenomena of the mineral world when they are compared with those exhibited by living beings during the lapse of time represented by the fossiliferous stratified rocks. The former present a general uniformity, the latter a constantly increasing divergence from the present state of things. Nevertheless the amount of the divergence of the ancient forms of life from those which now exist is limited, inasmuch as there is no extinct animal which may not at once be classified in one or other of the larger groups of the existing fauna.

In order to illustrate the extent of the modification which can be proved to have taken place within a single group in a given time, the speaker selected the class of birds, which, at the present day, is one of the most sharply defined of all the larger divisions of the animal kingdom; and he pointed out that the numerous remains of birds which have up to this time been discovered in the quaternary and tertiary strata, have not made us acquainted with a bird which departs in any essential respect from living types.

About twenty years ago, however, the Solenhofen slates (which are of middle secondary age) yielded, first, the impression of a feather and then a large part of the skeleton of a bird, the structure of which in some respects differs less from that of a reptile, than that of any other known bird. And recently the cretaceous deposits of the western territories of the United States have yielded to the laborious explorations of Professor Marsh two kinds of birds, the *Ichthyornis* and the *Hesperornis*, which differ from all other birds in the possession of teeth, and thus still further diminish the interval between birds and reptiles.

In conclusion Professor Huxley referred to his discourse on Feb. 7, 1868, on "the Animals which are most nearly intermediate between Birds and Reptiles," when he demonstrated that, "in past times, birds more like reptiles than any now living, and reptiles more like birds than any now living, did really exist."[*]

[T. H. H.]

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[*] 'Proceedings of the Royal Institution,' vol. v. pp. 278–287.

WEEKLY EVENING MEETING,

Friday, March 16, 1877.

William Spottiswoode, Esq. LL.D. Tr. R.I. Secretary and Vice-President, in the Chair.

James Bryce, Esq. D.C.L.

Armenia and Ararat.

The lecturer began by some remarks on the peculiar interest attaching to the Caucasian countries; their remarkable physical structure and the great variety of races, religions, and languages to be found in them. He dwelt upon the part they had played in history, and the importance which their commanding geographical position seemed to reserve for them now, as well as upon the commercial advantages which their situation on two seas secures to them.

Proceeding to speak of Armenia, the lecturer described at some length its orographical structure. It is a country of lofty, irregular plateaux, formed by the meeting of three great mountain systems, the Caucasus, whose southern offsets, forming the watershed between the Kur and the Rion, ramify over the north of Armenia, the Taurus, whose easternmost branches enter it on the west, and the ranges of Iran, which run up into it from the S. and S.E. A more detailed account was given of the Caucasus, which the speaker had himself traversed, and the characteristics of its structure, its comparative narrowness, its steepness, and the great persistent elevation which it maintains, were described. The volcanic phenomena of Armenia were then commented on, and the peculiarities of its soil, climate, and vegetation. It is a high and bare country, with one or two rich valleys, the largest and finest of which is that traversed by the Araxes; and with three remarkable lakes, two of which, those of Van and Urumiah, are close basins, not discharging to the sea. The climate is a climate of extremes, intensely cold in winter from the great general elevation of the country, and the prevalence of N. and E. winds, and in summer very hot and dry. There is therefore a want of wood, and a vegetation on the whole scanty: cultivation is in many places only possible by means of artificial irrigation.

Some account was then given of the inhabitants of Armenia. Excluding the Georgians, who live rather to the north of Armenia proper, and the Russians, who are recent immigrants and comparatively few in number, they belong to four distinct races: the native Armenians, the Kurds, the Persians, and the Tartars or Tatars. The Armenians are to all appearance the aborigines of the country, and were once its rulers, having been for a time the masters of a powerful kingdom, whose sway extended far down the valley of the Euphrates. After the Seljukian conquests a great emigration took place, and the bulk of the race is now scattered over Asia Minor, Georgia, and parts of Eastern Europe. Those who have remained at home are for the most part peaceful and submissive peasants, who do not show either that active intelligence or that admirable aptitude for trade which distinguish the travelled Armenian.

The number who inhabit old Armenia, both Russian and Turkish, is probably under two millions, while the total number of the nation has been estimated at five.

The Kurds, who are apparently the Carduchi of Xenophon and other ancient authors, seem to be also an aboriginal race, and speak a tongue belonging, like the Armenian, to the Iranian family. They are nomads, who wander during the summer over the mountains with their flocks and herds, and in winter descend to the valleys, where they often quarter themselves on the Armenian peasantry, and plunder wherever they go. They are mostly nominal Mohammedans, but in practice care little for any form of religion, and have never shown any disposition to adopt a settled or civilized life. Their presence is, indeed, one of the greatest obstacles to the pacification and improvement of these countries, although they are not without some fine qualities. The Persians inhabit only the towns in the eastern part of Armenia, a region which they held till conquered by the Russians in 1828, and the Tartars, who appear to have immigrated from the steppes north and east of the Caspian in the earlier part of the Middle Ages, form a large part of the pastoral and agricultural population of the valleys. They are a branch of the Turanian race, speaking a language similar to, though rather rougher than, the Osmanli of the Ottoman Turks, and are Mohammedans, generally of the Shiah persuasion. Between these races there is no intermarriage, nor indeed any intermixture whatever: each retains its religion, laws, and customs, unaffected by the presence of the others.

The lecturer then proceeded to give an account of Mount Ararat, dwelling especially on its geological character, and wound up with a description of his ascent to its summit on the 12th of September, 1876; an account which it is hardly necessary to condense here, because it has since appeared at full length in a book published by him under the title of 'Transcaucasia and Ararat.'

[J. B.]

WEEKLY EVENING MEETING,

Friday, March 23, 1877.

Warren De la Rue, Esq. D.C.L. F.R.S. in the Chair.

Professor J. H. Gladstone, Ph.D. F.R.S.

Influence of Chemical Constitution on the Refraction of Light.

The object of the present discourse was to describe the advance that had been made in the subject of refraction equivalents since the lecture delivered on Friday, April 24, 1868. The speaker commenced by explaining the terms employed. The index of refraction of a body expresses the amount to which a ray of light is bent in passing from a vacuum into that body at any other than a right angle. It varies with the temperature, pressure, and every other condition which affects the volume of the substance. The specific refractive energy of a body is its refractive index minus unity, divided by the density, and this is a constant unaffected (or nearly so) by changes of temperature or pressure, passage from the solid to the liquid or gaseous condition, solution in other substances, or chemical combination within certain limits. The refraction equivalent is the specific refractive energy of a body multiplied by its chemical equivalent.

In the spring of 1868 the number of chemical elements of which the refraction equivalent had been determined with more or less accuracy was 17, a number which has since been increased to 52. The additions are principally the metals, and their refraction has been determined mainly from salts by taking advantage of the fact that a salt is not altered in specific refractive energy by solution in water or alcohol. About 200 salts have been examined, and by a comparison of the results the value of each constituent has been obtained.

The following table gives the complete list of the elements, and their optical properties, as far as they are known :—

Element.	Atomic Weight.	Refraction Equivalent.	Specific Refractive Energy.
Aluminium	27·5	8·4	307
Antimony	122	24·5—31·8	201—260
Arsenic	75	15·4	205
Barium	137	15·8	115
Bismuth	210	39·3	187
Boron	11	4	364
Bromine	80	15·3—16·9	191—211
Cadmium	112	13·6	121
Cæsium	133	13·7 ?	103 ?
Calcium	40	10·4	260

Element.	Atomic Weight.	Refraction Equivalent.	Specific Refractive Energy.
Carbon	12	5	416
Cerium	92	13·6 ?	148 ?
Chlorine	35·5	9·9—10·7	279—301
Chromium	52·2	15·9—23	305—438
Cobalt	58·8	10·8	184
Copper	63·5	11·6	183
Didymium	96	16	166
Fluorine	19	1·4 ?	73 ?
Gallium	..	..	..
Glucinum	9·3	5·6	606
Gold	196·7	24	122
Hydrogen	1	1·3—3·5	1·300—3·500
Indium	74	..	..
Iodine	127	24·5—27·2	193—214
Iridium	198	31·7 ?	160
Iron	56	12·0—20·1	214—359
Lanthanum	92	15·8	168
Lead	207	24·8	120
Lithium	7	3·8	540
Magnesium	24	7	292
Manganese	55	12·2—26·2	222—476
Mercury	200	21·3—29·0	107—145
Molybdenum	92	..	..
Nickel	58·8	10·4	177
Niobium	97·6	..	..
Nitrogen	14	4·1—5·3	293—378
Osmium	199	..	..
Oxygen	16	2·9	181
Palladium	106·5	22·2	208
Phosphorus	31	18·3	590
Platinum	197·4	26	132
Potassium	39·1	8·1	207
Rhodium	104	24·2 ?	232 ?
Rubidium	85·5	14	164
Ruthenium	104	..	..
Selenium	79	30·5	385
Silicon	28	7·4—6·8	263—238
Silver	108	13·5	125
Sodium	23	4·8	209
Strontium	87·5	13·6	155
Sulphur	32	16	500
Tantalum	137·5	..	..
Tellurium	128	..	..
Thallium	204	21·6	106
Thorium	231·5	..	..
Tin	118	27·0—19·2	203—162
Titanium	50	25·6	512
Tungsten	184	..	..
Uranium	120	10·8	90
Vanadium	51·2	25·3 ?	494 ?
Yttrium	68	..	..
Zinc	65	10·2	156
Zirconium	90	22·3	249

The sign ? indicates that the values have been deduced from only one compound, or that the different determinations are not fairly accordant.

A glance at the above table reveals several remarkable features. It is evident, for instance, that such non-metallic elements as phosphorus, sulphur, selenium, carbon and boron have remarkably high specific refraction, while hydrogen is more than double any other in value. A more remarkable relation is one that appears when the specific refractive energy of the metals is compared with their combining proportion, that is to say, with the absolute amount which unites with one univalent atom—say 35·5 parts of chlorine—to from a stable compound. This is exhibited in the following table, in which the metals are ranged according to their combining proportions, hydrogen being included on chemical grounds.

Element.	Specific Refractive Energy.	Combining Proportion.
Hydrogen	1·300	1
Glucinum	606	4·7
Lithium	540	7
Aluminium	307	9·1
Chromium	305	17·4
Magnesium	292	12
Calcium	260	20
Zirconium..	249	22·4
Rhodium	232 ?	34·8
Manganese	222	27·5
Iron	214	28
Sodium	209	23
Palladium	208	26·6
Potassium..	207	39·1
Arsenic	205	25
Tin	203	29·5
Antimony..	201	24
Bismuth	187	69
Cobalt	184	29·4
Copper	183	31·7
Nickel	177	29·4
Didymium	166	48
Rubidium..	164	42·7
Iridium	160 ?	49·5
Zinc	156	32·6
Strontium..	155	43·8
Cerium	148	46
Platinum	132	49·3
Silver	125	108
Gold	122	65·7
Cadmium	121	56
Lead	120	103·5
Barium	115	68·5
Mercury	107	100
Thallium	106	204
Cæsium	103 ?	133
Uranium	90	120

It is evident that as the figures in the first column decrease, those in the second, as a rule, increase; that is to say, that the specific refractive energy of a metal is inversely as its combining proportion, or, in other words, that those metals which have the greatest power in retarding the passage of a ray of light, are those which have the greatest power of saturating another element.

This law does not hold good absolutely: thus potassium and sodium, which have very different combining proportions—39·1 and 23 respectively—have practically the same refraction.

The extent of these discrepancies would be better seen if the above table were represented graphically. It would then appear as if there were a tendency to form not one curve, so much as three or four nearly concurrent curves, according to the particular character of the elements: thus the univalent metals, lithium, potassium, rubidium, silver, cæsium and thallium, would seem to form a curve somewhat within the others.

These two properties—the saturating power and the refractive energy—are not directly proportional. It would rather appear that the specific refractive energy is inversely as the square root of the combining proportion. Thus for the univalent metals:—

Metal.	Specific Refractive Energy.	Square Root of Combining Proportion.	Product of these two Numbers.
Hydrogen	1·300	1	1·3
Lithium	540	2·6	1·4
Sodium	209	4·8	1·0
Potassium	207	6·2	1·28
Rubidium	164	9·2	1·5
Silver	125	10·4	1·3
Cæsium	103 ?	11·5	1·18 ?
Thallium	106	14·3	1·51

The law evidently holds true, roughly, for this group, with the exception of sodium, which as noticed above is discrepant. This is the more remarkable, as the group includes the elements of the highest and the lowest saturating power.

Another point in regard to which an advance has been made, is the following. If an element always had the same refractive energy in whatever way it was combined, we should be able to calculate beforehand the refraction equivalent of any compound of which the composition was known, and its refractive index also if we knew its density. This would be interesting, but we should evidently gain no information as to the chemical constitution or structure of the compound. But the fact is otherwise. As a rule, the refraction does not vary: carbon, for instance, whether alone as in the diamond, or combined in such diversified bodies as bisulphide of carbon, coal gas, cyanogen, alcohol, paraffin, sugar, and a hundred others, is exerting the same influence on the rays of light which traverse it; but there are certain compounds of carbon, in which it exerts a different influence, and it is from the consideration of this property—a wholly independent means—that we are able to confirm, or otherwise, our theoretical views of their constitution. This may be illustrated by the simpler case of iron. It is well known that one atom of iron will combine with either two or three univalent atoms, forming two classes of salts, which are totally distinct. Now in the ferrous salts, such as $FeCl_2$, that have been examined, the refraction equivalent is 12·0, while in the ferric salts, as $FeCl_3$, it is 20·1.

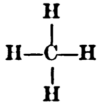
To return to carbon. It is well known that one atom of this element usually combines with four univalent atoms or radicles such as $H . Cl . HO . NO_2 . CH_3$, or two divalent atoms such as $O . S$. In the immense multitude of carbon compounds of this order, the carbon is invariably found to have a refraction equivalent of 5·0. This is independent of the manner of combination, so that isomers such as chloride of ethylene and chloride of ethylidene have precisely the same value.

There is, however, a large group of bodies, in which the carbon is not saturated in the same way, but we are obliged to imagine that six atoms are associated together in each molecule, and that they thus combine with six univalent atoms. This large group forms what chemists call the aromatic group, of which benzene, aniline, oil of bitter almonds, benzoic acid, carbolic acid, are well-known members.

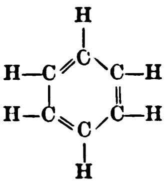
To the mind of the chemist, there is a very wide distinction in the fundamental constitution of these two classes of carbon compounds. The refraction goniometer exhibits this wide distinction in another way, for every member of this aromatic group which has been examined gives a value 6 or 7 above that which it would be if calculated on the supposition that carbon equals 5.

The naphthalene group has evidently a very different ultimate molecular structure, in which ten atoms of carbon are supposed to be associated. The refraction is 14 or 15 higher than the amount calculated from the value already given.

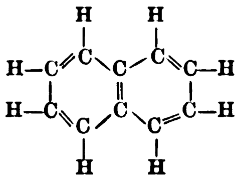
These three groups are typically expressed:—



Marsh Gas.



Benzene.



Naphthalene.

Anthracene is probably more complicated, and its refraction exceeds the theoretical amount by a still larger quantity. In fact, refraction and dispersion increase very rapidly with the number of atoms of carbon that are not combined with at least two of hydrogen or their equivalent.

It is evident that this may afford a means of determining to what group a doubtful body may belong. Thus a colourless oily body, with a pleasant odour, named furfurol, has been obtained from bran. The proportions of the elements are $C_5H_4O_2$, but it was quite a matter of opinion whether the five atoms of carbon were combined as above with H and O counting for eight, and therefore that furfurol was analogous in constitution to the essential oils ($C_{10}H_{16}$ or $C_{15}H_{24}$), or whether it contained two of hydroxyl, HO, in which case it would be more analogous to the aromatic group, since there would be five carbon atoms to four univalent radicles, like benzene C_6H_6 minus CH_2. Now, the refraction equivalent of furfurol is 42·3, which differs from the calculated value by 6·3, about the same difference as we find in the aromatic group, and far exceeding that observed in the case of the essential oils.*

Besides determining in this way the class of bodies to which a substance belongs, it is also evident that the prism may throw light on the constitution of groups of complex isomers, and we may expect that its indications will in many ways have an important influence on the theoretical chemistry of the future.

[J. H. G.]

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* Since this discourse was delivered I have learned that recent researches have shown chemical reasons for associating furfurol with the benzene group.

GENERAL MONTHLY MEETING,

Monday, April 2, 1877.

GEORGE BUSK, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Mrs. S. Bircham,
Sir Robert Burnett, Bart.
James Edmunds, M.D.
George Barnet Goolden, Esq.

were *elected* Members of the Royal Institution.

The following arrangements of the Lectures after Easter were announced:—

PROFESSOR J. H. GLADSTONE, Ph.D. F.R.S.—Five Lectures on the Chemistry of the Heavenly Bodies; on Tuesdays, April 10 to May 15.

(No Lecture on May 1st, the day of the ANNUAL MEETING, 2 P.M.)

PROFESSSOR J. DEWAR, M.A. F.R.S.E.—Three Lectures on the Chemical Philosophy of Sir Humphry Davy; on Tuesdays, May 22, 29, and June 5.

PROFESSOR TYNDALL, D.C.L. LL.D. F.R.S.—Eight Lectures on Heat; on Thursdays, April 12 to May 31st.

EDWARD DANNREUTHER, Esq.—Two Lectures on Chopin and Liszt: with many Illustrations on the Pianoforte; on Saturday, April 14, and Thursday, June 7.

The REV. A. H. SAYCE, M.A.—Three Lectures on Babylonian Literature; on Saturdays, April 21, 28, and May 5.

WALTER H. POLLOCK, Esq. M.A.—Three Lectures on Modern French Poetry; on Saturdays, May 12, 19, 26.

CHARLES T. NEWTON, Esq. C.B.—Two Lectures on the Recent Discoveries at Mycenæ; on Saturdays, June 2 and 9.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same.

FROM

The French Government (by the Minister of Public Instruction)—Inventaire des MSS. Français de la Bibliothèque Nationale, Tome I. 8vo. Paris. 1876.
Rapports sur le Service des Archives de la Bibliothèque Nationale, 1876. 8vo. 1877.

Government of Bengal—General Report of Public Instruction in Bengal, 1875–6. fol. 1876.

The Lords of the Admiralty—Nautical Almanack for 1880. 8vo. 1876.
Greenwich Observations for 1874. 4to. 1876.
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WEEKLY EVENING MEETING,

Friday, April 13, 1877.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

William Spottiswoode, Esq. M.A. LL.D. Treas. R.S. Sec. R.I.

Experiments with a Great Induction Coil.

The experiments shown in this discourse were in illustration of the power of an induction coil constructed for me by Mr. Apps, and described in full in the 'Philosophical Magazine' for January of this year. The following are, however, the particulars of its principal parts. The coil, as arranged on this occasion, has a core consisting of a bundle of iron wires, each ·032 inch thick, and forming together a solid cylinder 44 inches in length, and 3·56 inches in diameter. Its weight is 67 lbs. The copper wire used in this primary is 660 yards in length, ·096 inch in diameter, has a conductivity of 93 per cent., and offers a total resistance of 2·3 ohms. It contains 1344 turns wound in six layers, has a total length of 42 inches, with an internal diameter of 3·75 inches, and an external of 4·75 inches. The total weight of this wire is 55 lbs.

The secondary consists of 280 miles of wire, forming a cylinder of 37·5 inches in length, 20 inches in external, and 9·5 inches in internal diameter. Its conductivity is 94 per cent., and its total resistance is equal to 110,200 ohms.

The condenser required for this coil proves to be much smaller than might have been at first expected. After a variety of experiments, it appeared that the most suitable size is that usually employed with a 10-inch spark coil, viz. 126 sheets of tinfoil 18 inches by 8·25, separated by two thicknesses of varnished paper, the two thicknesses measuring ·011 inch.

When so arranged, this coil gave, with five quart cells of Grove, a spark of 28 inches; with ten similar cells, one of 33 inches; with thirty such cells, one of 37·5 inches, and subsequently one of 42 inches.

When the discharging points were placed about an inch apart, a flowing discharge was obtained, both at making and at breaking the primary circuit. The sound which accompanies this discharge implies that it is intermittent.

With a 28-inch spark, produced by five quart cells, a block of flint glass 3 inches in thickness was pierced.

When a Leyden battery, having a coated surface of 10 feet, was inserted as a secondary condenser, the spark was sufficiently brilliant to allow its spectrum to be projected on a screen. In this spectrum the principal metallic lines were distinctly visible to the audience.

A Leyden battery, having a coated surface of 20 feet, was charged with 4 sparks from the coil.

In further exemplification of the power of this induction coil, some experiments were devised so as to exhibit to the audience effects previously observed only in a revolving mirror.

When, as is generally the case with an induction spark, the discharge occupies an appreciable interval of time, the image of it seen in a revolving mirror appears spread out to a breadth proportional to its duration, and to the velocity with which the mirror revolves. The successive phases of the phenomena are then arranged in successive positions, and may be studied separately, even when too rapid to be disentangled by the unassisted eye. This method, however, is adapted to only one or, at most, two observers at a time.

In experiments with luminous objects where there is sufficient brilliancy of light, the image as seen in a revolving mirror may be projected upon a screen by replacing the plane by a concave mirror of suitable focal length; but the comparative feebleness of light in vacuum tubes, even when illuminated by the great coil, rendered even this method inapplicable for the purposes of an audience. With a view to overcoming the difficulty, I next tried the effect of causing the tube itself to move during the discharge; and after a variety of experiments, I finally attached the tubes to the wooden arms about 6 feet in length, which were made to revolve about their centre like the arms of a windmill, and succeeded in exhibiting the phenomena on a large scale. The instrumental arrangements were in substance similar to those of a Gassiot's star; but the discharges, instead of being almost instantaneous, were of long duration; and instead of appearing as mere radii, they covered sectors of appreciable angle, sometimes semicircles, or even more, about the centre.

This being so, every stratification which remained fixed during the entire discharge described the arc of a circle, and a column of striæ appeared as a series of concentric luminous rings or portions of rings, according to the duration of the discharge. Similarly, striæ which were moving in one direction or another along the tube, described arcs of spirals, the pole of which was situated towards the beginning or end of the discharge, according as the motion was from the centre towards the extremities of the arms, or from the extremities towards the centre.

Any alteration in the velocity of the striæ along the tube, or proper motion as I have termed it, was shown by a corresponding alteration in the curvature of the spiral; and any intermittence in the discharge by a dark sector in the field. In this manner the whole behaviour of the striæ during their brief period of existence was delineated on the screen.

The following are some of the general conclusions derived from the experiments repeated in this discourse.

I. The thin flake-like striæ, when sharp and well defined, are either short lived or have little proper motion, or both.

II. The apparent irregularity in the distribution of the striæ as seen by the eye, is due to their unequal duration, and to the various periods at which they are renewed.

III. The proper motion of the striæ is generally directed towards the positive terminal; and its velocity varies only within very narrow limits.

IV. Flocculent striæ, such as are usually seen in carbonic acid tubes, are a compound phenomenon; and are due to a succession of short-lived striæ which are regularly renewed. The positions at which the renewals take place determine the apparent proper motion of the flocculent striæ.

V. The velocity of proper motion varies, other circumstances being the same, with the diameter of the tube. This was notably exemplified in a conical tube.

VI. When the proper motion exceeds a certain limit, the striæ appear to the eye to be blended into an unbroken column of light, and all trace of stratification is lost. The mirror will in many cases resolve this column into its component striæ, but not in all.

The discharge from a Leyden jar, when sufficiently charged, is, as is well known, continuous through its entire length; but it still exhibits a dissymmetry in extending from the point of the positive to the hilt of the negative terminal. It is, moreover, so far as our present instrumental measurements extend, instantaneous, and cannot be spread out by a revolving mirror. Lastly, it exhibits no trace of stratification. This difference of effect appears to depend on that of tension. This was established by some experiments described in the 'Proceedings of the Royal Society,' 1877, vol. xxv. p. 73, and tend to show that this character of the jar discharge is due to tension.

A curious illustration of this was shown by inserting a Leyden jar in a loop circuit with the tube. By this means the current was divided; one part went to charge the jar, while the other part went directly through the tube. As long as the tension in the jar remained below a certain limit, the coil alone acted in the tube; but as soon as the charge in the jar was sufficient, it discharged itself through the tube. As seen in the mirror, the field of view showed first a stratified discharge, and then, following immediately upon it, a jar discharge or unbroken line of light.

It is, however, impossible to convey any adequate idea of these phenomena by mere description; and the reader who desires fuller illustration of the subject is referred to the figures which accompany my paper in the 'Proceedings of the Royal Society,' 1877, vol. xxv. p. 73.

At the close of the discourse I expressed a hope that it might prove possible with this great coil to photograph the image of the phenomena here described; and I am glad to add that subsequent experience tends to confirm this hope.

[W. S.]

WEEKLY EVENING MEETING,

Friday, April 20, 1877.

WILLIAM SPOTTISWOODE, Esq. LL.D. Treasurer R.S.
Secretary R.I. and Vice-President, in the Chair.

FREDERICK POLLOCK, Esq. M.A.

Spinoza.

THE commemoration of the two hundredth anniversary of Spinoza's death which has this year taken place at the Hague has produced many signs of the interest taken in him by students of philosophy, and has also stirred up new interest, one may hope, among some who are not philosophers. I shall now endeavour, without going into a critical exposition for which the time would be insufficient, to tell you in outline what manner of man Spinoza was, and of what sort was the work done by him in philosophy which has had so great an influence in the world.

It may seem absurd to talk of the influence of philosophy in the world at all. It is a common notion that philosophy is a sort of intellectual playground where people who have a taste for it exercise their wit by attempting to frame answers to insoluble questions, and that such pastime can have no bearing on the practical conduct of life. I might answer by the usual generalities about the power of thought in human affairs. I might say that the ideas made in the workshops of the philosophers are the real moving forces which set up or cast down governments, and rule the issues of peace or war. I might say that the only complete conquests are those made by ideas; the sword may smite and destroy, princes and rulers may grind to powder, but thought only can dissolve. But I choose to produce more palpable examples.

I first ask you to look back to a short half century in a critical time of the development of the Roman Empire. It was the time when the laws and institutions which have left an abiding stamp on the greater part of the civilized world were being expanded from a municipal to a cosmopolitan scale. It has been described by the greatest of English historians as "possibly the only period of history in which the happiness of a great people was the sole object of government."

During that period considerable parts of the world enjoyed beyond question a vastly better government than they ever knew before or have ever known since; and not only was happiness procured for the generations then living by the wisdom and virtue of their rulers, but a step was thereby made good in the civilization of mankind. The rulers of the Empire at that time were T. Antoninus Pius, and his adopted son M. Aurelius Antoninus, surnamed the Philosopher.

My other example is almost from our own times. In the darkest hour of Germany, when her life and freedom seemed hopelessly crushed under the first Bonaparte, there lived and taught in Prussia a man named Fichte. He was a very philosopher of philosophers, a constructor of magnificent theories, a dreamer of splendid dreams surpassed only by Plato's, and he spoke of duty and patriotism with a voice that no decree of Napoleon's could stifle. Meanwhile Scharnhorst, a man with a different genius of his own—the inventor of the modern Prussian army—was forging the weapon of deliverance. When the hour struck, Fichte was among the first to proclaim it; he sent forth his students to fight for Germany, and himself died, though not in battle, yet as truly for his country as if he had fallen on the field. More than this, Scharnhorst's instrument is one that must have intelligent patriotism to work it. Fichte was among the foremost who saw the need of a national system of education for Germany, and this dreamer of dreams was one of the chief actors in founding the University of Berlin. Years passed on, and there came, as there will in all great works, backsliding and disappointment; and men said again that the Germans were a poor dreamy folk, fit for nothing but music and philosophy. But in due time there rose up another man whose name was Otto von Bismarck; he is no philosopher that I know of,* but his gift was to see that the crop Fichte and Scharnhorst had sown was full ripe; and he set to his hand and reaped the harvest whose fruit we may all see at this day. These particular examples are not taken at random. M. Aurelius Antoninus was the disciple and zealous follower of the Stoic doctrine; and the Stoic view of the world was strikingly like Spinoza's on its practical side. As to Fichte, his philosophy, though very different from Spinoza's as a whole, is full of ideas taken from him or suggested by him.

Before entering on Spinoza's doctrine it may be well to give a brief summary of his life. He was born at Amsterdam on the 24th of November, 1632; his parents were members of the Portuguese synagogue, a community founded by Jewish exiles from Spain and Portugal. A high standard of knowledge and culture prevailed in this Jewish society, and at an early age Spinoza was learned in all the wisdom of the Rabbis. He made himself thoroughly familiar with Latin under the teaching of Dr. van den Ende. The story told by his first biographer, and since often repeated, of his love for Van den Ende's daughter, and his rejection in favour of a richer suitor, is now known not to be true in that form, and is probably mere fiction. In 1656 he was excommunicated; a proceeding of doubtful policy, and perhaps even of doubtful validity, for according to Jewish law such a sentence cannot properly be founded on heretical opinions alone. Spinoza, however, did not dispute it. He had already left Amsterdam, where an attempt on his life by some nameless fanatic had warned him that he was not safe. His successive dwelling-places were Rhijnsburg, near Leyden (1661), Voorburg, near the Hague (1664), and lastly, the Hague itself (about 1670). He died on the 21st of February, 1677. He was well known to men of letters and science, and had many friends among them; but his life was extremely retired. He never taught in public, and his chief philosophical works were not published in his lifetime at all.

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* Since writing this I have seen it stated that Prince Bismarck was at one time a student of Spinoza.

Sources of Spinoza's Philosophy.

I have tried to show the chief lines of deviation in the annexed table. First in the order of time came the Jewish and Arabian followers of Aristotle, whose importance in this and other respects was underrated till quite lately. These men were the schoolmen of mediæval Jewry and Islam. Like the Catholic schoolmen, who drew largely from them, they set themselves to recast the doctrines of current theology in an Aristotelian mould. Chief among the Jewish school were Ibn-Ezra and the great Maimonides (Moses ben Maimon). Their predecessor Avicebron (Ibn-Gebirol) had a curious fate. He was an independent thinker who constructed a system of his own, chiefly with Neo-Platonic materials. His speculations were ill received by his own people, and fell into such oblivion that his very name in its native shape was only lately recovered by the research of Dr. Munk. But a Latin version of his work (*Fons Vitæ*) became current in Europe, and found a kindred spirit in Giordano Bruno, who in turn passed on Avicebron's ideas, along with his own, to Spinoza. In later times at least one Jewish philosopher was found to break with the Peripatetic tradition. This was Chasdai Creskas, of Barcelona, who flourished about the end of the fourteenth century. His ideas were original and daring, and some of the most peculiar features in Spinoza's philosophy are now accounted for by his influence.*

The Catholic schoolmen were not directly known to Spinoza; but their habits of thought and language still pervaded philosophical writing, and Descartes, though he rebelled against their methods, could not wholly shake off their influence.† Through Descartes, as well as through the Jewish philosophers whom the schoolmen themselves had followed, Spinoza got much of the scholastic forms.

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* Joel, 'Beiträge zur Geschichte der Philosophie.' Breslau, 1876. J. P. N. Land, 'Ter Gedachtenis van Spinoza.' Leiden, 1877. (At pp. 14, 51.)

† Land, *op. cit.* p. 15.

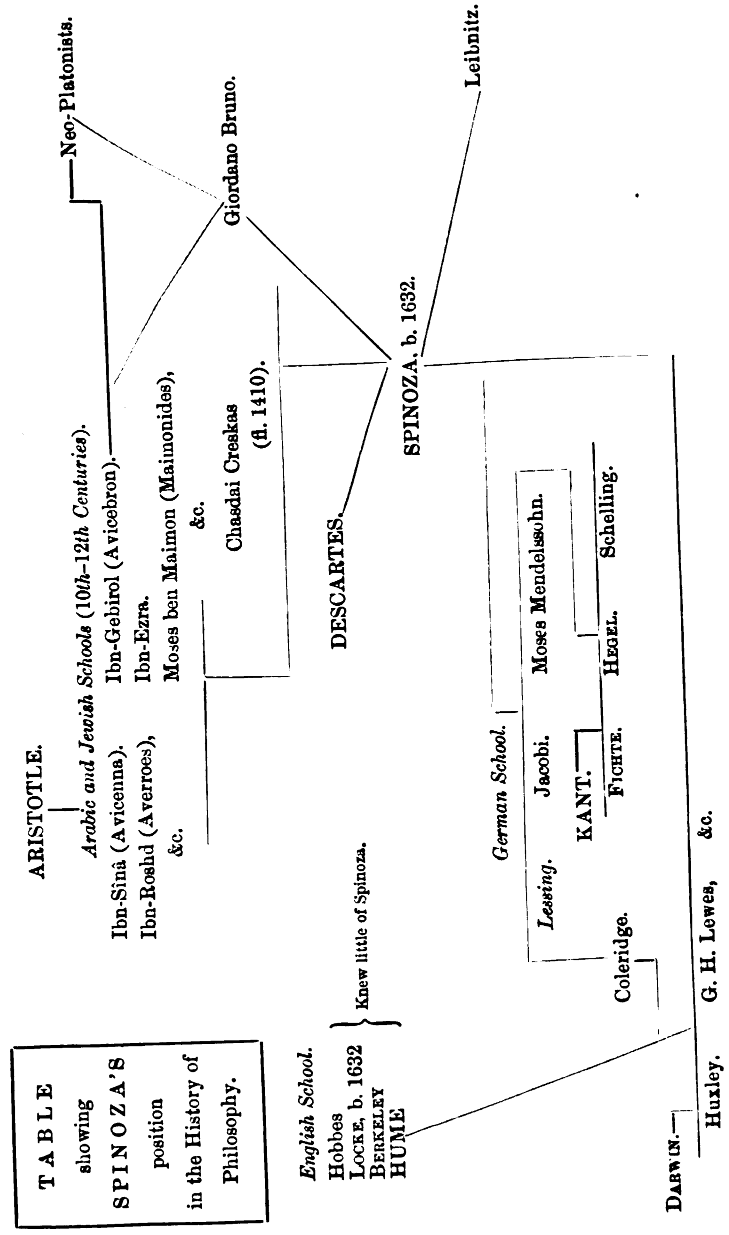

TABLE showing SPINOZA'S position in the History of Philosophy.

ARISTOTLE.
Arabic and Jewish Schools (10th–12th Centuries).
Ibn-Sînâ (Avicenna).
Ibn-Roshd (Averroes),
&c.
Ibn-Gebirol (Avicebron).
Ibn-Ezra.
Moses ben Maimon (Maimonides),
&c.
Chasdai Creskas
(fl. 1410).
Neo-Platonists.
Giordano Bruno.
DESCARTES.
SPINOZA, b. 1632.
Leibnitz.
English School.
Hobbes
Locke, b. 1632
Berkeley
HUME
Knew little of Spinoza.
German School.
Lessing.
Jacobi.
Moses Mendelssohn.
KANT.
Fichte.
Hegel.
Schelling.
Coleridge.
Darwin.
Huxley.
G. H. Lewes,
&c.

Descartes' own part in the formation of Spinoza's philosophy, after being for a long time overrated beyond measure, is perhaps in danger of being underrated now. Leibnitz said of Spinoza that he only cultivated certain seeds sown by Descartes. No shallower saying was ever uttered by a man of genius; but it is doubtful whether the saying was even honest. Leibnitz's systematic depreciation of Spinoza can under all the circumstances hardly be accounted for by honest misapprehension. The differences between the philosophy of Spinoza and that of Descartes are of a radical kind. Spinoza used Cartesian terms as he used scholastic terms; but he was no more a Cartesian than he was a schoolman. There is no doubt, however, that the study of Descartes gave a most important stimulus to Spinoza's philosophic research, and had much to do with settling its form and direction. Spinoza himself did not seek to extenuate his obligation. When he expressly marks (as he sometimes does) his dissent from Descartes, it is in a decided but respectful tone such as he does not use towards other opponents, and something like Aristotle's manner in speaking of Plato. After all, it is a task of not very profitable conjecture to measure against one another all the several elements that went to make up the materials of a great man's work. It is enough to know that Spinoza wrought, as all men must, upon the matter given him by the conditions of his environment; and in that environment Descartes certainly filled a prominent place.

One point, however, deserves to be distinctly noted; no man ever grasped more clearly than Spinoza the idea that a philosophy which professes to account for things in general must include and harmonize with the account of sensible things given by the natural sciences; it may transcend it, but it must not contradict it. It is this idea that has enabled philosophy to enter on a new life; and it is just this that Spinoza could learn from Descartes, and, so far as I know, from Descartes alone. His application of it was, indeed, a very different one.

The Principles of Spinoza's Philosophy.

It is for many reasons extremely difficult to explain Spinoza's philosophy in any language but his own. His work does not lend itself to any of the current phrases with which we are accustomed to label systems of philosophy. For example, it is easy to call it *Pantheism;* but what does that tell us? Almost nothing. The Hindu philosophers are pantheists likewise, and so were the Stoics, but the difference of their systems from Spinoza's and from each other is immense.

Brahmanism, together with its overgrown offshoot Buddhism, is hopelessly remote from our Western ways of thinking in the very groundwork it builds upon. It assumes as an elementary truth that life is altogether a bad thing; not only that man's life is unhappy, but that all finite existence is in itself an evil and a mistake, and that consequently the problem set before man in the world is to escape from it as speedily and entirely as he can. This *dogmatic pessimism* would be almost inconceivable to us here and now, had not certain philosophers in Europe taken up with it on their own account quite recently.

The Stoics held the very opposite belief. For them the universe was the product of perfect Reason, and as such was perfectly good: and man's wisdom and happiness were to be found in the conviction that nothing could happen, however hurtful it might seem in particular relations, but what was in some way good for the world. In the place of the Oriental pessimism we find a no less *dogmatic optimism.*

Spinoza does not admit either of these opinions. He does not call the universe either good or bad, for *good* and *bad* are to him unmeaning terms except in relation to the welfare of some individual or kind. But he holds no less firmly than the Stoics, that the order of the universe, including the order of human nature, determines certain conditions for man's welfare. It is in man's power to observe these conditions, and thus to make the world good for himself. Moreover, Spinoza holds, in distinct opposition to Brahmans and Buddhists, that life is in itself a good thing, or rather that by *good* we really mean that which helps us to fulness of life. Thus his view of the world might be called a *rational optimism.*

It will be seen that a common term which includes such widely different doctrines is really little better than illusory.

The really important truth about Spinoza's way of thinking which is hinted at by the vague term Pantheism is this: his first fundamental idea is the unity and uniformity of the world. This had been conceived by many of Spinoza's forerunners from the purely speculative side, and notably by Chasdai Creskas. To Descartes, and therefore also to Spinoza, the same idea presented itself from the scientific side on grounds of physical evidence. Spinoza seized upon the idea in both aspects at once, carried it out into its fullest development, and made it the groundwork of his whole philosophy. This distinguishes him from Descartes, who declined to pursue it thoroughly.

The manner in which Spinoza works it out shows a struggle to express scientific ideas in scholastic forms. I will now give a sort of free translation of Spinoza's thought: I do not say that it is such as he would have given himself, or that it was present to his mind in anything like that form. But we are not thereby forbidden to make it for our own use, and I give it thus: "You have here scholastic terms such as *essence*, *existence*, *attributes*, which have been bandied about for centuries without acquiring an intelligible meaning. You have here, again, the scientific idea of the unity and uniformity of the world, irresistibly suggested by all our advances in knowledge, and spreading and prevailing day by day. Sooner or later no corner of the universe, not even the inmost recesses of human thought, will escape from its dominion. Therefore, if you would save the old terms from perishing utterly, you must breathe new life into them by means of the scientific idea." So far my own modernized version. What follows is intended to be tolerably close to Spinoza's own way of putting his doctrine. There is no absolute existence but in the sum of all things; not a mere addition of unlimited parts, but an infinite whole from which the parts derive their reality. Things come and go; but the universal order is one and infinite, an eternal unity manifested in endless variety. Manifested, but how? Shall we say that reality is only what is manifest *to us*, and make man the measure of all things? Or shall we say that the things we know are not the real things, and bewail the impotence of human faculties? Not so: that which we know is real, but it is not all. A world of thought where space and extension have no meaning, and a world of extended things in space out of all whose atoms not one thought can be built—these we know, and so far as we know them they are real. But we must believe in endless other ways of existence, to us inconceivable, and differing in kind from thought and extension and from one another, even as thought differs from extension. We cannot conceive an Infinite which is infinite in limited ways. Extension and Thought can no more fill up the sum of existence than an infinitely extended plane could fill space. They are only two of the infinite *attributes* wherein God, the one eternal self-existent *substance*, the source of all reality, brings forth, not by a special act of creation, but by the very fact of his existence, the boundless modes of being which infinite understanding can conceive. This magnificent speculation is open to grave difficulties, besides the impossibility of testing it by experience, which to us is the most obvious objection of all, but is the very last that would have occurred to any philosopher of Spinoza's time. But, inasmuch as Extension and Thought are the only attributes we have to do with, the Infinite Attributes lie outside the rest of the system, whose real contents (the psychological and ethical part) are not affected by this singularity of form. The infinite attributes may perhaps be regarded as a metaphysical *tour de force* devised to avoid (if we may use modern terms) the dangers of *Naturalism* on the one hand, and *Subjective Idealism* on the other.

This may prepare us for Spinoza's treatment of that vexed question of metaphysics, the relations of mind and matter. We are all aware of two series or classes of events which are distinct in kind. I wish to move my hand: that is a feeling, as we say, in my own mind—an event of which nobody can be conscious but myself. Each of you may have a notion of it by the analogy of what you have known in *your* own minds. But you can never have direct experience of it. My wish is a mental event, and the sum of all events of that class is the world of mind.

Again, I do move my hand. That is an event which happens in space—you can see it as well as I, or in certain positions better. It is in some way—we do not now stop to ask how—part of your experience as well as mine. This is a material event, and the sum of all events of that class is the world of matter.

How are we to imagine the relation between these two trains of events, so different in kind and yet so corresponding, as daily experience tells us, in numberless ways?

We cannot really conceive mind as acting upon matter, or matter upon mind: there is no common measure. There is no fellowship between foot-pounds and emotion, between the sensation of warmth and the mechanical equivalent of heat. One thing we may do is to give up the question as hopeless, either by saying plainly that we can make nothing of it, or by saying that mind and matter do act upon one another, but how they do so is a mystery. This last was in effect Descartes' answer. Leibnitz started the famous doctrine of pre-established harmony (which was really taken from Spinoza with a variation for the worse): mind and matter, he said, are like two clocks which are independently wound up and set to the same time, and are so perfectly made that they keep the same time ever after. But Spinoza says (to keep the very rough illustration of the clocks), There are not two clocks at all, but one clock with two faces. What we call mind, and what we call matter, are manifestations of one and the same reality—not an *unknowable* reality, for it is known in the manifestations, and they themselves constitute the reality—and they correspond just because the reality is one. Holding this view, Spinoza holds that the correspondence is exact. The chain of mental events is matched by a parallel chain of material events; but no link in the one can be a link in the other. There can be no material cause of a mental event, and conversely.

Materialism—which consists in mixing up the two classes of events—is utterly foreign to Spinoza's way of thinking.

It is impossible to discuss Spinoza's position here; and I may say at once that I do not think it strictly capable of demonstration. But this or something like it is the conclusion to which the study of life and thought aided by the lights of modern science—not direct light, for the question is not one which physical science can answer, but not the less by important side-lights—is in our own time converging. For this I call to witness the various important writings in which the problem has been treated within the last few years in our own country alone by Mr. Herbert Spencer, Mr. G. H. Lewes, Mr. Huxley, and Professor Clifford.

Spinoza's Psychology.

Having fixed his leading ideas in the earlier part of the 'Ethics,' Spinoza proceeds to apply them to the particular consideration of the human mind. The exact correspondence of material and mental phenomena is expressed in the proposition, "The order and connexion of ideas* is the same as the order and connexion of things"†—which does not mean that thought is the measure of things in the sense of idealist systems. The thing corresponding in "order and connexion" to an "idea" in my mind is not the thing I think about—or of which I have an idea—but the state of my bodily organism corresponding to the mental state. The human mind is an exceedingly complex thing made up of simpler mental elements, just as the human body is an exceedingly complex thing made up of simpler material elements.* Spinoza goes on to state the law of mental association as distinctly as any modern psychologist, and also states that there is a physical law of the organism corresponding to it.†

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* *Idea* is with Spinoza the *nomen generalissimum* for "mental state" (*mode* of the Attribute Thought). It need not be, and often is not a state of *consciousness*, or a state of *human* mind at all.

† Eth. 2, Pr. 7.

The ethical part of Spinoza's doctrine is introduced by a purely scientific analysis of the passions, in which he proposes to "consider men's actions and desires just as one would consider lines, surfaces, or solids." As to the scientific value of his results, I need only appeal to Johannes Müller, one of the first of European physiologists. When Müller, in his classical work on Human Physiology, came to speak of the passions, he expressed himself thus:

"With regard to the relations of the passions to one another, apart from their physiological conditions, it is impossible to give any better account than that which Spinoza has laid down with unsurpassed mastery. In the following statement I shall therefore confine myself to giving the propositions of Spinoza on that subject."‡ And he translates accordingly, without further criticism or comment, the greater part of the Propositions of the third part of Spinoza's 'Ethics.' I know of no other case where science has been able to accept in this unreserved way the fruits of philosophy.

Spinoza reduces the passions to the elements of pleasure, pain, and desire. Pleasure is defined as the passage from less to greater, pain as the passage from greater to less perfection: which is curiously like the account of pleasure and pain lately given by Mr. H. Spencer on biological grounds—namely, that pleasure is originally correlated to actions beneficial to the organism, pain to those which are injurious to it. Desire does not mean for Spinoza a desire of pleasant things as such. All living things, whether conscious or not, have *appetite*—a physical impulse determined by the universal tendency or *effort*, as Spinoza calls it, towards self-preservation. Desire is conscious appetite, and as such is prior to the voluntary pursuit of pleasant things as pleasant. Pleasure and desire are related not as cause and effect, but as effects of a common set of causes or functions of the same conditions. Nothing is more remarkable in Spinoza's philosophy than the ease with which these conceptions adapt themselves to our later theories of life and the "struggle for existence." It is hardly too much to say that Spinoza's notion of self-preserving *effort*—"the competence to be"—has been all this time only waiting to be filled in by Mr. Darwin. As it stands in the 'Ethics,' it is a grand conception which seems insufficiently connected with the world of sensible facts. The theory of Evolution has put it in gear, so to speak, with the working machinery of science, and filled it with life and motion. Whether by fortune or by insight, Spinoza left a gap in his system at exactly the right place; and it is no small part of a philosopher's merit to leave questions unanswered which cannot be answered with the materials existing in his own time.

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* Eth. 2, Pr. 15. † Eth. 2, Pr. 17, 18.

‡ Muller, 'Physiol. des Menschen,' vol. ii. p. 543.

Spinoza's Ethical Doctrine.

Spinoza places the foundation of moral action, as of all action whatever, in the self-preserving effort. "The foundation of virtue is no other than the effort to maintain one's own being, and man's happiness consists in the power of so doing." * But self-preservation does not lead, as might be supposed, to *selfishness*. Spinoza's morality is neither more nor less egoistic than that of the Stoics, which has often been called impracticable, but never selfish.

The principles of the two systems are in fact identical, for the Stoics really meant by "following nature" very much what Spinoza meant by "suum esse conservare." And the consequences of these principles run so closely parallel that one could hardly think it a case of pure coincidence, but for the certainty that Spinoza knew little and cared less about Greek philosophy.

Every man seeks and must seek his own welfare; that is the first law of our moral nature in the scientific sense of law—that is, a universal fact which you cannot get rid of. But it is part of the same order that man is a reasonable and sociable animal. He cannot be solitary if he would. Hence the only true self-preservation of man is to be found in society; the self-maintaining effort is not individual but social. The desire of man's nature is fulfilled only in the fellowship of reasonable men. The welfare of the citizen is one with the welfare of the city, and the life *according to reason*—a favourite expression both with Spinoza and with the Stoics—is a life devoted to the common weal.

We have seen that Spinoza carried out without exception of any kind the principle of the uniformity of nature. He applied it to the actions of men no less than to the motion of a planet in its orbit. The question is naturally asked, does this not crush morality under the load of necessity, and drive us, if we really accept it, into mere fatalism? We have no room here for any but a practical answer. The Stoics believed in universal law no less firmly than Spinoza, and the belief did not destroy—there is no evidence that it weakened—their morality.

The sentimental objection to the doctrine of universal order proceeds in great measure from a confused notion that it means an arbitrary destiny imposed upon the world as it were from outside. But this is not what Spinoza and the Stoics meant. They believed in uniformity—the unbroken "reign of law"—as the very life of the world, not outside it, but in it and of it. Every creature is part of this order and must in some way serve it; but man's privilege is to serve it consciously and therein find his happiness. The fool serves blindly and against his will, the righteous man in wisdom and gladness of heart. This is the service which is perfect freedom, to fulfil in one's own being the law of the world, which for man is the law of peace and righteousness, of love and fellowship. The man who in our own time has perhaps best understood Spinoza, and done most to make him understood—I mean Berthold Auerbach—has summed up this thought in the saying, ***Frei und Eins mit dem Gesetz:*** "Free and made one with the law." Freedom is thus the fruit and reward of righteousness—nor does she seek any other—and the wise and righteous man alone is free.

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* Eth. 4, Pr. 18, Schol.

From Ethics we should naturally pass on to Politics, but an account of Spinoza's doctrine on that subject, which offers a striking parallel, not without contrasts however, to that of Hobbes, would lead us far beyond the space at my command. I proceed, therefore, to a rapid survey of the fortunes of Spinoza's philosophy since his death.

Spinoza's Influence.

To give a complete account of Spinoza's influence would be to write the history of a great part of modern philosophy. The most notable exception would be the English school down to the end of the last century, which showed its independence of Continental thought in this as well as in other ways. Locke entirely neglected Spinoza, taking him probably for a sort of erratic Cartesian. Berkeley was to some extent acquainted with him, but was prevented, either by some similar misconception or by the repute of atheism which then attached to Spinoza's name, from giving due consideration to him. Hume barely mentions him; and if his knowledge of Spinoza's philosophy had been more than a slight and second-hand one, it is inconceivable that he should not have seen its importance. The infinite Attributes and the half mystic turn of the last book of the 'Ethics' might have pleased him little, but the excursus on Final Causes would have been after Hume's own heart; nor could he have failed to welcome Spinoza's attacks on the logical doctrine of universals and other cherished traditions of the schools. What is true of Locke, Berkeley, and Hume, is also true, so far as I know, of the English philosophical literature of the eighteenth century generally: Spinoza's name is not unfrequently mentioned, but in such a way as to show very little real knowledge of him. The writers who show most are (a strange conjunction) Clarke and Toland.

In Spinoza's own country the immediate effects of his teaching were greater than is commonly known; I do not mean by way of reaction alone, though in this manner Europe is indirectly indebted to him for the greatest physician of the last century. Boerhaave was in his youth destined for the ministry. Hearing Spinoza violently abused by a fellow-traveller one day, he asked the speaker if he had ever read Spinoza. Though he was in fact no follower of Spinoza, he got by this the reputation of being a Spinozist, and seeing his prospects of ecclesiastical advancement cut off, betook himself thenceforth to medicine.

There were more direct consequences, however, of a very curious kind. Not long after Spinoza's death there sprang up in the Dutch Reformed Church a whole tribe of heretics, whose doctrines were founded on a mystical interpretation of Spinoza's 'Ethics.' Their opinions were repeatedly and expressly condemned as Spinozistic; but after being cut off from the Church they survived in small dissenting bodies, of which remnants are said to exist at the present day. The chief names among their leaders are those of Van Hattem, (1641–1706), and Van Leenhof, who in 1703 published 'Heaven on Earth,' an ethical treatise mostly taken from Spinoza.*

In the European world of letters Spinoza was entirely misunderstood and neglected for the best part of a century. Leibnitz, the man most capable of doing him justice, preferred to take the opposite course, and he was ill-treated even by the people who might have been expected to take him up if only for the reason that he was hateful to theologians. He fared little better at the hands of Bayle and Voltaire than at the hands of orthodox apologists. To Lessing, the founder in some sort of German literature and criticism, belongs the credit of having seen and announced Spinoza's real worth. In a certain memorable conversation with Jacobi he said, in so many words, "There is no philosophy but the philosophy of Spinoza." This and much more came out after Lessing's death in a long correspondence between Jacobi and Moses Mendelssohn, which finally degenerated into a controversy. After the report of that one conversation, the record of all this is now of little interest; from these, however, and from other letters preserved among Lessing's works, the fact comes out that Lessing thoroughly understood Spinoza, and had grasped the leading points more firmly than many of Spinoza's later critics.

Meanwhile Goethe too had found out Spinoza for himself, and he has recorded how the study of the 'Ethics' had a critical effect on the development of his character.† And his statement is fully borne out by the witness of his mature work. Goethe's poems are full of the spirit of Spinoza; not that you can often lay your finger on this or that idea and give a reference to this or that proposition in the 'Ethics,' but there is a Spinozistic atmosphere about all his deeper thoughts. There is a set of speculative poems (*Gott und Welt*), which gives the most striking instances; but the same ideas are woven into all parts of his work, and may be found alike in romance, tragedy, lyrics, and epigrams.

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* See Van der Linde's 'Spinoza, seine Lehre und dessen erste Nachwirkungen in Holland.' Göttingen, 1862. The '*Revue des Deux Mondes*' for July 15, 1867, contains a notice of this work (*inter alia*) by M. Paul Janet.

† 'Aus meinem Leben,' Book 14.

The influence thus started in philosophy and literature spread rapidly. Kant's great work in philosophy was independent of it; but a strong current of Spinozism set in immediately after Kant, and acted powerfully on his successors. Fichte, though his system widely departs from Spinoza's, had obviously mastered his philosophy and felt the intellectual fascination of it; and many of his metaphysical ideas are simply taken from Spinoza. Hegel said, "You are much of a Spinozist or no philosopher at all." So Schelling also said that no one could arrive at philosophical truth who had not once at least plunged into the depths of Spinozism. Novalis, Schleiermacher, Heine, and many others have spoken of Spinoza in words of enthusiastic praise. There is in Germany a whole recent literature of exposition and discussion about him, which is fast increasing, and to give an account of which would itself need a monograph.

In France the prevailing tone of philosophy has not been one that accords well with Spinoza; but he has met there with keen and intelligent criticism, which is the next best thing to intelligent admiration; and the beautiful address lately delivered by M. Renan at the Hague (besides the serious attention given to the subject by M. Paul Janet [*] and others) is a sufficient proof that Spinoza has now at least found a response in the highest thought of France.

In England Coleridge, in this as in other things the advanced guard of the peaceful invasion of German culture and philosophy, spread the name of Spinoza, and much of his ideas, among the friends whom he delighted by his conversation. He used to say that the three great works since the introduction of Christianity were Bacon's 'Novum Organum,' Kant's 'Kritik,' and Spinoza's 'Ethics.' Coleridge's own position as to Spinoza was something like Jacobi's; he admired and honoured him without accepting his teaching. It may well be that some part of the Nature-worship of Wordsworth's poetry, which has been a most important element in our later English literature, was derived through Coleridge from Spinoza. But we must come down many years later before we find any certain manifestation of this part of Coleridge's influence. Those who have spoken of Spinoza to English readers as he deserves to be spoken of are still among us and working for us. We have Mr. G. H. Lewes' various articles and writings on Spinoza, to which he has given a finished form in his 'History of Philosophy.' We have Mr. Froude's essay on Spinoza,[†] perhaps the best general account of his doctrine which has been given in our language for those who do not make philosophy their special study. There is Mr. Matthew Arnold's admirable monograph on the Tractatus Theologico-Politicus,[‡] whose only fault is that he has not completed it by a companion piece on the 'Ethics.' There are Mr. Huxley's contributions to pure philosophy, which do not treat of Spinoza directly, but which—as well as Mr. G. H. Lewes' most recent work—have done much to put Spinoza's fundamental ideas into shapes adapted to the present state of our knowledge. Nor are other signs wanting of an active and increasing interest in Spinoza both at home and abroad.

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[*] See his paper on "French Thought and Spinozism," in the 'Contemporary Review' for May, 1877.

[†] In 'Short Studies on Great Subjects.'

[‡] "Spinoza and the Bible," in 'Essays in Criticism.'

It has been said of Spinoza by an able and not unfair critic (M. Saisset), that his theory was after all but a system, which has passed away like all other systems, never to come back. It is true that Spinoza did not found a school, and had few or no disciples in the proper sense. It would be difficult to name anyone who ever formally accepted his system as a whole. But the worth of a philosopher to the world is measured not by the number of people who accept his system, or by the failure of criticism to detect logical flaws in it, but by the life and strength of the ideas he sets stirring in men's minds. Systems are the perishable body of philosophy, ideas are the living soul. Judged by this test, Spinoza stands on a height of eminence such as very few other thinkers have attained.

It is singular that the two men whose thought has on the whole, perhaps, had the most signal effects in modern times in guiding and stimulating the thoughts of others on the highest problems of speculation, have both been members of that race which has undergone more reverses and sufferings than any other, only to rise up again and again with fresh proofs of its marvellous persistence and vitality. Their lives, conditions, and circumstances present to the outward view the most striking contrasts. The one of these men was a retired student, who never left his native land, and shrank from the distractions of public office. The other was a traveller, an orator, a shaper of new forms of society, a born ruler of men. The one compelled men's attention by the steady pressure of intellectual persuasion, the other by the impulsive stress of indomitable zeal and eloquence; the one showed in its highest manifestation the power of reason, the other the power of faith. The unlikeness is great indeed; yet their conduct and the principles which animated it present in some ways a likeness which is hardly less remarkable. Each of these men was on the point of rising to the highest distinction that in his country and time could be attained among his own people; each of them without hesitation abandoned his fairest prospects of advancement to fulfil the duty of serving the truth above all things. Both were accused of holding opinions that struck at the root of moral and social order, at the very same time that they were proclaiming in the strongest terms the sacredness of the bond that holds men together in society, and the duty of ungrudging obedience to civil government. Both of them set little store by their individual gains or losses, but also held it the duty of a citizen, for the sake of the common weal, to resist injustice in his own case no less than in another's, and were ready to act on that conviction. Reckoning themselves as they did the servants of the truth, they taught for the truth's sake and not for their own, and each of them forbade his friends to call a doctrine after his name. Though cut off from the protection and fellowship of their own law, and condemned by the professors of its letter, they never ceased to honour the spirit of it, and they both fulfilled the precept which bids the teacher live by the work of his own hands. It is hardly possible for a man to consider seriously the profoundest questions of life without at some time, and in some measure, taking the one or the other of these men for his master. One of them was Baruch de Spinoza, the Jew of Amsterdam, afterwards called Benedict; the other was Saul, the Jew of Tarsus, afterwards called Paul.

[F. P.]

WEEKLY EVENING MEETING,

Friday, April 27, 1877.

Sir W. Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

John Rae, M.D. LL.D. *M.R.I.* &c.

Arctic and Sub-Arctic Life.

Arctic life on land has this well-known peculiarity, that from the beginning of April until the first or second week in June, and in some places to a later date, every living thing has its head turned northward; the reindeer, the musk-ox, with the wolf (in close attendance, with deadly intentions), are among the earliest of the migrants, several kinds of grouse being about the same date. Then towards the last day of May, or early in June, come the geese and other aquatic birds with wonderful regularity as to date. During the two springs passed at Repulse Bay, the Hutchins goose (*A. Hutchinsii*) arrived on the one occasion on the 31st May, and the other on the 1st June.*

Then come the lemmings, travelling *always by night*, sometimes in great numbers; not one of which would have been seen, were it not that at the time our own habits were nocturnal, as during the day they were hid away under stones, snow, &c., wherever they could find shelter. There also were flies and spiders, lively and active, making their northward progress during the warmth and sun of the day, but lying dormant in a half or wholly frozen state during the cool of the night.

In the sea I had not equal opportunities of observation, but I do not think the same universal northward movement takes place there; certainly no such southward travel of marine animals was observable in the autumn as was noticed on the land. In fact, the white porpoise swims northward instead of southward on the approach of winter, in great numbers from the large rivers of Hudson's Bay. The cause of marine animals being less migratory than those on land, may be that the temperature of the sea is much more equable, never varying more than eight or ten degrees; whilst the difference between the atmospheric temperatures of summer and winter is in some places as much as 130 degrees.

It had been my intention to endeavour to mention briefly all the Arctic animals that have come under my notice, but this I find to be impossible in the short space of one hour; I have therefore chosen from among them a few only, the peculiarities of which seem to me of the greatest interest.

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* During ten years' residence at Moose, on the shores of Hudson's Bay, the first Canada geese were always first seen on St. George's Day, 23rd April, or once or twice on the previous day.

The first among these are the musk-rat, or musquash (*Fiber zebithicus*), and the beaver (*Castor fiber*), the two house-building fresh-water amphibia, a term rather improperly, but commonly, applied to these animals.

The ***musk-rat*** is found everywhere in the Hudson's Bay territory: wherever there are low marshy places, and where there is a great growth of the aquatic plants on which the animal feeds, it builds its house in pools of water several feet deep. The material used for building is mud, weeds, and grass, which are built up to the height of about four feet, in the form of a small haycock.

The floor of the dwelling is a few inches above the water level, the external entrance to which opens from underneath the water, of necessity near the bottom of the pond, for were it anywhere near the surface, it would be frozen up, as the ice forms to the thickness of one or two feet in winter. Besides the principal dwelling, a number of little breathing places, or lodges, are scattered at intervals all over the pond, at distances of fifteen or twenty yards apart. The uses of these will be obvious, when I mention that during the whole winter of five or six months' duration, from the first frost to the disappearance of ice in spring, these little animals obtain all their food from the plants growing in the pond under the ice. As the winter advances the food near the house would be consumed, and the rats would have to extend their range of feeding ground, in going to and from which a great deal of time and labour would be expended; in fact, they could not go to any very great distance, as the period they can live without breathing is very limited. They therefore prepare these little resting places all over their domain, which enables them to get without difficulty to every part of it. I have seen one of my men, when travelling in winter, not unfrequently go up quietly to one of them, knock it over with a sharp blow from an axe, and generally find a rat inside, with some of the vegetable substance on which he had been feeding lying beside him. The murdered rat on such occasions usually formed part of our supper. The musk-rat, notwithstanding that it has numerous enemies, and is constantly hunted by the Indians and others, is so prolific, that there is at present little chance of its becoming extinct. Upwards of two million skins are usually brought to London every year from America.

The *beaver*, unlike the musk-rat, always selects a running stream in which to build his house, and to make his dam. In choosing the stream, two important particulars must be considered. Care must be taken that there is a constant supply of water from a spring or other source during the whole or greater part of the winter, and that there is an abundance of the species of tree on the banks, the bark of which forms its food. Supposing then that a beaver has taken unto himself a wife, and they have decided to set up house, and that after much consultation they have fixed upon a favourable position, the first thing is to form a weir or dam, which is accomplished by cutting down one or more trees on each side, in such a manner as to fall transversely, or nearly so, across the stream; many of the branches are then cut off, and placed crosswise; after which leaves, mud, stones, and pieces of wood are accumulated on these, so that a powerful barrier is constructed; it is left a little lower on one side than the other, so that any surplus water may readily escape.

The house-building is even more arduous work than the construction of the dam, the materials composing it being mud, clay, stones, and pieces of wood several inches in diameter, put together with great ingenuity, the walls being often more than a foot thick, a strength absolutely necessary to keep out their most determined enemy, the wolverine!

The beaver house is much larger, of course, than that of the rat, and of a different shape, being much flatter, and wider on the top in proportion to its height. The floor is so placed as to be an inch or two above the level of the top of the overflow part at the dam; so there is little danger of a flooding of the dwelling similar to the almost periodical one that the poorer dwellings along the Thames are subjected to. With a method almost to be expected from such good engineers, there are to the dwelling at least two doors, presumably one for entrance, the other for exit, the external opening being under water, close to the bottom of the pond, as described in the rat house.

Unlike the rat, however, the beavers do not build up little huts for breathing places, but scrape holes in the banks of the stream instead, sloping upwards, so that although the entrance is deep under water, the upper or farther end is dry. To these they resort for feeding and fresh air, when the food is at a distance from home, or when an enemy has attacked and broken into their dwelling.

The next work is to cut down and haul into the pond a number of trees (usually the poplar) for the winter stock of food. These trees, in their green state, are heavier than water, and consequently sink to the bottom. As the bark alone is eaten, it may be imagined what a large supply is needed, and what care must be taken that a sufficient quantity is stored up, otherwise *starvation* would be certain.

For at least six weeks or two months of the autumn, the labours of the beaver must be very severe (as is indeed indicated by their being then in somewhat poorer condition than at other times), and most of their work is done in the night.

One thing has puzzled naturalists, and that is, *what* is the use of the beaver's curiously-shaped tail? Some people have asserted that it is used as a trowel for plastering and smoothing the mud of its dwelling! I do not think this is so; but one use for it I *do* know; and that is, to give an alarm on the approach of real or supposed danger. When crossing the Rocky Mountains, I tested this over and over again in calm nights, when listening to these industrious creatures hard at work at, no doubt, some kind of useful and necessary labour.

On intentionally making a noise, there was *instantly* a sharp report like a pistol shot, certainly caused by the tail being brought forcibly down on the water; after which, for an hour or more, there was perfect silence; not the slightest signs of the work being resumed could be heard. As the beavers carry the mud and clay for their house-building in their fore paws, while walking on their hind legs, it is probable that the tail forms a good support for the animal when in an upright position, at least in preventing it falling backwards.

As the whole winter, of perhaps 180 days' duration, is passed by the beaver in this house in total darkness, or in swimming abroad for food or exercise in his dam, where some amount of light may penetrate through the two or three feet of snow and ice above him, we must, in imagination, believe that the home-life of the beaver in winter is dismal in the extreme; yet the appearance of the animal when killed at this season does not warrant us in coming to this conclusion, for he is found to be in splendid condition, both as regards his corporal state, and the beauty and thickness of his fur.

When the family increases and becomes too large for the house, a portion of them leave their old habitation, and if there are still sufficient trees of the proper kind for food in the neighbourhood, these offshoots build a new dwelling on the old homestead; but if the food-producing tree is becoming scarce, there is a migration, generally down stream, when a new dam is formed, a new house built, and another colony established; and so the work goes on and on as long as the beavers are not destroyed by their enemies. The immense amount of their labours, and the large quantity of trees cut down by them, can alone be estimated by persons who have travelled through a country that is or has been much occupied by them.

A beaver sometimes separates himself from his kind, and leads a wandering solitary life. He is usually of large size, as may be told by the markings of his teeth on the trees he has been cutting, and is generally met with along the banks of large rivers such as the McKenzie and Fraser, in passing down which his roughly made bachelor establishment may be seen in some little eddy of the stream where there is a small indent in the bank. How or where he lays up his winter stock of food I am unable to tell; for he, like the more orderly members of the family, is never seen during the winter.

There is one circumstance connected with beaver life that cannot be easily explained. The house in which he lives appears to be as impervious to air as it is to light, and when the family amounts to six or eight there cannot be more than a cubic yard of air—if so much—to each individual! This quantity must last for twenty weeks or more without change or purification of any kind that I am aware of, unless indeed the water has the quality of *re-oxydizing* and *decarbonizing* the air that has been rendered impure by constant respiration. Probably the clay and mud are to a certain degree porous.*

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* That the clay and mud were sufficiently porous to admit air was suggested to me by the distinguished physicist, Professor Guthrie, F.R.S., of the educational department of the South Kensington Museum.

Each Indian or family of Indians has special hunting grounds, the boundary of which is almost as well defined as a gentleman's estate; and any hunter who killed a beaver, or destroyed the beaver house, on the lands of another, would be considered guilty of an aggravated offence, and of a disreputable act of poaching.

The *fox family* are curious as to their relations to each other, and for their wonderful sagacity.* Their skins differ widely in appearance, and also in value; that of the silver fox has been known to sell in London at as much as 45*l.* or 50*l.* for one skin, whilst the usual value of that of the red fox is not more than 20*s.* or 25*s.* Yet these two varieties may be the produce of one litter. The silver is the rarest, the cross next, and the red, no doubt the normal colour, far the most common. The white and blue or sooty differ so very much in appearance, that they are supposed by many to be distinct from each other, or that the dark skin is that of the white fox in its summer clothing. This is not the case; for both of these are winter skins in a high state of perfection, and the blue one is only a variety, and may, like the other foxes, be in the same litter of young with the white. The blue is much more rare than the white, and of much greater value.

I have only time to mention two cases, showing what I call the reasoning powers of the fox. A steel trap, set in a peculiar manner, is a very common mode of catching foxes; but there are certain of these animals, probably taught by having seen others of their kind entrapped by this means, which elude all the efforts of the trapper, who then resorts to another plan. A gun is set, with a line, one end of which is attached to the trigger, the other to the bait, generally some odorous substance. The line for several yards next to the bait is carefully covered up with snow, so that the fox cannot see any connection between the bait and the gun; usually one or more foxes are readily shot in this manner, but afterwards something like this not unfrequently happens. The trapper finds, when he visits his gun in the morning, that the line is cut, the bait gone, and the gun undischarged. To show that this is not a chance thing, I may notice another plan by which the bait is taken away in safety. The guns are usually set in some out-of-the-way place on a deep snowdrift, into which, beginning about a yard's distance from the bait, the fox scrapes a trench a foot or so deep to where the bait is. While lying in this trench he takes hold of it, sets the gun off, and trots off coolly with his supper. In both cases we see the tracks of the fox on the snow, indicating his evidently careful and cautious approach, before cutting the line; by his constant custom of scraping his trench at right angles, or nearly so, to the direction in which the gun is pointed (for if he did it in the same line, the shot would be likely to hit him), whilst the snow is ploughed up by the shot immediately over where he had been lying in his trench, and exactly where his head would have been had he taken the bait in the manner he was expected to do, there is demonstration of forethought and reason.

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* Red fox (*Vulpes fulvus*), cross fox (*V. decussatus*), silver fox (*V. argentatus*), Arctic fox, white (*V. lagopus*), Arctic fox, blue (*V. fuliginosus*).

Whilst travelling in 1851 along the shores of Wollastonland, I fell in with a number of hares feeding on a piece of ground which the sun had denuded of snow (I had approached without being seen); and while waiting patiently to get two or three of the hares in a line, so as to bag more than one, I noticed one animal that looked different from the others, yet wonderfully like, but the ears were not so long, nor the tips of them black, like those of the hare. In every other respect the likeness was complete. This proved to be a white fox that had tucked his long bushy tail underneath, hopped about, and sat up occasionally on his hind legs exactly as his companions did, always edging nearer and nearer to them; but the hares appeared aware of the deception, for they usually moved away, but not rapidly, or as if much alarmed. The fox was not within shot, so when I fired and bagged three hares he scampered off.

There are two kinds of *reindeer* (*Cervus tarandus*); the one frequenting the woods, the other, a much smaller variety, occupying the barren grounds of America. This last migrates far to the north in spring and summer, returning to its winter home on the barren lands in September and October. This deer is to the northern Indians of these parts, and to many of the Eskimos, as much a necessity of life as the seal is to the Eskimos of Greenland. The skin of the animal, in different modes of preparation, forms the clothing, shoes, blankets, and tents, and even the cords with which the animal itself is snared. Their venison constitutes the principal meat diet, and the contents of their stomach the only substitute for bread and vegetables of these people.

The female reindeer have horns (an unusual circumstance), and those of the full-grown male have a peculiar brow antler, which has led to a very curious error; for it has been represented, at no very distant time, that this palmated branch of the horn was a sort of natural shovel to scrape away the snow from the grasses and lichens on which the animal feeds. Yet it cannot be so, as these horns are shed in November, and during the whole winter the old male reindeer has no horns at all, whilst the female and young males retain their horns (which have no palmated branches) all winter, and do not shed them until about May or June. The reindeer when feeding in winter invariably uses its foot to remove the snow. They migrate as far north as Banks Land, Melville Peninsula, and to the Parry Islands; but I believe that many of them winter at the latter. They are killed by the Indian in various ways, by snaring and with the gun, formerly with bows and arrows; they are also speared in great numbers when crossing rivers.

By the Eskimos they are also speared in crossing lakes, as already mentioned, shot with bows and arrows, and caught in pitfalls dug out in the deep snowdrifts in spring. It is surprising that the Indians have never domesticated this docile animal, as the Lapps and Thuchi have done, as they are easily tamed. The only hindrance to their doing this is the superstition of the savage, who thinks that were he to do so, various misfortunes would happen to him and his people.

I know of only three Arctic animals that hibernate: there may be more. One is the *marmot*, which burrows deeply into some dry bank, and there has his winter bed. Most of these clever little fellows take up their quarters during the summer among rocks and stones, where they are safer from their many enemies.

The next I shall mention is a brown *bear* (*Ursus arctos*), resembling in colour the grizzly bear of the Rocky Mountains, nearly as large, but differing in having shorter legs. This animal feeds on roots, marmots, lemmings, and all kinds of carrion; robs deposits of provisions as dexterously as the wolverine; and at the beginning of winter stows himself in a snug hole in the earth, where he sleeps away five or six months of his life every season, the door of his bedroom being shut up with snow that has drifted over it, and keeps him warm. My acquaintance with this animal is so slight that I can tell little more about him. I endeavoured frequently to get a skeleton of this bear, but failed, because the Indians have a superstition about its being unlucky to preserve the bones, or at least all of them, of any single animal.

The female white bear (*U. maritimus*) also hibernates under certain circumstances, whilst the male roves about all winter from one sheet of open water to another in search of its constant prey, the small seal, which forms the bear's chief food. They also kill the walrus, and the white whale (*Beluga*) occasionally. The latter sometimes defers too long his voyage to the open water, where he spends the winter, and is caught by frost. Under these circumstances, he is obliged to keep a breathing place open in the ice by constantly rising to breathe. As he has generally only one of these places, he cannot go to breathe elsewhere, so if the bear finds one of these holes, all he has to do is to watch this opening, and every time the porpoise* comes to breathe, the bear fastens upon him, and by repeated attacks kills him.

The only *seal* (*Phoca vetulina*) I shall speak of is the small one, frequently called by the sealers the "floe-rat." It is abundant in Repulse Bay, particularly in the autumn; and when the sea began to freeze, it was interesting to watch them keeping breathing holes open, in doing which they seemed to splash about the water much more than at first appeared requisite, until an examination of one of these places led me to discover the cause. We all know that the specific gravity of ice is only so little less than that of water, that ice six inches thick floats with certainly not more than one inch above the surface.

I found, on going to one of these breathing places, that by the accumulation of broken ice and water thrown out by the seal, the ice was twelve inches thick, whilst only six inches elsewhere, and that round the opening it was about seven inches above the surface. The advantage of this raised ice was apparent on the first fall of snow occurring, which, if of the depth of only half an inch, was drifted by the wind into the opening, and the seal's head was not seen again, he having been able to make a small air chamber under the snow, which had the double effect of concealing him from his enemies, and, by keeping off the cold, preventing the opening from freezing up so readily. I believe each seal has two or more of these breathing places, because if he had only one, his death would be certain if his enemy, the bear or Eskimo, found that one.

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* This cetacean is as frequently called a porpoise as a whale, and it is of its skin that the much valued porpoise-leather boots are often made.

During winter the female seal scrapes out a little house, in which she has her young seal beside her, protected as far as possible by a good covering of snow from cold and danger. The chief food of this kind of seal in Repulse Bay is a small crustacean, somewhat resembling a shrimp in shape, of the family named "Grammarus," which are in such myriads, that the water seems thick with them. On opening the stomachs of seals and salmon, I always found that these formed the chief contents.

The *Eskimos* may be fairly classed into three great divisions. Those from the McKenzie river westward to Behring Strait, live in houses, or Ig-loos, built of wood; those from the McKenzie eastward to the shores of Hudson's Bay, live in huts constructed of snow; and those of Greenland have dwellings made of earth, stones, bones, &c.

Some anthropologists believe that the Eskimos migrated from the north to their present position, being driven thereto by a gradually increasing severity of climate and scarcity of game in the higher latitudes. Others, among whom is the distinguished Dr. Rink, think that the Eskimos are American, and came to the coast from the south. The third, and perhaps most popular idea, is that they migrated from Siberia, which I believe to be the true locality from which their exodus took place.

At *three places* on the American coast, far apart from each other, where I have seen the Eskimos, I learnt, through an excellent interpreter, that they had a tradition of having come from the west, or setting sun, in doing which they had crossed a narrow sea separating the two great lands. Driven out probably from Asia by some great pressure, and finding on reaching America that their advance southward into the interior was barred by numerous and hostile Indians, they were forced to migrate eastward along the sea-shores, and thus gradually extend themselves to the coasts of Greenland and Labrador, still retaining their language with so little variation, that an interpreter from the shores of Labrador can make himself perfectly understood by the natives living west of the McKenzie, several thousand miles apart.

If the theory of the migration from Siberia be true, a curious sequence of events takes place. On the shores of Siberia the winter houses of these people were constructed chiefly of earth, stones, and bones. These bones show, by their size and shape, that large marine animals, whilst forming a great part of the Eskimo's food, would yield abundance of oily substance for light and fuel to warm these Yourts or dwellings.

When the Eskimos crossed to America, they found large quantities of driftwood, and they (being from my own knowledge wonderfully adaptive) built their houses of wood, but did not use it for fuel; even to the present time (according to Dr. Simpson) burning oil in stone lamps for heat and light; whales, walrus, and seal being easily obtainable.

East of the McKenzie there is little or no wood to be found, there are no walrus, and comparatively few seals and whales, and the chief food of the Eskimos here is deer, musk cattle, and fish, which yield no oil. Here, then, they cannot build a house of wood, because they have not the materials. If they made a house of the same kind (of stone, earth, and bones) as they dwelt in on the shores of Siberia, they would have no fuel to warm it. They therefore take the very wisest step they could possibly adopt, and build a hut of snow, which without fuel is warmer than any other. These snow-hut-building Eskimos extend all the way eastward to Hudson's Bay; but when Baffin's Bay, Greenland, and Hudson's Straits are reached, the form of house used in Siberia is again resumed. The cause seems sufficiently apparent, for the wanderers have now again come to seas teeming with marine life of almost the same forms as were found on the shores of Siberia, from which they obtain oil enough for lighting and heating these otherwise wretched dwellings.

It is of the life of the snow-hut Eskimos that I shall now speak, as I know more of them than of any other. In size they are by no means so dwarfish as is generally supposed, being taller, I believe, than the average Chinaman or Japanese, whom they very much resemble both in face and figure. They are of great solidity, their defect of height arising from the shortness of their legs and neck. Their faces are pleasant, especially among the young; and all the young girls and women have very fine teeth, and remarkably small feet and hands, the latter becoming coarse and clumsy after a time, by working up seal and other skins into a condition fitted to make dresses, &c.

The duties of the women are, however, not very arduous; and they are, as far as I have seen, treated with great kindness by their husbands, showing in this respect a remarkable contrast to the Red Indian of America. The woman's duty is to attend to and trim the lamp [of which a sample was shown], and to cook when there is cooking; but as they prefer their venison and fish raw, this does not occupy much of their time; they prepare the skins, make the dresses and boots; all this entails a large amount of, however, not very hard work. Their sewing is beautiful, not only for its neatness, but for the manner in which it is done, with very rough materials, so that the seams are perfectly water-tight. Their best winter dresses are always made of the finest of the reindeer skin; two coats being worn, the inner one with the fur inwards, the outer one (which is generally taken off when indoors) having the fur outside.

Their beds are made of a bank of snow covered with two or three folds of thick, hairy deerskin, over which there is one blanket of the same material, large enough to cover the whole family or occupants of the hut. When going to rest at night, they generally remove the upper part of the dress, just as our athletes do when they are to run or row a race, but with an entirely opposite object. The athlete strips himself for the purpose of coolness, the Eskimo does so to keep himself warm.

The routine of Eskimo life at or near Repulse Bay is as follows. In the autumn and early winter they live in tents made of deer skins with the hair on (the tents for spring and summer being of skins with the hair removed), which they set up in a convenient position for killing reindeer, usually on the south shore of some narrow lake lying east and west or at right angles to the route of migration, across which the animals readily swim, instead of going several miles out of their way to get round the lake.

When in the water they are chased in the swift kayak and speared in great numbers, and dragged easily on shore afterwards, as they float on the surface when killed. If the season is favourable, sufficient venison is obtained and put into "cache" to last the whole winter; and with this for food, and some carefully hoarded up oil from seals killed the previous spring, the cold season is passed in plenty and comfort. Several families assemble, build their snow huts close to one of these provision depôts, and live upon its contents until finished, when they remove to another depôt, build a new village of huts, and go through the same process as before. Their chief meal, as is common among all natives of America, is eaten before going to bed.

During this time they are not idle, for the men have their hunting weapons, and perhaps frames of canoes, to make, whilst the women are occupied in sewing by the light of their lamps; the children are being educated; the girls are stitching pieces of skins together in the form of dolls' clothes, &c., whilst the boys are making miniature sledges, bows and arrows, and spears. When the weather is not too stormy they also angle for trout. This kind of life, if provisions hold out, continues until late in spring, when all move to the sea-coast, first for the purpose of killing seals, the skins of which are wanted for making boots and canoes, and the oil for light and cooking during the following winter. The seal killing goes on until the ice begins to break up, when a number of families resort to the mouth of some small river, which, whilst very shallow, discharges a considerable amount of water; across the mouth of this is built a slight barricade of stones, about two feet high, put together in such a way that the water runs through easily, but the fish cannot pass. Over this barrier, which is much below high-water mark, the salmon pass with the flowing tide, but are prevented returning with the ebb, and remain in pools about two feet deep (a necessary part of the arrangement), where they are struck with a fish spear, such as I show you, far superior in almost every respect to anything of the kind I have ever seen used by civilized man.

As soon as the salmon fishing is concluded, which lasts two weeks or so, they occupy their time in various ways: some go to the sea and along coast in search of walrus, bears, &c., whilst others go to the usual feeding grounds of the musk-ox, the skins of which make an excellent covering of the snow bed to sleep upon, and the horn of the bull is very useful to form into a dish or ladle, one of which may be seen in the library.

As to their social habits, they are a cheerful, lively, gossiping race, who spend most of their spare time in games of various kinds, and in concerts, a combination of the vocal and instrumental; these frequently taking place in the night, which in June and July is as light as day.

These Eskimos were cleanly in their persons and dresses, quite as much so in the winter time as we could be ourselves; for it was almost impossible to wash in the winter, as the water immediately froze on the hair and beard, and the only way in which to get a comb or brush through it was by holding your head under the blankets. Our usual wash was a rub of dry snow, in which material our blankets and flannels were well trampled once or twice a week, with excellent effect.

Their domestic relations are praiseworthy, the women being treated with much kindness and consideration; and I am sure if there were such an institution as a parliament in the far north, they would not only have a vote, but probably be eligible for a seat in that important assembly. I am certain that if the Eskimos had a Geographical Society or a Hydrographic Department, their women, from their knowledge of the coast, and the neat manner in which they make a sketch of it, would be unanimously voted fellows of the one and appointed officials of the other. A chart of about 200 miles of coast, drawn by a woman of Ig-loolik for Sir Edward Parry, I found, on my survey, to be wonderfully accurate.

They readily accommodated themselves to the ways and customs of my men, all steady, fine fellows, and at once the men and boys took to smoking tobacco, although they had never done so before. On giving one of the women a silver fork to eat some food with, she handled it as neatly as if she had been accustomed to its use all her life; and when taking some tea and biscuit, said she had tasted them before when a little girl at Ig-loolik, when on board large Oo-miaka-yukes (ships) there—Parry's vessels of course, which had been at the Straits of Fury and Hecla about twenty-four years before.

The Eskimos are said to be very untruthful: I did not find them so, and I had good opportunities of putting this to the test. I had with me the narratives of Ross and Parry, the contents of which my very excellent interpreter did not know; but through him I got from the Eskimos information identical with what was to be found in these books; and more than this, they mentioned things that had occurred twenty-four years before, which were not in Sir Edward Parry's narrative, but which Sir Edward told me he perfectly remembered to have occurred, although thought of too little interest to be recorded. They did tell falsehoods occasionally, which might be in some measure justifiable, because they did so to endeavour to prevent us from travelling over their favourite hunting grounds, believing that we would kill or frighten away the game, or destroy caches of provisions they might have there, upon which perhaps the lives of these poor people depended.

Their delicacy of feeling was shown in a curious way. Whenever my men were about to take the kettle off the fire at breakfast or dinner time, any natives then in the tent immediately got up to go away, and would have done so unless asked to remain; the same thing often occurred when they saw my servant bring me my dinner.

Their gratitude for kindness was shown in a refinement of manner that could not be excelled in the most civilized community. When going to a distance in spring to kill seals, the young men asked leave that some three or four old people might be allowed to encamp near us, so as to be under our protection, which of course was readily granted, with a promise that we would give them some food if we had any to spare. For a fortnight or three weeks these old folks lived near us, and never asked for anything; and the only way I could learn whether they had food or not, was by sending over my man Corrigal (who was as great a favourite with the natives as he was with everyone else) to find out, and supply them if required.

When the seal-hunters returned, they thanked me for my kindness; and as my men wanted some seal fat to fry their venison with, I told them to buy some; but a deputation of the natives came with the interpreter, and said that as I had been so good to their people, they would supply us with what fat we required, but would not receive any pay for it. Every morning a large piece of seal grease was laid at the door of the men's tent.

They believe in a good and a bad spirit, and say that the good spirit is so beneficent that he will not himself hurt them, but he will leave them to the evil influence of the bad spirit if they do what is wrong. They therefore propitiate the spirit of evil. They believe that meteors (falling stars) and the brightly moving aurora are the spirits of their dead visiting each other in the heavens.

Whenever the Eskimos come in contact with civilized man they are found to be docile, and to assimilate themselves to our best qualities; such has been the case at Churchill, in Hudson's Bay, among the missionaries at Labrador, and among the Danes in Greenland; whilst several of the crews of the American Arctic Expeditions have been indebted for their lives to them. No braver, more affectionate, or more trustworthy men were ever seen than Augustus, the interpreter to the Franklin Overland Expedition; than Albert, the interpreter of Sir John Richardson's Searching Expedition of 1848; or the Eskimo and his wife who accompanied the American, Hall, in most of his Arctic wanderings.

Surely, people with such good qualities and such beautiful ideas have a right to a higher rank amongst the uncivilized races than has hitherto been awarded them.

[J. R.]

ANNUAL MEETING.

Tuesday, May 1, 1877.

William Pole, Esq. M.A. F.R.S. Manager, in the Chair.

The Annual Report of the Committee of Visitors for the year 1876, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 84,000*l.* entirely derived from the Contributions and Donations of the Members.

Seventy-two new Members paid their Admission Fees in 1876.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1876.

The Books and Pamphlets presented in 1876 amounted to about 164 volumes, making, with those purchased by the Managers, a total of 394 volumes added to the Library in the year, exclusive of periodicals.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, D.C.L.
Treasurer—George Busk, Esq. F.R.C.S. F.R.S.
Secretary—William Spottiswoode, Esq. M.A. LL.D. Treas. R.S.
Corresponding Member of Academy of Sciences, Paris.

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Thomas Sopwith, Esq. M.A. F.R.S. F.G.S.
George Andrew Spottiswoode, Esq.
Sir Harry Verney, Bart. M.P.

WEEKLY EVENING MEETING,

Friday, May 4, 1877.

William Spottiswoode, Esq. LL.D. Treasurer R.S.
Secretary and Vice-President, in the Chair.

The Rev. W. H. Dallinger, F.R.M.S.

Recent Researches into the Origin and Development of Minute and Lowly Life-forms: with a Glance at the Bearing of these upon the Origin of Bacteria.

Biological science to-day presents us with a magnificent generalization; one that stands upon so broad and ever-dilating a basis, that enormous masses of detail, and many interlocking demonstrations, are, of necessity, still relegated to the quiet workers of the present, and the earnest labourers of the years to come. But to those whose grasp is wide enough, and their insight profound enough, it is a generalization absolutely immovable, because based and pillared upon *truth*. And that which lies within it, and forms the fibre of its fabric, is the establishment of a *continuity*—an unbroken chain of unity—running from the base to the apex of the entire organic series. The plant and the animal, the lowly organized and the highly organized, melt into each other, and are one splendid organic whole.

But does this imposing continuity find its terminus on the fringe and border of the organic series, and for ever pause *there?* or can we see it pushing its way down and onward into the unorganized and the not-living, until all nature is an unbroken sequence and a continuous whole?

That such a sublime continuity may be philosophically hypothecated I unhesitatingly believe. That its existence is absolutely essential to the existence of nature as we see it, I have no desire to dispute. That there *has been* a line of continuity, I believe that the facts presented by the natural and physical sciences may fairly warrant. But that we have *found* that line, that *data* have been presented to us demonstrating how, and by what path, the inorganic passes to the vital, the living into the not-living, I with equal fearlessness dispute. "The properties of living matter distinguish it absolutely from all other kinds of things;" and the facts to-day in the hands of the biologist "furnish us with no link between the living and the not-living."

That this is an inference which has been fiercely disputed, I need not remind an audience in this theatre. The data for such dispute, and the reasonings based upon them, are not now my concern. They have been carefully examined and weighed by the most competent biologists in the world; and it is by them distinctly affirmed that "spontaneous generation" is no *discovered* part of nature's processes.

But we may pause for a moment to consider what was the nature of the proofs relied upon to establish the "spontaneous," or not-living, origin of living things. They were chiefly thermal experiments upon the lowest septic organisms, without an attempt to discover what was their life-history, and whether they propagated by germs or not. It was argued that the adult organism being killed at a given temperature, much below the boiling point of water, if an infusion were boiled, with every possible precaution, and, whilst boiling, hermetically sealed; then if after a lapse of time on opening the vessel the organisms were found in a living state, they must have arisen *de novo;* that is, the not-living would have produced the living.

That this method is useful, and that it must be pursued in an exhaustive inquiry into the whole subject, must be freely admitted. But that it is the best, or at least the only method of inquiry for the *biologist*, we may gravely doubt. The difficulties which as a method encumber it are simply without number. They cannot be exaggerated; they cannot be too keenly sought for.

Six months employed in such experiments by a mind trained to exactness, a mind which will have nothing if it cannot have facts, and which will admit only inevitable sequences, will be quite enough to establish a conviction of the almost infinite sources of possible error, and show bristling ranks of difficulty.

I frankly confess my conviction to be that if this method is to yield a scientific result, it must be in the hands of the highest masters of precise experiment; and therefore to my mind the experiments of Professor Tyndall on this subject have a value entirely their own.

On the other hand, the biologist is eminently concerned with the organism proper, its *morphological* mutations and developmental history. It will most conserve the science of things that live, to study, to the very border of possibility, *living things*—not as masses of matter to be manipulated chemically or physically as a whole,—but as *individual life-histories*. And this in the case before us was the more important as in no instance was the complete life-cycle of a septic organism known.

This fact early and deeply impressed me, and fully ten years since I was animated with a hope that such a life-history might be worked out. Four years were spent in accumulating facts, obtaining experience, devising methods, and becoming thoroughly acquainted with the nature of the problem; and it then became plain that to do it effectually it was needful, *First*, to employ lenses of the highest attainable power, and the utmost perfection of structure. *Second*, it was absolutely necessary that the forms studied should be watched *continuously* from the earliest to the last condition. Without continuity of observation there could be no *certainty* of continuity of development. A broken observation is for the end in view a useless observation. The attempt to *infer* or deduce developmental changes without unbroken following of these minute forms is a scientific impropriety, of which experience has only increased intolerance. There *must be* unbroken observation. *Third*, in order to accomplish this, means must be devised by which a minute drop of a septic fluid under a delicate cover-glass could be kept from evaporating for an indefinite time, and yet be continuously examined with the highest magnifying powers constructed. And, finally, for the efficient accomplishment of the work two observers were needful, to secure at once continuity and confirmation. Happily, each of these conditions was met or complied with. The last was not the least. Dr. Drysdale, an accomplished physiologist, saw at once the force of the position and the broad bearings of the subject, and eagerly desired to engage in it; and when preparation was mature, we entered on the work together. Our plan of labour was, never to accept what we had not mutually confirmed, and to relieve each other in following a metamorphosis from its beginning to its close.

You are aware that putrefactive organisms present an immense variety both in form and size. The Bacteria group themselves, are immensely diverse.* Between this giant, *Spirillum volutans*, and this minute form, *Bacterium termo*, there is a vast divergence, and yet this huge spirillum rarely exceeds on an average, except in cases of excessive development, from the 1500th to the 2000th of an inch in length.

But you may see here forms analogous, but not Bacteria. They are septic organisms strictly; usually the accompaniments of a more prolonged putrescence, and popularly known as *Monads.* They are larger, and in many ways more amenable than the Bacteria; and we fixed on *these* as the subject of our earliest work. By quiet persistence we worked out six forms, and the continuous history of *two* of these I will now endeavour to present.

I will not take them in the order of our researches, but shall find it best conserve my purpose to examine the largest and the smallest of the organisms. The appearance of the former is now before you here. It is divergent from the common type when seen in its perfect condition, avoiding the oval form; but it resumes it in metamorphosis. It is comparatively huge in its proportions; its average extreme length being the 1000th of an inch. Its normal form is rigidly adhered to as that of a Rotifer or a crustacean. Its body-substance is a structureless sarcode. Its differentiations are a nucleus-like body, not common to the monads; generally a pair of dilating vacuoles, which open and close like the human eyelid, ten to twenty times in every minute; and lastly, the unusual number of four flagella. That the power of motion in these forms and in the Bacteria is dependent upon these flagella, I believe there can be no reasonable doubt. In the monads the versatility, rapidity, and power of movement is always correlated with the number of these. The one before us could sweep across the field with majestic slowness, or dart with lightning swiftness and a swallow's grace. It could gyrate in a spiral, or spin on its axis in a rectilinear path like a rifled bullet. It could dart up or down, and begin, arrest, or change its motion with a grace and *power* which at once astonish and entrance. The flagella are extremely fine, and their number was at first with difficulty determined. Indeed, every observer has noticed how difficult it is to *discover* the unknown, but when once it has been found, how easily in comparison it may be recalled or seen again. Fixing on one of these monads, then, we followed it doggedly by a never-ceasing movement of a "mechanical stage;" never for an instant losing it, through all its wanderings and gyrations. We found that in the course of minutes, or of hours, the sharpness of its outline slowly vanished, its vacuoles disappeared, and at last, it lost its sharp caudal extremity, and was sluggishly amœboid. This condition intensified, the amœboid action quickened, the agility of motion ceased, the nucleus body became strongly developed, and the whole sarcode was in a state of vivid and glittering action. If now it be sharply and specially looked for, it will be seen that the root of the flagella *splits*, dividing henceforth into two separate pairs. At the same moment a motion is set up which pulls the divided pairs asunder, making the interval of sarcode to grow constantly greater between them. During this time the nuclear body has commenced, and continued, a process of self-division. From this moment the organism grows rapidly rounder, the flagella swiftly diverge, a bean-like form is taken, the nucleus divides, and a constriction is suddenly developed. This deepens, the opposite position of the flagella seen here ensues, the nearly divided forms now vigorously pull in opposite directions, the constriction is thus deepened, and the tail formed. The fibre of sarcode, to which the constricted part has by tension been reduced, now snaps, and *two* organisms go free.

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* Transparencies drawn from nature by a simple method devised by Mr. Dallinger were exhibited by the oxy-hydrogen microscope.

Now it must be remembered that this body is not a homogeneous rod of sarcode like a bacterium. That *it* should by mere growth elongate and then divide by fission is not so wonderful. But here we have an organism of distinctive form and some structure; yet each part divides to form a counterpart to itself, and the whole process may be complete in five minutes, and in the separated individual commence again to divide in twenty minutes more; so that in a few hours a single form will in this way have given rise to an almost inconceivable host.

But it will have struck you that the new organism enters upon its career with only *two* flagella, and the normal organism is possessed of four. But in a few minutes—three or four at most—the full complement were always there. *How* they were acquired it was the work of months to discover, but at last the mystery was solved. The newly-fissioned form darted irregularly and rapidly for a brief space, then fixed itself to the floor, or to a rigid object, by the free ends of its flagella, and with its body motionless, an intense vibratory action was set up along the entire length of these exquisite fibres. Rapidly the ends split, one half being in each fibre set free, and the other remaining fixed; and in 130 seconds each entire flagellum was divided into a perfect pair.

Now, the amœboid state is a notable phenomenon throughout the monads, as precursive of striking change. It appears to subserve the purpose of the more facile acquisition and digestion of food at a crisis. And this augmented the difficulty of discovering further change; and only persistent effort enabled us to discover that with comparative rareness there appeared a form in an amœboid state that was unique. It was a condition chiefly confined to the caudal end; the sarcode having become diffluent, hyaline, and intensely rapid in the protrusion and retraction of its substance, while the nuclear body becomes enormously enlarged. These never appear alone: forms in a like condition are diffused throughout the fluid, and may swim in this state for hours. Meanwhile the diffluence causes a spreading and flattening of the sarcode, and swimming gives place to creeping; while the flagella violently lash. In this condition, two forms meet by apparent accident—the protrusions touch, and instant fusion supervenes.

In the course of two minutes there is no disconnected sarcode visible; and in five to seven minutes the organisms are completely united, the swimming being again resumed, the flagella acting in apparent concert. This may continue for two or more hours, when movement begins to flag, and then ceases. Meanwhile the bodies close together, and the eyespots or vacuoles melt together. The two nuclei become one, and disappear, and in eighteen hours the entire body of "either has melted into other," and a motionless, and for a time irregular sac, is left.

This now, becomes smooth, spherical, and tight, being fixed and motionless. This is a typical process; but the mingled weariness and pleasure realized in following such a form without a break, through all the varied changes into this condition, is not easily portrayed.

But now the utmost power of lenses, the most delicate adjustment of light, and the keenest powers of eyesight and attention must do the rest. Before the end of six hours the delicate glossy sac opens gently at one place, there streams out a glairy fluid densely packed with semi-opaque granules, just fairly visible when their *area* was increased six millions of times; and this continued until the whole sac was empty, and its entire contents diffused.

To follow with our utmost powers these exquisite specks was an unspeakable pleasure. A group seen to roll from the sac when nearly empty, were fixed and never left. They soon palpably changed by apparent swelling or growth, but were perfectly inactive; but at the end of three hours a beaked appearance was presented. Rapid growth set in, and at the end of another hour—how has utterly baffled us—they acquired flagella and swam freely; in thirty-five minutes more they possessed a nucleus, and rapidly developed, until at the end of *nine* hours after emission a sporule was followed to the parent condition, and left in the act of fission. In this way, with what difficulties I need not weary you, a complete life-cycle was made out.

And now I will invite your attention to the developmental history of the *most minute* of the six forms we studied. In form it is a long oval; it is without visible structure or differentiation within; and is possessed of only a single flagellum. Its utmost length is the 4000th of an inch. Its motion is continuous—in a straight line—and not intensely rapid nor greatly varied, being wholly wanting in curves and dartings.

The copiousness of its increase was, even to our accustomed eyes, remarkable in the extreme; but the reason was discovered with comparative ease. Its *fission* was not a division into two, but into many. The first indication of its approach in following this delicate form was the assumption, rapidly, of a rounder shape. Then followed an amœboid and uncertain form with an increased intensity of action, which lasted for a few moments, when lassitude supervened; then perfect stillness of the body, which is now globular in form; while the flagellum feebly lashed, and then fell upon and fused with the substance of the sarcode: and the result is a solid, flattened, homogeneous ball of living jelly.

To properly study this in its further changes, a power of from three to four thousand diameters *must* be used; and with this, I know of few things in the whole range of minute beauty more beautiful than the effect of what is seen.

In the perfectly motionless flattened sphere, without the shimmer of a premonition, and with inconceivable suddenness, a white cross smites itself, as it were, through the sarcode. Then another with equal suddenness at right angles; and while with admiration and amazement one, for the first time, is realizing the shining radii, an invisible energy seizes the tiny speck, and, fixing its centre, twists its entire circumference, and endows it with a turbined aspect.

From that moment intense interior activity became manifest. Now the sarcode was, as it were, kneading its own substance; and again an inner whirling motion was visible, reminding one of the rush of water round the interior of a hollow sphere on its way to a jet or fountain. Deep fissures or indentures showed themselves all over the sphere; and then, at the end of ten or more minutes all interior action ceased, and the sphere had segmented into a coiled mass.

There was no trace of an investing membrane; the constituent parts were related to each other, simply as the two separating parts of an ordinary fission; and they now commenced a quick writhing motion like a knot of eels, and then, in the course of from seven to thirty minutes, separated, and, fully endowed with flagella, swam freely away, minute but perfect forms, which by the rapid absorption of pabulum attained speedily to the parent size.

It is characteristic of this group of organic forms that multiplication by self-division is the common and continuous method of increase. The other and essential method was comparatively rare, and always obscure. In this instance, on the first occasion the continuous observation of the same "field" for five days failed to disclose to us any other method of increase but this multiple-fission; and it was only the intense suggestiveness of past experience that kept us still alert, and prevented us from inferring that it was the *only* method.

But eventually we perceived that while this was the prevailing phenomenon, there were scattered amongst the others, forms of the same monad *larger* than the rest, and with a singular granular aspect towards the flagellate end. Its granulated forepart may be easily contrasted with the normal or ordinary form. Now by doggedly following one of these through all its wanderings, a wholly new phase in the morphology of the creature was revealed. This roughened or granular form seized upon and fastened itself to a form in the ordinary condition. The two swam freely together, both flagella being in action; but it was shortly palpable that the larger one was *absorbing* the lesser. The flagellum of the smaller one at length moved slower, then sluggishly, then fell upon the sarcode, which rapidly diminished, while the bigger form expanded and became vividly active until the two bodies had actually fused into one.

After this its activity diminished; in a few minutes the body became quite still, leaving only a feeble motion in the flagellum, which soon fell upon the body-substance and was lost. All that was left now was a still, spheroidal, glossy speck, tinted with a brownish yellow.

A peculiarity of this monad is the extreme uncertainty of the length of time which may elapse before even the most delicate change in this sac is visible. Its absolute stillness may continue for ten, or it may be prolonged for thirty-six hours. During this time it is absolutely inert; but at last the sac—for such it is—opens gently, and there is poured out a brownish glairy fluid; at first the stream is small, but at length its flow enlarges the rift in the cyst, and the cloudy volume of its contents rolls out, and the hyaline film that enclosed it is all that is left. The nature of the outflow was like that produced by the pouring of strong spirit into water. But no power that we could employ was capable of detecting a *granule* in it. To our most delicate manipulation of light, our finest optical appliances, and our most riveted attention, it was a homogeneous fluid, and nothing more.

This for a while baffled and disturbed us. It lured us off the scent. We inferred that it might possibly be a fertilizing fluid, and that we must look in other directions for the issue. But this was fruitless, and we were driven again to the old point, and having once more obtained the emitted fluid, determined to fix a lens worked up to 5000 diameters upon a clear space over which the fluid had rolled, and near to the exhausted sac, and ply our old trade of *watching*—unbroken observation.

The result was a reward indeed. At first the space was clear and white; but in the course of a hundred minutes there came suddenly into view the minutest conceivable specks. I can only compare the coming of these to the growth of the stars in a starless space upon the eye of an intense watcher in a summer twilight. You knew but a few moments since a star was *not* visible there, and now there is no mistaking its pale beauty. It was so with these inexpressibly minute sporules; they were not there a short time since, but they grew large enough for our optical aids to reveal them, and there they were. And here I would remark that these delicate specks were unlike *any* which we saw emerge directly from the sac *as* granules. In that condition they were always semi-opaque; but here they were transparent, and a brown yellow, the condition always sequent upon a certain measure of growth.

To follow these without the loss of an instant's vision was pleasure of the highest kind. In an hour and ten minutes from their first discovery, they had grown to visible specks. In two hours more the specks had become beaked and long; and the pointed end was universally the end from which the flagellum emerged, which was drawn ninety minutes after the last, is perfectly formed.

With the flagellum comes motion, and with that abundant pabulum, and therefore rapid growth. But when motion is attained, we are compelled to abandon the mass and follow *one* in all its impetuous travels in its little world; and by doing so we were enabled to follow the developed speck into the parent condition and size, and not to leave it until it had, like its predecessors, entered on and completed its wonderful self-division by fission.

In this way its life-history was known, and as it were re-entered.

It will be seen then that the equivalent of a genetic process of reproduction, universal, in some form, throughout the entire range of biology, is absolutely essential in the *monads;* and is thus carried to the very border and fringe of the outermost organized entity.

Nor is this all. There is no caprice even in *this* realm. There would be leaps, and halts, and uncertainties here surely if anywhere in the entire biological series. But it is not so. The cycle of a monad's life is as unvarying as that of a Batrachian. There is no unusual, no intense method; nothing more, indeed, than those resulting from the secular processes involved in the great Darwinian law.

I have already told you that we worked out *six* of these remarkable forms. They are all fairly represented in the histories I have given you. Five of them poured out from their genetic cysts, spores or ova, and one discharged living young. These spores or ova, and young, were, in *every* instance, followed from the moment of their emission from the sac, through all their changes to the parent condition or mature state, and each instance was several times repeated.

It will be manifest then that such a fertilized product—one of such exquisite minuteness—being found, it would be a matter of deep interest to ascertain if these spores possessed *any* power to resist the action of heat, *greater* than that possessed by the adult. We determined to conduct experiments so as to discover *directly* what was the temperature *beyond which* the spore would not go through their developmental processes.

It was settled to our complete satisfaction that the death-point of the adult organisms hovered extremely near 140° F. We could therefore start with this.

We took a series of slips of glass, and placed upon them for examination drops of the septic fluid containing each of our six monads. We carefully examined and watched these with our continuous stage and high powers, until in every case we had convinced ourselves by actual observation that spore had been emitted. This was done in every separate investigation with not less than six slips for *each* of the *six* organisms. These slips of glass—covered of course with thin discs of cover-glass—were put into a cold metallic box arranged for heating. This was closely covered, and the bulb of a long delicate thermometer was so placed as to be in the centre of the box, while its stalk rose above the cover into the air, and could easily be read.

This box of air was now slowly raised to any required temperature; and when that was reached, it was maintained for at least ten minutes. In this way, with a large series of specimens, we passed in the course of months, by a series of successively higher temperatures, from 140° F. to 300° F.

When the glass slips were cool, they were taken out and at once examined. I need not say that in every case they presented a dry amorphous appearance, in which little structure could be discovered.

We then took some freshly distilled water, which was again boiled in a platinum capsule previously heated red hot, and with a red-hot platinum wire plunged into it and allowed to cool, a drop of water was taken and placed at the edge of the covering glass; this was at once drawn in by capillarity, and the slide was put into the non-evaporating stage for immediate examination.

Now it should be observed that even this amount of precaution was *unnecessary* for the end in view; for in our examination, after heating, we disregarded whatever else might appear but the spore for which we were looking, and which we knew existed in the unheated fluid, and we confined ourselves to the development of these, with which we were fully familiar.

In every case then you will understand that in the instances of which I shall now speak, the spore were followed *after* heating, just as they had been *before;* from the beginning through all their changes to the end—the adult condition.

In one case—the cyst of the monad producing *living* young—the highest temperature that could at all be borne, and that only feebly, was 180° F. In other cases the spore were seen repeatedly to become vital and perfectly develop after exposure for ten minutes to 300° F. These were the two extremes. In the remaining instances, the highest temperature which the spore would bear and afterward develop, was 250° F. Thus if we include the young emitted from the cyst in a living state, the genetic products of these six minute and lowly organisms, possess a heat-resisting power greater than their parents in the proportion of 11 to 6. In other words, the minute spore can resist heat nearly twice as intense as the adult.

Now in relation to the question of so-called "spontaneous generation" as it has been discussed during the last five years, this is a fact that cannot be ignored. Let it be remembered that all the organisms whose life-histories I have dealt with, are as entirely septic organisms as the Bacteria themselves. Let it be further remembered, that the largest of these monad forms, described to you to-night, does from its general conformation, and such differentiation as it possesses, point upwards in the organic series toward such forms as *Stylonichia pustulata* and the *Paramœcia* themselves.

Whilst on the other hand the smallest of these organisms, whose history we have also examined to-night, is smaller than many Bacteria, is indeed only three times as large as a large *Bacterium termo* itself, and, like it, is absolutely without any differentiation, or discoverable structure. But both these forms, and all the forms standing between them, that have been completely studied, are propagated essentially and ultimately by spore, ova, or whatever else such a primitive representative of a genetic product may be called. That is to say, nature adopts the same method substantially in the lowest regions of organic forms as in the entire series above it.

As far down as we can reach, as far down as modern optics can carry us—down indeed to a point which not only *touches* the Bacteria themselves, but in some sense overlaps them—it is still true that the simplest organized being is only capable of deriving existence from other organized beings—parents, that have lived before it.

Can it be philosophical then, with the actual life-history of the *Bacteria* still unknown, to assume a *new* method of propagation—otherwise unknown in nature—for them?

By means of the monads—the very least and lowest of them—we appear to stand as it were in the *middle* of the Bacterial series. Why, then, should the Bacteria be thought more likely to grow spontaneously than these kindred forms that lie so near them?

To point to the exceptional results of a few *physical* or chemical experiments which future investigation may fully explain, is in reality no answer. Besides which the light pierces even farther down than I have carried you. We have been at work upon the *Bacteria* now for eighteen months. Whether *their* life-history or histories will ever be worked out or not, I cannot tell; the tangle of varieties is so great, the difficulties so immense, that it can only be as the fruit of years of patient work.

But Professor Tyndall has put new life into us in the study of this question. If one of the *monads* shed a spore too fine for our most powerful glasses to detect, is it not a natural inference that if the Bacteria propagate ultimately by the same means, that such spore *must* be invisible to our utmost optical aids?

By the electric beam, as used by Dr. Tyndall under conditions which you know so well, particles indefinitely ultra-microscopic may be clearly demonstrated.

In a suitable closed chamber, in which the motes of the air have time to subside, the electric beam will demonstrate its optical purity by leaving no trace of its path; but whilst the most exquisitely minute particles linger in the air, the pale track of the beam will show that they are there.

You further know that Professor Tyndall has shown that if putrefactive infusions, filtered, be boiled in, and exposed to such a moteless, or optically pure air, that Bacteria—putrefactive organisms—do *not* appear; but if on the other hand the air be charged with the finest of ultra-microscopic motes, the same infusion will become at once infected, and swarm with Bacteria.

The inference, backed by what we know of all the organic series above, is, that these otherwise invisible motes are Bacteria germs.

Permit me briefly to test this inference by facts.

It has been shown that there not only are monad germs, but that they vary in size. During these researches, we found that an old maceration of fish reduced by putrefactive action to pulp, might be dried, and baked in the sun; and if it were re-moistened, it would soon give rise to a prolific host of the monads it might contain. When the mass was dry it was light, porous, and friable; easily indeed reduced to an impalpable powder. Clearly this powder must be rich in monad germs. Now as soon as I read Professor Tyndall's experiments on the behaviour of the mote-laden air as the origin of Bacteria, I at once asked myself, "May not the inference that these motes are germs be strengthened or otherwise by diffusing the dust from an old fish maceration known to be charged with certain monads, in a suitable chamber with a suitable fluid? Should not the one behave practically as the other?" This I determined to test. I will pass over my earlier experiments, and give you merely the results of the later. A suitable chamber was prepared with all the required appliances. A fluid was needed known to be free from germs. This was supplied by a compound of inorganic elements known as "Cohn's nutritive fluid." It contains no albuminous matter, but only mineral salts and tartrate of ammonia. While the tartrate of ammonia is kept apart from the fluid no organic forms can arise in it, but when mixed with the other ingredients, even the Paramæcia will live and flourish if placed in it.

I took then the dried remainder of an old infusion charged immensely with two *monads;* the larger—the first I described to you to-night—and another, a smaller one. I exposed it first to a temperature of 150° F.—10° higher than the heat needed to kill the adult monads. It was then reduced to powder, quite impalpable, and laid upon a plate of glass evenly distributed, and once more heated to 145° F. for ten minutes—5° higher than was required to kill the adults.

This powder was now diffused through a chamber, in the main like Professor Tyndall's, and a beam from the lime-light was sent through. It was so intensely luminous that time was given for subsidence. Six small basins of the fluid were then exposed, and left for twenty-four hours; and then four more basins of fluid were exposed.

At the end of four days more the air in the chamber was optically pure; all its motes had subsided. The first six basins were then withdrawn and examined. Fifteen drops were taken from each basin, and laid upon the microscope. In *every drop* from *every vessel* the *large monad* appeared, while the smaller monad appeared plentifully in *every vessel.*

Two days after the other four vessels were taken out and examined. Thirty drops were taken from each vessel, but to my surprise the *large* monad was *wholly wanting* in *three of the vessels,* and only feebly present in the fourth.

But now the air was optically pure; five vessels of fluid were inserted, and left untouched for five days. They were then carefully examined, thirty drops being taken from each; but not a single monad of any kind was found in any vessel. Only Bacteria were there; against which no protection had been provided, and which were expected. This was repeated three times during four months, and with, on the average, unvarying results. Thus an atmosphere—the motes of which were *known* to be charged with the germs of monads—inevitably "smote" the nutritious fluid with the monads in question. But in an atmosphere made optically pure by the subsidence of these mote-germs, the same fluid was absolutely without the trace of a monad.

Further, I found that if the comparatively large germ of the largest monad were intimately mixed with dust containing the smallest monad, whose germ, as I explained just now, was not visible until it had grown; and if these two together were diffused through the air of the chamber, it was found that whilst the germs of the large monad had all fallen through the air in thirty hours, the germs of the smaller and lighter monad would show their presence after sixty, or even seventy hours. Proving that the lighter the germ the longer it lingers in the air.

But in no case did a vessel show the presence of any monad after five days; that is, when the air was moteless, and free from the diffused germs. The inference is irresistible. The germs of minute and widely diffused septic organisms may be present, and be proved to be present, in the air; and the presence or absence of these—the proper conditions being provided—is a determining cause of the presence or absence of the developed organisms themselves. This is *proved* with organisms which we know arise *in* germs; that is to say, the monads. It is hypothecated in relation to organisms whose life-history we do not perfectly know, viz. the Bacteria; because when dealt with in a similar way they display precisely similar phenomena.

To me, then, what we hypothecate is sustained almost to absolute certainty by what we *know.*

The motes which determine the monads contain monad germs.

The motes which determine the Bacteria are Bacteria germs.

[W. H. D.]

GENERAL MONTHLY MEETING.

Monday, May 7, 1877.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:

Adm. Sir Henry John Codrington, K.C.B.
Warren De la Rue, Esq. D.C.L. F.R.S.
The Lord Arthur Russell, M.P.
George Busk, Esq. F.R.S. Treasurer.
William Spottiswoode, Esq. LL.D. F.R.S. Secretary.

Mrs. Julie Farmer,
Miss Catherine Alethé Fry, A.A.
Charles James Lacy, jun. Esq.
Wm. Alexander Mackinnon, Esq.
Major-General Sir Thomas T. Pears, K.C.B.
Frederick Ricardo, Esq.
Arthur Williams, Esq.

were *elected* Members of the Royal Institution.

John Tyndall, Esq. D.C.L. LL.D. F.R.S.
was re-elected Professor of Natural Philosophy.

The Managers reported that on April 9th, they appointed **James Dewar**, Esq. M.A. F.R.S.E. Jacksonian Professor of Natural Philosophy, Cambridge, to be Fullerian Professor of Chemistry for three years.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Rome—Atti, Serie III. Transunti, Vol. I. Fasc. 4. 4to. 1877.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVII. No. 5. 8vo. 1877.
Author—Ambition's Dream. New Edition. 16to. 1877.
British Architects, Royal Institute of—Sessional Papers, 1876–7, Nos. 8, 9. 4to.
Carrière, D.—Journal de l'Analyse. 4to. 1876.
Chemical Society—Journal for March, April, 1877. 8vo.

Editors—American Journal of Science for April, 1877. 8vo.
Athenæum for April, 1877. 4to.
Chemical News for April, 1877. 4to.
Engineer for April, 1877. fol.
Horological Journal for April, 1877. 8vo.
Journal for Applied Science for April, 1877. fol.
Nature for April, 1877. 4to.
Nautical Magazine for April, 1877. 8vo.
Pharmaceutical Journal for April, 1877. 8vo.
Telegraphic Journal for April, 1877. 8vo.
Franklin Institute—Journal, No. 616. 8vo. 1877.
Klaassen, H. M. Esq. (the Translator)—Dr. Daniel Schenkel: Sketch of the Character of Jesus. 8vo. 1869.
Manchester Geological Society—Transactions, Vol. XIV. Part 8. 8vo. 1876-7.
Mechanical Engineers' Institution—Proceedings, Jan. 1877. 8vo.
Meteorological Office—Quarterly Weather Charts, 1874. Part 4. 4to. 1877.
Photographic Society—Journal, New Series, Vol. I. No. 7. 8vo. 1877.
Royal Society of London—Philosophical Transactions, Vol. CLXVI. Part 2. 4to. 1877.
Statistical Society—Journal, Vol. XL. Part 1. 8vo. 1877.
Société Hollandaise des Sciences—Archives Néerlandaises: Tome XI. Liv. 4, 5: Tome XII. Liv. 1. 8vo. 1876-7.
Smithsonian Institution, Washington, U.S.—Annual Report for 1875. 8vo. 1876.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, April, 1877. 8vo.
United Service Institution, Royal—Journal, No. 89. 8vo. 1877.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, März, 1877.
Washington Observatory, U.S.—Professor S. Newcomb: On Corrections to Hansen's Tables of the Moon. 4to. 1876.
Professor Eastman: On Difference of Longitude between Washington and Ogden, Utah.
Wild, H. (the Director)—Annalen des Physicalischen Central Observatoriums Russia, Jahrgang 1875. 4to. 1876.
Zoological Society—Transactions, Vol. IX. Part 11. 4to. 1877.
Proceedings, 1876. Part 4. 8vo. 1877.

WEEKLY EVENING MEETING,

Friday, May 11, 1877.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

D. Mackenzie Wallace, Esq. M.A.
MEMBER OF THE IMPERIAL RUSSIAN GEOGRAPHICAL SOCIETY.

Secret Societies in Russia.

Mr. Wallace began with a brief sketch of Russian history down to the reign of Peter the Great, when the political system was entirely changed, and the rulers began to aim at the civilization and moral reformation of their subjects, by a bureaucratic centralized system. The arbitrary Paul, assassinated in 1801, was succeeded by Alexander I., educated by a Swiss philosopher, and deeply imbued with French revolutionary doctrines, which he proposed to put into practice by establishing a pure federal republic, with virtuous, happy citizens, and retiring into private life. He soon discovered his mistake; he saw insubordination and corruption everywhere; he lost his faith in Liberalism, fell a victim to religious melancholy, and became a devoted adherent of Metternich. The effect upon the young noblesse was different. It led to a passionate desire for reform, and the construction of secret societies to obtain it.

The first of these Associations was the "Union of Salvation," in 1816, chiefly composed of officers of the guards. It was reorganized in 1818, as the "Union for Public Welfare," and professed to help the government in suppressing official malpractices. But as the Emperor became more reactionary, a new society was formed, with the object of annihilating the Imperial family and constituting a federal republic. At the death of Alexander, in 1825, the attempted military insurrection failed. Five officers were hanged, and above a hundred transported to Siberia.

During the reign of Nicholas we hear of no secret societies, but they began to reappear through the depression caused by the Crimean War, and the present reign in some respects resembles that of Alexander I. It opened with a great enthusiasm for reform, and the emancipation of the serfs took place in 1863. But the Polish insurrection produced strong reactionary measures, and secret societies sprung up, of a very different type to their predecessors, proposing the abolition of religion, marriage, and private property, and the substitution of communism for the government. The fundamental principle of the latest of these societies is absolute equality and mutual responsibility, with much self-negation. Its officers succeed in rotation, part of them being educated and part uneducated. There is an active propaganda, by means of conversation, reading, excitement of discontent, publication and circulation of books and tracts, and the establishment of libraries and funds. Agitation is promoted to terrify the government and the privileged classes, and to raise the spirit of the people.

In conclusion, the speaker expressed his opinion that the extreme devotion of the mass of the nation to the Czar will prevent these societies having any more success than Fenianism had in Great Britain.

WEEKLY EVENING MEETING,

Friday, May 18, 1877.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Lieut.-Gen. Richard Strachey, R.E. F.R.S.

Physical Causes of Indian Famines.

Life, whatever shape it takes, is passed in a continued struggle between the forces that tend to preserve and to destroy it. Among the most active of these forces are the conditions of local climate, and notably those of atmospheric heat and moisture. Speaking broadly, the same conditions stimulate at once the conservative and destructive tendencies by adding to their intensity; so that where the productive powers of nature are the greatest, there also the opposite influences are commonly the most active. Thus, in the warmer parts of the earth, where the gifts of nature are most profuse, we have to encounter in their extreme form the devastating forces of tempest, drought, flood, and disease; a few weeks, or may be hours, sweep away the results which it has taken years of the labour of man, or the more silent industry of nature to accumulate. The evidence of the truth of this is unhappily too close at hand. You will find it in the drought which is now laying the iron hand of famine on a large part of southern and western India; in the similar calamity that fell on parts of Bengal three years ago; in the sudden disaster which within the last few months destroyed perhaps 100,000 human beings in two or three hours in the delta of the Ganges by a flood caused by a cyclone; in the ravages of cholera, which sweeps away the survivors of famine or flood; and in the long series of past occurrences of a similar character recorded in the history of India.

The conclusion is painfully forced upon us that practical civilization has gone a very little way when we find that hardly more than a first step has yet been taken in the successful application of our knowledge and material resources to warding off calamities such as these. It is not, however, my intention to dwell on this aspect of the subject, serious though it be. My present task is a much more limited one, that of placing before you, to the best of my ability, an explanation of the physical conditions under which one class of these scourges of India arises, namely seasons of drought.

Before I enter on the more special matters with which I shall have to deal, I think it desirable to draw attention to certain points bearing on the general subject of Indian droughts and famines, and their consequences, on which it seems to me that erroneous impressions are prevalent.

In the first place, great though the calamity of an Indian famine be, both to the population directly affected by it, and to the general community that has to bear the loss it causes, yet its gravity may easily be exaggerated. There has been not a little declamation to the effect that the frequent recurrence of Indian famines renders the permanent improvement and well-being of the country almost hopeless. Let us compare India in this respect with our own country, the prosperity of which cannot be questioned. The two last and severest Indian famines of recent years, that of Bengal, and that under which a large tract now suffers, may be reckoned to cost the State in all six or seven millions sterling each. Of this sum, in each case, perhaps one and a half or two millions may represent loss of revenue, and the remaining four and a half or five millions the cost of relief. Now the ordinary yearly cost of Poor Law relief in England is about seven millions sterling, while a few years ago it was eight millions. Again, at the present time in India the population in receipt of relief in the British districts is estimated at about one and a quarter millions in all. The number of persons who receive relief in England at the present time may be reckoned at 700,000 for every day of the year, permanently, and a few years ago it was as much as one million.

Further, it is stated, on good authority, that of the thirty-three millions of inhabitants of the United Kingdom, eighteen millions are fed by the produce of our islands, and fifteen millions by imported food. It is not possible to form any opinion as to the proportion of food produced locally and that imported for an Indian province suffering from famine; but of the country as a whole it may be said with certainty, that the food supply easily provides for the entire population under all known circumstances, and that in the late severe Bengal famine the export of grain continued without very great diminution in spite of the local failure of crops.

Much misconception also exists as to the density of the population of India. It is no doubt true that in some districts the population is very great, but this is by no means universal. In the provinces now suffering, the population in the worst districts is only 150 to the square mile, and the highest average does not come up to 250 to the square mile; the average of the British Isles being about 260 per square mile. In the Bengal famine the density of population was much greater; so that it is approximately true, that now the total population of the suffering districts is nearly equal to that of the Bengal districts that suffered, namely, thirty to thirty-three millions; though the area of the latter 75,000 square miles was only half of that now affected, 150,000 square miles. There is no sort of positive evidence that the population of any part of India has rapidly increased of late years. On this subject we have no facts at all on which to rely, and the opinions frequently expressed on it are necessarily based on surmise, and are in my own judgment more likely to be wrong than right. The first and only census in the province of Bengal was not taken till 1872, and it will show the utter want of value of guesses on such subjects, when I mention that the actual enumeration showed that the population was sixty-seven millions, where previous estimates had made it forty-two millions.

I shall now proceed to the main subject of my address.

Let me ask your attention to the map of India before you. On the north, you see the mountainous region of Tibet, the southern border of which is formed by the ranges known as the Himalaya, extending over a length of about 2000 miles, and rising along the whole line into the region of perpetual snow; few of the passes being at a less altitude than 15,000 or 16,000 feet, peaks of 20,000 feet being abundant, and the highest summit yet measured reaching 29,000 feet above the sea level. On the west, this elevated region is connected with the table-land of Afghanistan and Baluchistan, the border of which forms the frontier of India along the Indus. On the east, high mountains spread out from Tibet southwards, constituting the western parts of China, and extending to the sea in Burmah, the Malay peninsula, and Cochin-China; the eastern frontier of British India lies along the outer ramifications of these ranges from Assam, by the delta of the Ganges and Brahmaputra, to the districts of Arracan and Burmah.

The northern provinces of British India occupy a great plain which flanks the Himalaya along its entire length, expanding at both extremities to the sea; on the east over the delta of the Ganges, which is roughly coincident with the province of Bengal, and on the west through the Punjab and Sindh, along the course of the Indus and its great tributaries to the Arabian sea. This plain rises at its highest point between the Sutlej and the Jumna to something less than 1000 feet above the sea, presenting over its whole extent an almost unbroken surface, which has the appearance of perfect horizontality.

What is commonly spoken of as the Indian peninsula is occupied by a table-land, roughly triangular in shape, having its broadest end immersed, so to speak, in the continent, to the extent of about one-third of the distance from its base to its apex, the immersed portion being surrounded by the great northern plain. The southern half of the western flank of this table-land forms the line of mountains called the Western Ghats, and the northern half the hills that bound Rajpootana on the west, ending at Delhi. The highest points on the Western Ghats proper hardly exceed 4000 feet in elevation, though the Nilgheri mountains, near the southern end of the peninsula, rise to 8000 feet.

The eastern margin of the table-land is less sharply defined than the western, and is less in elevation also, having a varying extent of low-lying land between it and the sea. The northern border is still less strongly developed; it gradually declines in the north-west, where it can hardly be distinguished, the table-land merging into the plain. The average altitude of the plateau is probably about 1500 feet above the sea, being most in the south, where it rises in some places to 3000 feet, and generally greater on the west than on the east, so that with two remarkable exceptions, all the more important rivers that carry off the drainage, flow off to the eastward.

These exceptions are due to the occurrence of a line of geological disturbance or discontinuity, which crosses the table-land from south-west to north-east, being marked by the valleys of the Nerbudda river on the west, the first of the exceptional rivers just referred to, and of the Sone on the east, and the lines of elevation that flank them, known as the Vindhya, Satpúra, and Kaimor ranges. The Taptee, the second of the exceptional rivers flowing westward, occupies a valley, nearly parallel to, and not far removed from that of the Nerbudda, and no doubt having its origin in the same physical causes.

The table-land of the peninsula, strictly speaking, should be held to terminate at the Nilgheri mountains, immediately south of which a complete interruption of the Western Ghats takes place. The mountains which extend from this point to the apex of the peninsula at Cape Comorin, rise to a considerable elevation at some places, but have no longer any of the characters of a table-land.

The part of India which is at the present time suffering from the effects of drought, includes nearly all that which lies on the summit of the table-land, from a line 100 miles south of the Taptee river to the Nilgheris, along a band 200 miles wide, measuring from the Western Ghats eastward, as well as the low-lying districts on the east coast between the sea and the foot of the eastern slope of table-land south of the Kistna river. The British area affected is about 150,000 square miles, with a population of more than thirty millions. You may compare with this the area and population of Spain, which has 200,000 square miles and seventeen millions of inhabitants; and Italy, with 100,000 square miles and twenty-four millions of inhabitants; you can thus judge of the vast magnitude of the task before the Government in dealing with a scarcity spread over such an extent of country.

These same regions are always liable to suffer from deficient rainfall, and the low-lying coast districts not now affected, north of the Kistna, have also on former occasions suffered in an extreme degree. The Bengal drought of 1873 extended over the part of the great northern plain immediately contiguous to the Gangetic delta, and the western border of the delta. It was very exceptional so far as concerns the portion of the area affected that lay north of the Ganges and in the delta. The portions of the great plain lying south of the Ganges, along the northern border of the table-land, from the junction of the Sone with the Ganges, as far as the Sutlej, have at intervals suffered from severe drought, proximity to the Himalaya being, as a rule, associated with more favourable rainfall.

The part of the table-land north of the Nerbudda embracing Rajpootana has suffered very severely within recent times.

The first point to consider in relation to the matter before us, will naturally be the ordinary distribution of the rain. This is shown on the map, on which have been drawn lines indicating approximately the average amount of rain over the whole country.

With some exceptions, to which special attention will be necessary, the chief fall of rain in India takes place during the period of the south-west monsoon, that is from May to October, the remainder of the year being comparatively dry. These summer rains prevail over the whole of the coasts of southern Asia which are under the influence of the south-west winds, namely, from Arabia to China; and they are felt in a more or less marked manner to great distances inland, being abundant over India generally, and along the whole of the southern slope of the Himalaya from the Brahmaputra to the Indus.

The rain at this season is very heavy along the coast of the Malay peninsula, through Burmah to Assam and the Himalaya beyond. On the north its progress is in a great degree arrested by the snowy mountains, so that it only just reaches in the slightest manner the southern borders of Tibet. In the north-west quarter these rains gradually cease in eastern Afghanistan, the city of Kabul being beyond their influence, and generally they do not extend in an important degree beyond the outer ranges of hills that flank the lower course of the Indus.

On the west, or Malabar coast of the peninsula, the summer rainfall is extremely high. On the east, or Coromandel coast, the quantity at this season is, however, comparatively small, the principal fall there taking place in October and November after the south-west monsoon has ceased. To this apparent anomaly I shall again revert.

Besides the principal season of summer rain, there is clearly developed in the north of India a distinct season of winter rain, which has its maximum nearly corresponding with the period of greatest cold, in January and February. This rain becomes less important as we move southward, being hardly noticeable in Bombay or Calcutta, or farther south; while in Afghanistan, where it is prolonged into the spring, it is the heaviest and most valuable of the whole year.

I need perhaps hardly remark that the source from which the summer rains of India are supplied is the continued stream of air highly charged with vapour, which is poured over the land from the Indian Ocean by the winds that blow during the season, known as the south-west monsoon. To apprehend correctly how these winds thus operate, some explanation of their cause and mode of occurrence will be requisite.

It is a common, but erroneous mode of stating the efficient cause of wind to refer it to difference of temperature of two contiguous areas; namely, that cold air being more dense and heavier, necessarily displaces hot air, which is lighter, the accompanying motion being wind. Indirectly, no doubt, the difference of temperature often operates in this manner. But the true cause of all movements of the atmosphere which we describe as wind is wholly mechanical, being difference of pressure at neighbouring places, and the facts cannot be properly dealt with if this is lost sight of.

The upper strata of the atmosphere press by their weight on those at the earth's surface, and the effect is indicated by help of the barometer, which measures the weight or pressure of the air above us at any moment in inches of mercury; 30 inches of mercury over any surface being nearly equal in weight, or producing equal pressure, to that caused by the whole atmosphere over the same surface. The pressure of course diminishes as we ascend, there being less and less air above us, and the barometer falls. Moreover, when the air is at rest an equal weight or pressure will be found at all places equally distant from the earth's surface; or in other words, the planes of equal pressure will be truly horizontal. If, at any time, this is not the case, a result follows exactly like that which takes place in a body of water the surface of which is not horizontal; namely, the air flows from the higher to the lower points, which movement continues till the planes of equal pressure are restored to horizontality.

Changes of temperature over any area by causing the expansion or contraction of the superincumbent body of air disturb the levels of the planes of equal pressure in the upper strata of the atmosphere, heat causing them to rise and to separate from one another, and cold bringing them down and closer together; by reason of which the air flows off on all sides, above, *from* a relatively heated area, and flows in on all sides, above, *to* a relatively cooled one. The immediate consequence of such movements in the upper strata will be to reduce the pressure of the surface over the hot area from which air has been thrown off, and to increase it over the cold one towards which air has flowed in, and this will be accompanied by a wind at the surface, blowing *from* the cold area *to* the hot one. The movements in the upper and lower parts of the atmosphere are thus the converse one of the other, and tend to restore the equilibrium or horizontality of the planes of pressure disturbed by changes of temperature. But though this is the general law which regulates such movements, the actual conditions under which the atmosphere is placed, and more particularly the influence of the rotation of the earth, which is imparted by friction to the superincumbent air, are constantly causing disturbances of what might otherwise have been comparatively simple movements, so that the resulting directions of the wind as observed are not much more readily connected with the differences of pressures than with the differences of temperatures which lead to them. In our own latitudes, for instance, the direction of the wind is commonly more nearly parallel to the lines of equal pressure at the surface than perpendicular to them, which would clearly be the case if the winds were chiefly regulated by direct differences of pressure.

It is no doubt well known to you that two well-marked seasons of periodical wind recur regularly year by year along the coasts of southern Asia and over the neighbouring seas, known as the south-west monsoon of the summer months, and the north-east monsoon of the winter months. The high summer temperature of the great arid plateaus of Asia causes a dispersion of the air over them, which is marked by a very considerable fall of the barometric pressure over the whole continent, and is accompanied by a less great but perfectly distinct increase of pressure over the adjoining ocean regions south of the equator, which then have their winter season. Under the great difference of pressure thus established the air flows in powerfully towards the continent of Asia from the surrounding seas, the movements becoming well marked in April with the rapid rise of temperature that then commences, and attaining their maximum in July, soon after the summer solstice. This is the south-west monsoon. When the temperature of Asia falls, which it does in September, as the sun goes rapidly south, the barometric pressure begins to be restored, and it becomes generally equalized over the southern parts of the continent in October, when this monsoon ceases.

The converse action takes places in the winter. The same causes which led to the great summer heat in Asia, namely, the great extent of dry and barren surface, produce excessive winter cold, which is accompanied by a well-marked increase of barometric pressure, again to be followed by the development of winds from the land towards the sea, which give rise to the north-east monsoon.

As is well known, the winds blowing northward from the equator bring air which has there acquired a high eastward velocity of rotation to places where the velocity of surface rotation is less, and therefore are felt as winds impressed with an eastward movement, and this being combined with their northward movement produces the south-west winds of the summer monsoon. The movements towards the equator in a similar manner lead to the north-east winds of the winter monsoon.

But though this supplies the general key to the great changes in the winds of southern Asia, the local succession of their directions depends on special local influences, which disguise the general law, and may even in a great measure counteract it.

The south-west monsoon begins in the Arabian sea with north-west and westerly winds, which draw round to south-west as the summer advances, and again fall back by west to north in the autumn and winter. In the Bay of Bengal the south-west monsoon blows as a southerly or south-easterly wind, being succeeded by north-easterly winds after October, which in turn give place as the winter months succeed, to winds from north and north-west.

Again the winds during the south-west monsoon do not generally blow over the land in a south-westerly direction. The current of air flowing in from the sea is gradually diverted towards the area of least pressure, and at the same time becomes dissipated and loses much of its original force as it passes from the area of greater pressure and lower temperature to that of reduced pressure and higher temperature. The irregularities of the land surface also operate powerfully in destroying the uniform character which the winds maintain while passing over the uniform surface of the ocean. The great mountain ranges along the north of India act moreover as an effectual barrier to the movements of the wind in that direction. It is from these causes that we find the winds which blow over the Arabian sea from the south-west become southerly winds as they pass up the valley of the Indus; the winds of the Bay of Bengal which have already been diverted to the south, acquire an easterly tendency as they blow over the delta of the Ganges, and pass as south-east winds along the foot of the Himalaya towards the hotter regions in the north-west.

The body of air which is thus carried by the winds of the south-west monsoon from the ocean over India necessarily comes up highly charged with watery vapour; the conditions under which this vapour is released over the land in the form of rain, I next have to explain.

You are all familiar with the facts that water when heated passes into the condition of an invisible vapour, and that vapour when cooled is condensed again into water. Watery vapour, like air, being an elastic fluid, is liable to vary in density and weight, and the greater the density the larger will be the quantity of water held in suspension. But the quantity of water that can thus be kept in the state of vapour depends on the temperature, and therefore when the temperature falls below a certain point some of the vapour is restored to the state of water, and only that part remains in the gaseous form that the particular temperature permits. When air contains as much vapour as is consistent with its temperature, it is said to be saturated with moisture; the proportion of actual vapour to the greatest possible quantity is termed the proportion of saturation.

The vapour formed by evaporation at the earth's surface is much less dense than ordinary air, and therefore constantly tends to diffuse itself upwards, and thus becomes disseminated in the atmosphere. At the same time, since the temperature of the air falls about 1° Fahr. for each 300 feet that we rise from the sea level, the operation of this law leads to such a reduction of the heat in the upper strata, as to cool the ascending vapour, and at length to condense all that is in excess of what the reduced temperature of each successive stratum admits of being retained in the gaseous form.

The supply of vapour passing upwards is therefore being constantly reduced as it ascends, and it thus happens that nine-tenths of the whole quantity of vapour in the entire atmosphere are to be found below an altitude of 20,000 feet. Where the evaporation at the surface is very copious, as it would be in a tropical sea with an air temperature for instance of 80° Fahr., if the vapour spread itself upwards according to the known laws which it obeys, we might commonly find the air near the surface to contain say four-fifths of the quantity required for saturation, and it would therefore be perfectly transparent, and no condensation would occur. But these conditions would at length lead to the quantity of vapour that reached an elevation of 4000 feet being incompatible with its existence there in a gaseous form, because the air temperature would be only 68°, while a temperature of about 70° would be necessary to admit of the vapour retaining its gaseous condition. Condensation would therefore have taken place below 4000 feet, and a stratum of clouds would have formed, which according to circumstances might either be carried away and dispersed by winds, or discharge downwards the condensed water as rain. There is thus in the atmosphere at all times and places a more or less definite tendency towards the formation of cloud and the fall of rain, by reason of the conflicting laws of the diffusion of vapour and of the distribution of temperature in the atmosphere; and in strict truth our search might rather be for the causes that intercept or interfere with this action, than for those under which it continues to operate.

There are, however, circumstances which may so greatly add to the intensity of the condensation of vapour in the upper regions of the air as to call for special notice. Where a wind charged with vapour blows over an irregular surface, as from the sea across the face of a mountain, and the whole body of moving air is thus forced upwards, the expansion that follows is accompanied by a fall of temperature analogous to that which is observed in all the higher strata of the atmosphere; and as in this case the whole body of vapour is cooled down, instead of, as in the hypothesis I before made, only the portion that rose by diffusion to the higher level, the condensation is proportionally more copious and sudden, and takes the form of heavy clouds that rest on the mountain, or of rain that falls upon its slopes.

The mixture of cold with hot currents of air highly charged with vapour is another possible cause of the condensation of rain; though I think a not frequent one in the countries to which our attention is now specially directed.

We are now in a position to consider somewhat more in detail the phenomena of the Indian periodical rainy seasons. The primary agents are the high summer temperature of the continent, and the consequent influx of a current of air from the south, blowing over a great expanse of tropical ocean, and consequently highly charged with vapour. If at first there appears something anomalous in the chief rainy season occurring at the hottest part of the year, whereas the condensation that causes rain is essentially a result of cold, we have to remember that the requisite cold is relative and not absolute, and that as the water suspended in the air is greater in proportion as the temperature is high, the quantity likely to be released by any disturbances capable of producing condensation will also be greater in a similar proportion. Moreover, the dispersion upwards, over the heated continent, of the air that flows in from the south, conduces to the increase of the quantity of vapour in the upper parts of the atmosphere, and to the maintenance of a state of unstable equilibrium in which moderate local disturbances of temperature are likely to cause condensation and produce rain.

The fall of temperature which follows the commencement of the rainy season is first, no doubt, an effect of the fall of rain, which brings to the surface of the earth the water condensed in the much cooler higher strata; but the loss of heat is continued afterwards, in consequence of the sun's movement southward, and thus the disposition to condensation is maintained as long as the southerly winds blow. As the change from the southerly to the northerly monsoon is concurrent with the rapid fall of temperature which begins in northern India about the end of September, the close of the rainy season, like its commencement, is commonly marked by heavy falls of rain; the one caused by the first overthrow of the equilibrium of the vapour unstably suspended in the highly heated air at the solstice, and the other by the direct loss of heat which is experienced after the equinox.

The summer rains make their first appearance on the southern parts of the west coast in May, where the southerly winds have then become established. The region of greatest heat is now to be found at the extremity of the peninsula, the winds at Bombay not yet blowing from a point south of west. By the middle of June, as the area of greatest heat has advanced northward, and the winds have drawn well to the southward in the Indian seas, the rains begin to fall at Bombay, about which time also they become established at Calcutta.

The fall on the west coast is very abundant. The scarp of the table-land, or Western Ghats, which forms an almost continuous obstacle in the path of the south-west winds, gives rise to great condensation, from causes already explained, the result being excessively heavy falls of rain along the face of the mountains exposed to the sea, and on the country between them and the sea, on the more exposed parts of the former the quantity measured reaching more than 250 inches, and on the latter from 100 to 120 inches. To leeward of this line of elevation the rain very rapidly diminishes, so that the quantity measured at Poona, 60 miles from the coast, is only 25 inches. Manifestly, after any great quantity of rain has been once condensed from the south-west winds, there remains relatively little to supply the districts to leeward; and if, as is here the case, the obstructing range of heights is considerably more elevated than the surface of the table-land, probability of further condensation after the wind passes that range is much reduced. The ridge, however, in fact, is a good deal broken in its outline, and openings occur at intervals through which more or less vapour passes, and aids in supplying rain to the table-land.

The gradual extinction of the very heavy rainfall of the Malabar coast as we approach Cape Comorin, near which we have less than 30 inches, and the corresponding diminution at the northern extremity of the coast, are readily explained by the smaller amount of disturbance caused by the more broken character of the mountain ranges, and by the air being able to pass round the end of the barrier instead of being forced over it. The fact that during this season the rain only extends 40 or 50 miles from the coast out to sea plainly depends on a similar cause.

Following the coast westward, we find that though the south-west winds blow with great force and regularity over the coast, at the mouth of the Indus and in Sindh the fall of rain is very small and irregular. For here this wind meets with no such obstruction as that of the line of Western Ghats. On the contrary, the air passes from a comparatively cool sea surface, over a very hot surface of low land, and any tendency to condensation that might be caused by the slight rise over the land, is more than compensated by the increased temperature.

The current, therefore, passes on as a southerly wind, carrying with it the uncondensed vapour over Sindh, to be at length precipitated on the outer ranges of the Himalaya in the Punjab, or on the mountains of eastern Afghanistan. There is no room to doubt, that had a range of mountains, such as the Western Ghats, connected the high land of the Indian peninsula with that of Baluchistan, the whole of the Punjab must have been, what the country that lies between it and the sea is, almost entirely deprived of rain.

The importance of the great valleys of the Taptee and Nerbudda, in connection with the rainfall of central India, will now be apparent. Up these openings the south-west winds pour their vapour-bearing streams, and furnish to a large area in the heart of the peninsula the rain which is precipitated by the atmospheric disturbances that occur around the high lands in the centre of the table-land. This diversion of the vapour-bearing currents aids, no doubt, in producing the diminished fall of rain observed in the northern coast districts around Guzerat, as compared to those near Bombay and farther south, and serves to explain the increased fall of between 40 and 50 inches, in central India, as compared to that, between 20 and 30 inches, in the Deccan.

The south-west winds of the west coast do not, properly speaking, extend across the peninsula. On the Madras coast, during the south-west monsoon, the land and sea breezes continue to be strongly marked, whereas on the west coast they disappear, and the rainfall between May and September over the southern and eastern portion of the peninsula is relatively small.

On the opposite coast of the Bay of Bengal, and along the Malay peninsula, excessively heavy rain is the rule during this season, the quantity being from 100 to 200 inches. Here, too, a continuous line of mountain follows the coast facing the prevailing winds. On reaching the straits of Malacca, and passing under the lee of the island of Sumatra, the rain diminishes greatly; and at Singapoor, at the point of the peninsula, the months between April and September are decidedly the least rainy half of the year, the largest fall taking place in the north-east monsoon, when the winds reach Singapoor blowing directly from over the sea.

Phenomena exactly similar to those observed on the Western Ghats, occur on the mountains east of Bengal. At Chira Púnji, on the Khasiya hills, which rise abruptly to about 4000 or 5000 feet over the delta of the Ganges, the rainfall is believed to exceed that known at any other place on the earth, more than 600 inches being a not unusual annual amount.

The difference between Bengal, with a rainfall of from 60 to 70 inches, and Sindh, with hardly any, is very remarkable. It is sufficiently explained by the distribution of the high land that is contiguous to the former, and, indeed, almost surrounds it. The great mountains on the north communicate with the ranges on the east which separate Bengal from the upper parts of Burmah, and form a serious obstacle to atmospheric movements in that direction. These ranges are all well clothed with forest, and the temperature of the whole area over which they extend must be considerably lower than that of the country to the west of them. This is accompanied by a higher barometrical pressure, which leads to the development of easterly winds during the summer months blowing from eastern Bengal to the far hotter regions of north-western India, where the barometric pressure is least. From these causes, the rains begin much earlier in Assam and its neighbourhood than in any other part of India, viz. in April, soon after the southerly winds are established at the head of the Bay of Bengal; and a tendency towards such early rains is discernible in all the Bengal registers. Thus the conditions are very different from those of Sindh, the free onward progress of the winds being arrested in Bengal by the current set up towards the north-west, and the influx of relatively cool air from the east preventing any tendency to a rise of temperature in the air coming up from the Bay of Bengal as it passes over the land, such as would stand in the way of local condensation.

The rainy season rapidly develops itself from Bengal towards the north-west, the fall gradually decreasing in amount as the rain-bearing winds pass on; a fact readily explained by the consideration, that as the condensation of the vapour goes on, less remains to supply the more distant localities.

Concurrently with the reduction of the rainfall in passing from east to west in northern India, we find an increase in the quantity as we approach the Himalaya, and a diminution as we recede from the range, the gradation being distinctly marked from a distance of 150 or 200 miles to the foot of the mountains. On the outer slopes of the Himalaya the fall is very greatly increased, but it rapidly diminishes again in amount as we penetrate among the mountains.

These results are brought about in a manner that deserves particular attention. At all times of the year winds blow up the valleys of the Himalaya towards the highest parts of the chain, and down them at night; the day winds having their greatest force at the high passes into Tibet, and the night winds at the debouches of the great rivers into the plains. Such winds are well known to be characteristic of all mountain ranges. They are no doubt due to the disturbance of the planes of atmospheric equilibrium, caused by the alternations of temperature in the air over the mountains and low lands. A column of air over the low land, measured from any horizontal plane of equilibrium, being longer, will expand more and contract more; one over the mountain, being shorter, will expand and contract less. As the day advances, and the heat increases, the planes of equal pressure will all rise over the low land, and the air will flow towards the axis of the elevation; as the night comes on, and the temperature falls, the greater contraction of the longer columns over the low land will bend the planes of equal pressure from the axis of elevation outwards, and the air will move in that direction.

Thus a system of aspiration is established over the mountains, by which the vapour-charged air that comes up from the sea is drawn up from the plains along the valleys and over the outer ranges, and is so brought under the operation of that sort of action which we have already seen to be so efficacious in producing condensation. These movements serve also to explain what seems peculiar in the gradation of the rainfall within and without the mountains. It is, I think, also to the same cause that we may trace the circumstance, that the first heavy falls of rain in upper India take place on the mountains, and that the disturbances are thence developed and extended which lead to a general fall over the plains.

In northern India the summer rains usually cease before the end of September; in Bengal and along the Arracan coast they are prolonged into October. In western and central India also a little rain falls in October. Over all this area northerly winds are commonly established in October, and the rain that falls at this time must be regarded as the residual condensation under the increasing cold of autumn of the vapour previously brought up by the southerly winds.

With the fall of temperature in September, the region of least pressure is rapidly transferred to the south, and the northerly winds begin, first in the north and gradually extend southward; though the southerly winds still continue to blow at the end of the peninsula, till November. These changes lead to the establishment of a current of air from the Bay of Bengal, blowing as a north-easterly wind towards the Madras coast, along which there is, in consequence, set up an autumn season of heavy rain lasting from October to December, quite analogous in its efficient causes to the summer rainy season of the western coast. This rain usually extends from about the great bend in the east coast to Ceylon, though its influence is at times felt as far north as Cuttack; and it is of essential importance to all the eastern border of southern India, which, as was before explained, receives but a scanty supply during the south-west monsoon.

It may here be noticed, that the cyclones that originate in the Bay of Bengal in October and November appear to be results of atmospheric movements developed over an area of low pressure that remains in the bay, after a high pressure area has begun to form rapidly over eastern Bengal, with the fall of temperature accompanying the close of the year; and that the tendency to the formation of cyclonic movements is to be seen in the north-easterly winds that usually prevail in the north and west of the bay, while south-westerly winds are still blowing in the southern and eastern parts.

The map which is before you resumes the facts into which I have been entering in some detail. It shows that the part of India east of the 80th meridian has an average yearly rainfall for the most part exceeding 40 inches, and that, excepting the country between the Western Ghats and the sea, the portion west of that meridian has a smaller rainfall. Further, we see that the quantity is extremely small all over Sindh, and that the tract in which the fall is less than 30 inches includes all the Punjab excepting the mountain districts, a considerable part of the North-Western provinces extending to half way between Agra and Allahabad, a large part of Rajpootana, and Kattywar. Again, we observe a large area in the peninsula, occupying nearly the whole of the Deccan and Mysore, on which also the rainfall is less than 30 inches.

Of the area in which the rainfall is below 15 or 20 inches, it may be said in general terms that agriculture is not there possible otherwise than with artificial irrigation; and thus it has happened, that the population of the districts where the rain is of all others least abundant, have made themselves in a very great degree independent of the local rainfall. On the other hand, it may also be said that where the average rainfall exceeds 40 or 50 inches, the occurrence of such drought as will cause serious scarcity is rare, though when it does occur it may be very severe. It was in a portion of this area that the Bengal famine of 1873 took place, and also that of Orissa in 1866.

The region, the average rainfall of which is between 25 and 35 inches, is probably that which suffers most from droughts. Here, although on the average the supply of rain is sufficient to support an agricultural population, the fluctuations which reduce the fall below what is essential are so frequent, as to lead to repeated seasons of scarcity of greater or less severity. The north-western part of the North-West provinces, the north-western part of Rajpootana, and the Deccan, with a small part of the Madras districts at the end of the peninsula, fall within this category. The present scarcity affects the whole of the dry region of the peninsula. The drought of 1837–38 was extremely severe in the North-West provinces, as also was that of 1860. In Rajpootana, a very severe scarcity, followed by great destruction of life, occurred in 1870.

For reasons already suggested, the results of drought have a tendency to be more fatal where the average rainfall is abundant, than where it is scanty. In the latter case, the population is less likely to be dense, and better able by its habits to go in search of subsistence elsewhere, and by its sparseness to succeed in procuring it. A more dense population will be less easily provided for; and in proportion as experience of drought is small, skill in devising means of resisting its effects will be small also, when once present resources are exhausted. Drought also will be more or less fatal, according as it follows a season of bad or good rainfall; and it has commonly happened that severe famines have been caused by a severe drought following one or two years of indifferent rain. The stocks of food become exhausted, and the cattle and the population enfeebled, and thus less able to resist the pressure put on them.

In a country where there is no pasture, like India, the feeding of the agricultural cattle is always a difficulty, and their condition is commonly extremely poor, viewed in relation to the standard of temperate countries. The effect of drought in the destruction of the cattle is one of its surest and most pernicious evil consequences.

It will be apparent that the importance of the fall of rain in India, for general purposes of agriculture, will be determined by the requirements of the principal crops, and particularly on those crops on which the food supply of the people depends.

The people of India subsist for the most part on vegetable food, cereal grains, pulses, and vegetables, with milk and butter. Other animal food, excepting on the coasts where fish is procurable, is comparatively little eaten, even among the Mussulmans, who form about one-tenth of the population. The grain crops, therefore, are of unusually great importance. The grains most commonly consumed in three-fourths of India are millets, called, in the Hindee dialects, jowar and bajra. Rice is the ordinary food of the people of those regions only where the conditions of climate are suitable for the abundant production of this grain. This would include Bengal, the coast and southern districts of Madras, and the western districts of Bombay, as well as British Burmah. Contrary to what is commonly thought to be the case, the rice-eating population of India is altogether in a minority.

These food grains, and some others which I need not mention specially, are plants suited to tropical conditions of climate, and they are all raised in the summer or hot half of the year, the crop of which season is known in all parts of India as the khureef. There is, however, a second crop, more particularly characteristic of the parts of India which have the coldest winter, which is on the ground during the winter months, and is called rubbee.

The khureef is usually sown as soon as the rainfall admits of the ploughing of the land, and it is reaped in September or October. The rice crop varies somewhat in its time of sowing and of ripening, according to locality and variety. The chief harvest in Bengal is in December; on the Madras coast, farther south, the later rains lead to a still later harvest, where there is but one; but the more tropical climate of the south admits an almost indefinite succession of crops, wherever an artificial water supply is available to raise them.

It need perhaps hardly be said, that most varieties of rice require a large quantity of water to raise them; and aid in some shape or other is given by artificial means in most parts of India to the natural rain supply, to ensure the safety of this crop. Particularly in the south of India, multitudes of reservoirs have been constructed with this object, in which as large a supply of rain-water is collected as is practicable, to supplement the direct fall. Larger irrigation works have been carried out by the British Government, to divert, with a similar object, the waters of most of the rivers that discharge themselves on the east coast.

The grains called jowar and bajra, are commonly sown on the higher lands, and left to depend entirely on the natural rainfall. Their requirements in the way of water are very much less than those of rice, and it is very unusual to supply them with artificial irrigation.

The rubbee crop is usually sown in October, and ripens in March or April. In the north of India it consists chiefly of wheat and barley, which grains, however, hardly enter into the ordinary food of the agricultural population, being reserved for the better-to-do classes and town population. Certain pulses that are largely used for food also are raised at this season. Excepting on the high lands of central India and the Deccan, wheat does not thrive much south of the tropic; and in the southern parts of the peninsula the reduction of temperature in the winter months is hardly sufficient to develop any distinctly temperate agriculture, such as forms a very marked feature of northern India.

Great irrigation works exist also in several provinces of northern India. These probably afford more aid to the rubbee than to the khureef harvest; and their chief importance in connection with the food supply of the country is without doubt due to the security which they give to the wheat and barley harvests, though their general utility in adding to agricultural produce of all descriptions is very great.

In considering the more special requirements of agriculture in the matter of rain, and the precise manner in which a partial failure is likely to be mischievous or otherwise, the critical times have to be distinguished. First, as the great power of the tropical sun utterly dries up the soil in the hot months that precede the rainy season, the first showers are almost essential to admit of the final ploughing, and the sowing may thus be unduly late in unfavourable seasons. Where artificial irrigation is available, this delay is avoided. Next, the thorough saturation of the soil, and its maintenance in a sufficient state of moisture, are requisite for the germination of the seed, and this is always one of the most critical periods for every crop. In many cases, thoroughly favourable rain at this stage will secure a return of some sort, even in an otherwise very bad season; and a bad commencement may often be beyond remedy, however good the subsequent falls may turn out. The next critical stage is that of flowering, when a great excess of rain may be almost as fatal as a want of it, by causing the destruction of the parts of the flower essential for the development of the grain. The period immediately follows in which the grain is fertilized and takes its ultimate form, during which the supply of moisture to the plant is not less essential than in the first stage of its life, and any serious failure of rain is fatal to the harvest. Thus, even though the average fall in any year may be fully enough to carry on the ordinary processes of growth, the failure of rain even for a few days at one of the critical periods may lead to a complete loss. If I am rightly informed, the drought of 1873 in northern Bengal arose from the failure of the later rains alone. The troubles of the present time seem to be due to a general failure, both of the south-west monsoon rains on the west coast, and of those of the succeeding north-east monsoon on the east coast; a combination of evil fortune which, it will be seen, necessarily falls with the greatest weight upon the southern districts of Madras.

In attempting to give any general account of the phenomena of Indian rainfall, it has of course been necessary for me to deal with average quantities. But the departures from these averages in separate years are very great; and it is from the fluctuations that thus occur, that the areas of average small and large rainfall may be transformed, without any considerable derangement of the general sequence of phenomena that I have described, into areas of abundance or absolute drought. Thus the well-marked loops that are formed by lines of equal rainfall having their points directed to the eastward, and following generally the line of the Jumna and Ganges from the Punjab to Bengal, indicate the probability, which is supported by actual experience, of areas of drought being formed along this axial line. I apprehend that the Bengal drought of 1873, which affected, though with less intensity, the adjacent districts of the north-west provinces along the Ganges, may be regarded as due to the local exaggeration of the general causes that lead to the peculiar inflexion of the lines of rainfall to which I have alluded.

The great extent of the fluctuations of the rainfall will be shown by the following figures. The Madras average for sixty-four years is 48·5 inches, the greatest excess over the average being 39·9 inches, and the greatest defect below it 30·1 inches. At Calcutta, for forty-seven years, the average is 65·8 inches, with a maximum excess of 27·5 inches, and a defect of 22·2 inches. At Bombay, for fifty-two years, the average between May and October being 76·9 inches, the maximum excess was 42·0 inches, and the defect 41·8 inches. At the three places, the average deviation of a single year from the mean of all is found to be, for Madras, 12·4 inches, for Calcutta, 9·0 inches, and for Bombay, 13·4 inches.

No precise physical connection has hitherto been established between the local rainfall at any place in India, and the temperature or pressure of the surrounding area; and no certain step has yet been made, so far as I know, towards foretelling the character of the seasons. In these respects, however, our knowledge is not less as regards India, than other countries.

An opinion has, indeed, been quite recently published by Dr. Hunter, to the effect that the rainfall registers at Madras, which extend over sixty-four years, supply evidence of a connection between the quantity of rain and the sun-spot cycles of eleven years or thereabouts. This idea is not novel, having been advanced some years ago by Mr. Meldrum and others, on the alleged basis of facts collected from many different parts of the globe.

So far as Dr. Hunter's views are concerned, I have no hesitation in stating my own conviction, that the facts on which he relies do not support his conclusions. He has inferred, from what must be held to be altogether insufficient numerical data, that sure indications of periodicity exist. He arrives, by an arithmetical process, at certain figures, which he regards as the probable mean amount of rainfall in the successive years of the eleven-year cycle, and finding a maximum and minimum among them, he infers that this is a proof of a true periodical variation. But such a result alone proves nothing. To test its value it is necessary to compare the calculated quantities of rain for the several years with the quantities actually observed, and then to consider whether the differences are of a character to justify the belief that the calculated quantities afford a reliable approximation to the truth, and what sort of approximation. Dr. Hunter does not seem to have been aware of the necessity for exercising this caution, though the extreme variation of the rainfall from year to year, to which reference has already been made, would appear to have been likely to suggest it. The only conclusion that seems possible, from such an examination of the figures as I have described, is the negative one, that they cannot be accepted as supplying any evidence in support of the views put forward by Dr. Hunter.

Though this argument is mainly negative, and goes rather to discredit the alleged proof of Dr. Hunter's conclusion, than the conclusion itself, yet much doubt appears to me to be thrown on the probability of any such direct connection between the rainfall at Madras and the sun-spot period as has been spoken of, by a comparison of the Madras observations with those made during the same period at Calcutta and Bombay. It is extremely difficult to conceive, that if such a connection existed at Madras it should not be apparent at the other two places; yet the same treatment applied to the Calcutta and Bombay figures as that adopted for Madras, shows no correspondence in the results. Neither can any persistent relation be seen to exist between the quantities of rainfall at the three places. There is an occasional likeness at one time between one pair, at another between a second, and again between the third, but no uniformity. And this is what might have been expected, from what we know of the general manner in which precipitations of rain take place, and the oscillations of wet and dry weather occur.

On the whole, our knowledge of the immediate physical causes of rainfall is very rudimentary; and though there be no present appearance of success in solving the intricate problems that an inquiry into those causes must involve, it can only be by help of a careful examination in detail of all the facts that it can become possible at all. Such a collection of facts has at length been seriously commenced; but what I have already said will indicate the great complexity of this subject, and the many difficulties that will have to be encountered in grasping it in a satisfactory way.

Such being the general position of India in respect to the rainfall necessary for its agriculture and its food supply, and man having no possible means of exercising any control over the atmospheric changes which are effective in adding to or reducing the quantity of rain, and having no present power of foreseeing these changes, it may be asked whether we therefore have no hope of escaping from the terrible consequences of drought. In my opinion there can be no doubt of the possibility of combating successfully its worst results by the progress of civilization; by which I mean, that improved social condition in which the accumulated knowledge and material resources of a community are applied in the most effectual way to meet its requirements. The result at which we aim in India can be brought about by this, and this alone; as this, and this alone, has been able to bring it about in other countries. But as such progress is necessarily slow, we must be content for the present to pass through a condition of periodical suffering of an acute kind, during which the bitter lessons of experience will continue to urge improvement; and thereby intelligence will be stimulated, and ways of escape from these evils will be gradually perfected. These ways of escape are indeed already sufficiently evident, and so far as they have been hitherto applied, have been found to be thoroughly efficacious. They are the provision of artificial irrigation, and of improved means of transport; the first, to give a certain supply of water for the purposes of agriculture, increasing production generally, and supplementing the rainfall so as to prevent calamitous drought; and the second, to provide facilities for the economical distribution of food, and for the operations of commerce, by which wealth is increased. Simple as these remedies may appear, the material difficulties in the way of their being furnished, to the extent necessary to remove the evils now under consideration, cannot at present be surmounted. They require the application of capital, that is to say, the accumulated results of labour, which, to meet the requirements of the present case, may possibly still have to be prolonged over very many years. For India must perforce supply her own needs from her own material resources. Nowhere, nor at any time, has the benevolence of others succeeded in removing the burden imposed on every community of providing for its own existence under pain of extinction. Even if it were possible that external aid could be given on an adequate scale to supply the requisite material appliances, it is certain that the moral qualities would not have been developed, by which alone those appliances could be successfully made use of—self-reliance, and, what this quality makes possible, self-sacrifice.

And if the people of India have on the one hand specially heavy burdens to bear, in resisting these and other destructive forces of nature arising from their climate, which will certainly tax their strength to the utmost, yet on the other they find ready to their hands unusual aid in the great reproductive powers of their soil, also due to the same cause. Neither can we estimate as of small value to them, the wealth, the knowledge, and the practical skill in all the arts of life, possessed by their British rulers, and so largely employed to their advantage. But that the task will in any case be a very heavy and tedious one to be got through, no one can doubt, who has passed a large part of his life, as I have done, in seeking for the means of extending those essential material allies in the battle of Indian life—irrigation works and railways.

True humanity assuredly demands of all Englishmen their co-operation in what will really conduce to the mitigation of the calamities caused by Indian droughts. But it is certain that there is only one possible mode of escape, namely through *labour*. The fruits of industry in years of plenty must be made to meet the want in years of scarcity; and relief is to be obtained by means of the combined and continued exertions of the localities immediately concerned, and no longer by relying on assistance to be supplied from without. There is no escape from the conclusion, that the conditions of their existence impose on the people of India severe suffering and periodic partial destruction, if they submit to these conditions unresisting; severe toil, and persistent intelligent effort, if they are to escape their extreme consequences. The Government and the people must everywhere have this practically enforced upon them, and until it is done the movement will not have fairly set in the right direction. Experience in India leads to exactly the same conclusions as those arrived at elsewhere, that a system of public relief in time of distress, not guarded by the sense of specific local financial responsibility, is a source of grievous abuse, misery, and demoralization; and it is my earnest hope that no temporary impulse of sympathy with present suffering, no selfish (if I may be allowed so to apply the term), no selfish effort to escape at any cost the pain of witnessing it, may be permitted to stand in the way of that real benevolence which is founded on sound principles, drawn by dispassionate intelligence from the lessons of experience, principles which I am glad to believe have been adopted by the highest authorities concerned in the government of India.

[R. S.]

WEEKLY EVENING MEETING,

Friday, May 25, 1877.

GEORGE BUSK, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

GEORGE J. ROMANES, M.A. F.L.S. &c.

Evolution of Nerves and Nervo-Systems.

NERVE-TISSUE universally consists of two elementary structures, viz. very minute nerve-cells and very minute nerve-fibres. The nerve-fibres proceed to and from the nerve-cells, thus (probably) serving to unite the cells with one another, and also with distant parts of the animal body. Moreover, nerve cells and fibres, wherever we meet with them, present very much the same appearances. Here, for instance, is a sketch of highly magnified nerve-tissue as we find it in the human brain, and here is one of my own drawings of nerve-tissue as I have found it in the jelly-fish; and you see how similar the drawings are —notwithstanding they are taken from the extreme limits of the animal kingdom within which nerve-tissue is known to occur.

Nerve-cells are usually found collected together in aggregates which are called nerve-centres or ganglia, to and from which large bundles of nerve-fibres come and go. These large bundles of nerve-fibres are what we see with the naked eye as nerves, permeating the body in all directions. When such a bundle of nerve-fibres reaches a ganglion, or collection of nerve-cells, it splits up like the end of a rope which has been teased out, and the constituent fibres pass into and out of the nerve-cells, so interlacing with one another in all directions, as here diagrammatically represented. More true to nature is this diagram, which represents a magnified section of human brain —the human brain being itself nothing more than a collection of very large ganglia.

To explain the *function* of nerve-cells and nerve-fibres, I must begin by explaining what physiologists mean by the word "excitability." Suppose this to represent a muscle cut from the body of a freshly killed animal. So long as you do not interfere with it in any way, so long will it remain quite passive. But every time you stimulate it, either with a pinch, a burn, or, as represented in the diagram, with an electrical shock, the muscle will give a single contraction in response to every stimulation. Now it is this readiness of organic tissues to respond to a stimulus that physiologists designate by the term excitability.

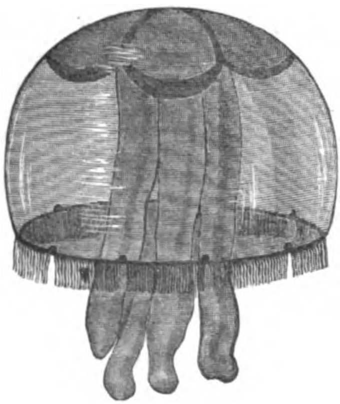
Nerves, no less than muscles, present the property of being excitable. Suppose, for instance, that this is another muscle prepared in the same way as the last one, except that together with the muscle there is cut out the attached nerve. Every time you pinch, burn, or electrify any part of the nerve, the muscle will contract. But you will carefully observe there is this great difference between these two cases of response on the part of the muscle; viz. that while in the former case the muscle responded to a stimulus *applied directly to its own substance*, in the latter case the muscle responded to a stimulus *applied at a distance from its own substance*, which stimulus was then *conducted* to the muscle by the nerve. And here we perceive the characteristic function of nerve-fibres, viz. that of conducting stimuli to a distance. This is the function of nerve-*fibres;* but the function of nerve-*cells* is different, viz. that of accumulating nervous energy, and at fitting times of discharging this energy into the attached nerve-fibres. The nervous energy when thus discharged from the nerve-cells acts as a stimulus to the nerve-fibre; so that if a muscle is attached to the end of the fibre it contracts on receiving this stimulus. I may add that when nerve-cells are collected into ganglia they often appear to discharge their energy spontaneously, without any observable stimulus to cause the discharge; so that in all but the lowest animals, whenever we meet with apparently spontaneous action, we infer that ganglia are probably present. But the point which most of all I desire you to keep well in mind this evening is the distinction which I here draw between muscle and nerve. A stimulus applied to a nerveless muscle can only course through the muscle by giving rise to a visible wave of contraction, which spreads in all directions from the seat of stimulation as from a centre. A nerve, on the other hand, conducts the stimulus without undergoing any change of shape. Now in order not to forget this all-important distinction, I shall always to-night speak of muscle as conducting a visible wave of *contraction*, and of nerve as conducting an invisible or molecular wave of *stimulation*. Nerve-fibres, then, are functionally distinguished from muscle-fibres—and also, I may add, from protoplasm—by displaying the property of conducting invisible or molecular waves of stimulation from one part of an organism to another—so establishing physiological continuity between such parts *without the necessary passage of contractile waves.*

I will now conclude all that is necessary to say about the function of nervous tissue by describing the mechanism of reflex action. Suppose this to represent any peripheral structure, such as a part of the skin of some animal, this a collection of nerve-cells or ganglion, and this a muscle. The part of the skin represented is united to the nerve-cells composing the ganglion by means of this in-coming nerve-trunk, while the nerve-cells in the ganglion are united to the muscle by means of this out-going nerve-trunk. Therefore when any stimulus falls on the skin where this in-coming nerve-trunk takes its origin, the nerve-trunk conveys the stimulus to the nerve-cells in the ganglion. When the nerve-cells receive the stimulus they liberate one of their characteristic discharges of nervous energy, which discharge then passes down this out-going nerve and so causes the muscle to contract. Now this particular kind of response is called response by reflex action, because the stimulus wave does not pass in a straight line from the seat of stimulation to the muscle, but passes in the first instance to the ganglion, and is from it *reflected* to the muscle. This, at first sight, appears to be a roundabout sort of a process, but in reality it is the most economic process available; for we must remember the enormous number and complexity of the stimuli to which every animal is more or less exposed, and the consequent necessity that arises, in the case of highly organized animals, of there being some definite system whereby these stimuli shall be suitably responded to. Or, to adopt a happy illustration of Professor Bain, the stimuli are systematized on the same principle as the circulation of letters by post is systematized; for just as in the case of the letters there is no direct communication between one street and another, but every letter passes first to the central office; so the transmission of stimuli from one member of the body to another is effected exclusively through a centre or ganglion.

Those among you who are acquainted with Mr. Herbert Spencer's writings are doubtless well aware how strong a case he makes out in favour of his theory respecting the genesis of nerves. This theory, you will remember, is that which supposes incipient conductile tissues, or rudimentary nerve-fibres, to be differentiated from the surrounding contractile tissues, or homogeneous protoplasm, by a process of integration which is due simply to use. Thus, beginning with the case of undifferentiated protoplasm, Mr. Spencer starts from the fact that every portion of the colloidal mass is equally excitable and equally contractile. But soon after protoplasm begins to assume definite shapes, recognized by us as specific forms of life, some of its parts are habitually exposed to the action of forces different from those to which other of its parts are exposed. Consequently, as protoplasm continues to assume more and more varied forms, in some cases it must happen that parts thus peculiarly situated with reference to external forces, will be more frequently stimulated to contract than are other parts of the mass. Now in such cases the relative frequency with which waves of stimulation radiate from the more exposed parts, will probably have the effect of creating a sort of polar arrangement of the protoplasmic molecules lying in the line through which these waves pass, and for other reasons also will tend ever more and more to convert these lines into passages offering less and less resistance to the flow of such molecular waves—i. e. waves of stimulation as distinguished from waves of contraction. And lastly, when lines offering a comparatively low resistance to the passage of molecular impulses have thus been organically established, they must then continue to grow more and more definite by constant use, until eventually they become the habitual channels of communication between the parts of the contractile mass through which they pass. Thus, for instance, if such a line has been established between the points A and B of a contractile mass of protoplasm, when a stimulus falls upon A, a molecular wave of stimulation will course through that line to B, so causing the tissue at B to contract—and this even though no *contractile* wave has passed through the tissue from A to B. Such is a very meagre epitome of Mr. Spencer's theory, the most vivid conception of which may perhaps be conveyed in a few words by employing his own illustration—viz. that just as water continually widens and deepens the channel through which it flows, so molecular waves of the kind we are considering, by always flowing in the same tissue tracts, tend ever more and more to excavate for themselves functionally differentiated lines of passage. When such a line of passage becomes fully developed, it is a nerve-fibre, distinguishable as such by the histologist; but before it arrives at this its completed stage—i. e. before it is observable as a distinct structure—Mr. Spencer calls it a "line of discharge."

Such being the theory, I will endeavour to show how it is substantiated by facts. And here it becomes necessary to refer to my own work. You are all, I suppose, acquainted with the general appearance of a Medusa, or jelly-fish. The animal presents the general form of a mushroom. The organ which occupies the same position as the stalk does in the mushroom is the mouth and stomach of the Medusa, and is called the polypite; while the organ which resembles in shape the dome of the mushroom constitutes the main bulk of the animal, and is called the swimming-bell. Both the polypite and the swimming-bell are almost entirely composed of a thick, transparent, and non-contractile jelly; but the whole surface of the polypite, and the whole *concave* surface of the bell, are overlaid by a thin layer or sheet of contractile tissue. This tissue constitutes the earliest appearance in the animal kingdom of true muscular fibres. The thickness of this continuous layer of incipient muscle is pretty uniform, and is nowhere greater than that of very thin paper. The margin of the bell supports a series of highly contractile tentacles, and also another series of bodies which are of great importance for us to-night. These are the so-called marginal bodies, which are here represented, but the structure of which I need not describe. Lastly, it may not be surperfluous to add that all the Medusæ are locomotive. The mechanism of their locomotion is very

FIG. 1.



Aurelia aurita, $\frac{1}{8}$ nat. size.

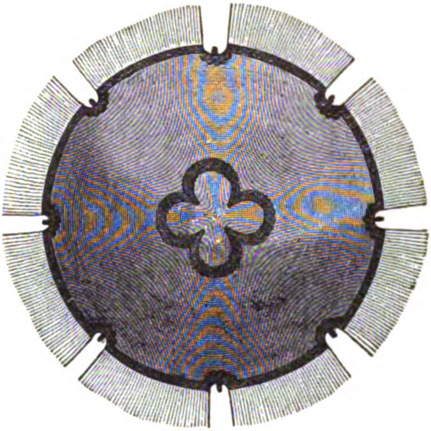
simple, consisting merely of an alternate contraction and relaxation of the entire muscular sheet which lines the cavity of the bell. At each contraction of this muscular sheet, the gelatinous walls of the bell are drawn together; the capacity of the bell being thus diminished, water is ejected from the open mouth of the bell backwards, and the consequent reaction propels the animal forwards. In these swimming movements systole and diastole follow one another with as perfect a rhythm as they do in the beating of a heart.

Those of you who were present at my lecture last year will no doubt remember my having told you, that on merely cutting off the extreme marginal rim of the bell I was surprised to find that the previously active motions of the animal suddenly and entirely ceased; the paralysis caused by this simple operation was instantaneous, enduring, and complete. On the other hand, you may remember, the severed margin which had just been taken from the swimming-bell invariably continued its rhythmical motions with a vigour and a pertinacity not in the least impaired by its severance from the main organism. For hours, and even for days after the operation, these motions persisted; so that the contrast between the death-like quiescence of the mutilated swimming-bell, and the active contractions of the thread-like portion which had just been removed from its margin, was a contrast as striking as it is possible to conceive.

These experiments, then, conclusively proved that in the marginal rim of the Medusæ there is situated an intensely localized system of nervous centres, or ganglia, to the functional activity of which the rhythmical motions of the swimming-bell are exclusively due. And as the Medusæ are thus the lowest animals in which a nervous system has yet been discovered, we have in them the animals upon which we may experiment with the best hope of being able to elucidate all questions concerning the origin and endowments of primitive nervous tissues. I have therefore spent much time and labour, both last year and this year, in cultivating this field of inquiry; and as it is a field whose ground had never before been broken and whose fertility has proved itself prodigious, it is not surprising that I should have reaped a rich harvest of results. So far as these results have any bearing on the general theory of evolution, their character is uniformly such as that theory would lead us to expect. For if I had two hours at my disposal instead of one, I might mention a number of facts which tend to show, in a very striking manner, that the primitive nervo-muscular tissues of the Medusæ, in respect of their physiological properties, present unmistakable affinities, on the one hand with the excitable tissues of certain plants, and on the other hand with the nervo-muscular tissue of higher animals. But not having time to go into this matter, I shall on the present occasion restrict myself to describing such of my results as tend to substantiate Mr. Herbert Spencer's theory concerning the mode in which nerves and nervo-systems have been evolved. And I adopt this course, not only because I feel that any facts bearing on so important a subject cannot fail to be of interest to all intelligent persons; but also because I think that this is a place best suited for publishing the somewhat speculative inferences which I have drawn from my facts. If these provisional inferences should by subsequent experiments be proved to be correct, their philosophical as well as their scientific influence will be great and far-reaching; but until they shall have been more completely verified I have not thought it desirable to adduce them in my communications to the Royal Society. Referring, therefore, those among you who may be interested in the research as a whole to the 'Philosophical Transactions,' I will now invite your attention to a connected interpretation of some of the facts that it has yielded—an interpretation which I here publish for the first time.

To begin, then, with this diagram. It represents *Aurelia aurita* with its polypite cut off at the base, and the under, or concave, surface of the bell exposed to view. The bell, when fully expanded, as here represented, is about the size of a soup-plate, and in it all the ganglia of the margin are collected into the eight marginal bodies; so that on cutting out these eight marginal bodies total paralysis of the bell ensues. But although the bell is thus paralyzed as to its *spontaneous* movements, it continues responsive to stimulation; for every time you prick or electrify any part of the contractile sheet, a wave of contraction starts from the point which you stimulate, and spreads from that point in all directions as from a centre. Such contractile waves, at ordinary temperatures, travel at about the rate of a foot and a half per second; and the important question with regard to them which we shall have to consider to-night is this—Are they

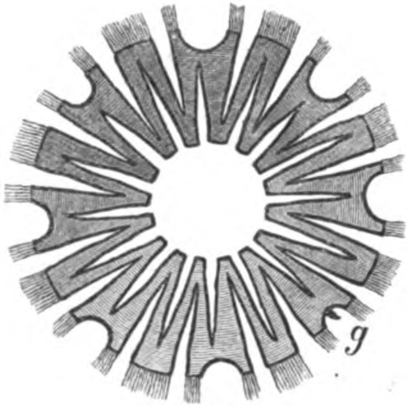
FIG. 2.



merely of the nature of muscle-waves, such as we see in undifferentiated protoplasm, or do they require the presence of *nerve*-fibres to convey them – the *stimulus* wave in the *nerve*-fibres progressively causing, as it advances, the *contractile* wave in the *muscle*-fibres?

Now the great argument in favour of these contractile waves being muscle-waves, and nothing more, is simply this – that the contractile tissue is able to endure immensely severe forms of section without the contractile waves in it becoming blocked. For instance, when the bell of *Aurelia* is cut as here represented, and any part of the circle is stimulated, a contractile wave radiates from the point of stimulation just as it did before the cuts were introduced, notwithstanding the wave has now to zig-zag round and round the ends of the overlapping cuts. Similarly, if instead of employing artificial stimulation a single ganglion (*g*) be left *in situ* while all the other seven are removed, contractile waves will radiate in rhythmical succession from the single remaining ganglion, and course all the way round the disc. Now this experiment seems to prove that the contractile waves depend for their passage, not on the conductile function of any primitive nervous network, but on the protoplasmic qualities of the primitive muscular tissue. The experiment seems to prove this, because so severe a form of section would seem of necessity to destroy the functional continuity of anything resembling such a nervous network as we observe in higher animals.

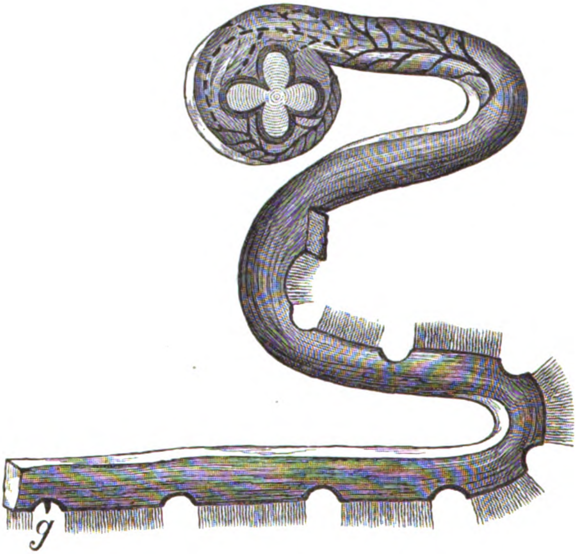
Fig. 3.



Here, again, is another form of section. Seven marginal bodies having been removed as before, the eighth one was made the point of origin of a circumferential section, which was then carried round and round the disc in the form of a continuous spiral—the result, of course, being this long ribbon-shaped strip of tissue with the ganglion (*g*) at one end, and the remainder of the swimming-bell at the other.

Well, as before, the contractile waves always originated at the ganglion; but now they had to course all the way along the strip until they arrived at its other extremity, and as each wave arrived at that extremity it delivered its influence into the remainder of the swimming-bell, which thereupon contracted. Hence, from this mode of section as from the last one, the deduction certainly appears to be that the passage of the contractile waves cannot be dependent on the presence of a nervous plexus; for nothing could well be imagined as more destructive of the continuity of such a plexus than this spiral mode of section must be.

FIG. 4.



Nevertheless there is an important body of evidence to be adduced on the other side; but as I can only wait to state a few of the chief points, I shall confine my observations to the spiral mode of section. First of all, then, I have invariably found it to be the case that if this mode of section be carried on sufficiently far, a point is sooner or later sure to come at which the contractile waves cease to pass forward: they become blocked at that point. Moreover, the point at which such blocking of the waves takes place is extremely variable in different individuals of the same species. Sometimes the waves will become blocked when the strip is only an inch or less in length, while at other times they continue to pass freely from end to end of a strip that is only an inch broad and more than a yard long; and between these two extremes there are all degrees of variation. Now if we suppose that the influence of the ganglion at the end of the strip is propagated as a mere muscle-wave along the strip, I cannot see why such a wave should ever become blocked at all, still less that the point at which it does become blocked should be so variable in different individuals of the same species. On the other hand, if we suppose the propagation of the ganglionic influence to be more or less dependent on the presence of a more or less integrated nerve-plexus, we encounter no difficulty; for on the general theory of evolution it is to be expected that if such fibres are present in such lowly animals they should not be constant as to position.

But there is a still stronger argument in favour of nerve-fibres, and it is this. At whatever point in a spiral strip which is being progressively elongated by section the blocking of the contractile wave takes place, such blocking is sure to take place *completely* and *exclusively* at that point. Now I cannot explain this invariable fact in any other way than by supposing that at that point the section has encountered a line of functionally differentiated tissue—has severed an incipient nerve.

Such, some of you may remember, was the state of the evidence when I last addressed you upon this subject. On the whole I provisionally adopted the view that all parts of the muscular sheet of the Medusæ are pervaded by a plexus of nerves, or "lines of discharge;" and I explained the fact of the tissues in some cases enduring such severe forms of section without suffering loss of their physiological continuity, by supposing all the nerve-fibres composing the plexus to be capable, in an extraordinarily high degree, of vicarious action. If you were to represent the hypothetical nervous plexus by a sheet of muslin, it is clear that however much you were to cut the disc of muslin with such radial or spiral sections as are represented in the diagrams, you could always trace the threads of the muslin with a needle round and round the disc without once interrupting the continuity of your tracing; for on coming to the end of a divided thread you could always double back on it and choose another thread which might be running in the required direction. And this is what I last year stated to be my opinion as to what took place in the fibres of the hypothetical nervous plexus; whenever a stimulus wave arrives at a cut, I imagined it to double back and pass into the neighbouring lines of discharge, which I thus supposed to act vicariously for the divided line.

Such, then, when last I addressed you, was the standing of this question as to the character of these highly remarkable contractile waves. On the whole I decided in favour of a nervous plexus, notwithstanding the improbability that such a plexus should be capable of vicarious action in all its parts to so almost unlimited a degree.* I am glad to say that this decision has now been further justified by some additional observations which are of the first importance. For since my last lecture I have noticed the fact that reflex action takes place between the marginal ganglia of the Medusæ and all the contractile tissues of the animal. Thus, for instance, if you seize the polypite with a pair of forceps, the marginal ganglia almost immediately set the swimming-bell in violent motion, thereby showing that the stimulus must have coursed up the polypite to its point of insertion in the bell, and then down the sides of the bell to the ganglia, so causing them to discharge by reflex action. Again, suppose that seven of the eight ganglia have been removed from the margin of an *Aurelia*, and that any part of the contractile disc is stimulated too gently to start a contractile wave from the point

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* This antecedent improbability is not so overwhelming as it is at first sight apt to appear; for we must remember that in a peripheral nervous plexus as we

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meet with it in the higher animals—i.e. in the fully evolved form of such a structure—each of the constituent nerve-fibres is provided with an insulating coat for the very purpose of preventing vicarious action among these fibres, and the consequent confusion among the reflex mechanisms which such vicarious action would manifestly occasion. But because insulation of peripheral nerve-fibres is thus an obvious necessity in the case of a fully evolved nervous plexus, it by no means follows that any high degree of insulation should be required in the case of an incipient nervous plexus. On the contrary, any hypothesis as to the manner in which nerve-fibres first begin to be differentiated from protoplasm, must suppose that the conductile function of the incipient nervous tracts precedes any structure, such as that of nerve-coats, whereby this function is strictly confined to particular tracts. The antecedent probability being thus in favour of the view that insulating structures are a product of later evolution than are the essential nervous structures which they insulate, it would clearly be very hazardous to draw any analogy between an incipient nervous plexus such as I suppose to be present in the Medusæ, and a fully-evolved peripheral plexus of any of the higher animals. A less hazardous analogy would be furnished by the fibres which occur in the *central* nervous system of the higher animals; for here it may be said, both *à priori* from Mr. Spencer's theory and *à posteriori* from histological indications, that the nerve-fibres occur in various degrees of differentiation. And that vicarious action is possible to some considerable extent through a bridge of the grey matter of the cord, has been shown by the double hemi-section experiments of Brown-Séquard. Moreover, the admirable experiments of Goltz would seem to indicate that vicarious action is also possible to some extent among the ultimate elements of the brain. I may add that recent research has tended to suggest a novel interpretation of the way in which certain poisons, such as strychnia, act upon the cord; for whereas it has hitherto been supposed that the abnormal reflex excitability which these poisons engender is due to their exerting a stimulating influence on the cord, the researches in question have fairly well proved that the very reverse is true, viz. that the action of these poisons is to *depress* the vitality of the cord. For a number of facts go to prove that the abnormal reflex excitability is due to the impairment of some function which has been provisionally termed "resistance of the cord," a function which in health prevents the undue spread of a stimulus through the substance of the cord, and the impairment of which by the poison consequently admits of a stimulus spreading to an undue extent, so giving rise to the abnormal reflex excitability in question. As bearing on this subject, I may observe that while the action of strychnia on the Medusæ is the same as it is on the higher animals, viz. that of causing paroxysmal convulsions, it certainly seems to exercise a *depressing* influence on the tissues; for an extremely weak sea-water solution has the effect of blocking contractile waves in any part of a spiral strip that is submitted to its influence.—G. J. R.

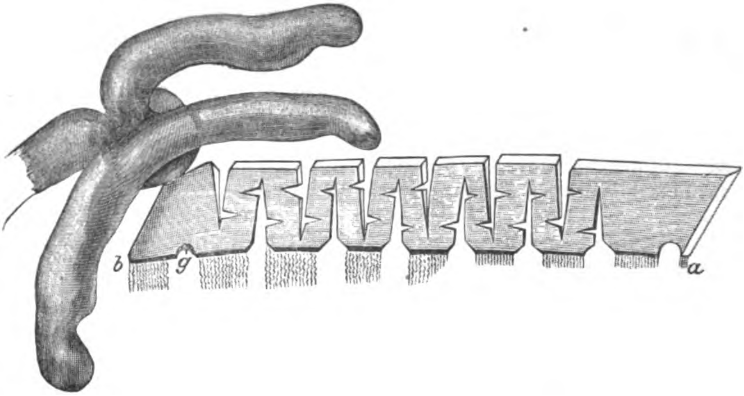
immediately stimulated, a contractile wave will nevertheless shortly afterwards start from the ganglion, thus showing that a *stimulus* wave must have passed through the contractile sheet to the ganglion, and so caused the ganglion to discharge. Indeed in many cases the passage of this stimulus wave admits of being actually *seen.* For it is a peculiarity of the numberless tentacles which fringe the margin of this Medusa, that they are more excitable than is the contractile issue of the bell. Consequently a stimulus may be applied to the contractile tissue of the bell which is not strong enough to start a contractile wave in the bell-tissue itself, and is yet strong enough to start a contractile wave in the tentacles—one tentacle after another contracting in rapid succession until the wave of stimulation has passed all the way round the disc. The latter, of course, remains quite passive until the tentacular wave, or wave of stimulation, reaches one of the ganglia (or the single remaining ganglion, if the disc has been prepared by removing seven of the ganglia), when, after an interval of half a second for the period of latency, the ganglion is sure to discharge, and so to cause a general wave of contraction.

Now these facts prove in a singularly beautiful manner—for this optical expression of the passage of a wave of stimulation is a sight as beautiful as it is unique—these facts, I say, conclusively prove that the whole contractile sheet of the bell presents, not merely the protoplasmic qualities of excitability and contractility, but also the essentially nervous quality of conducting stimuli to a distance irrespective of the passage of a contractile wave. So I conclude there can be no longer any question that we have here to deal with a tissue already so far differentiated from primitive protoplasm, that the distinguishing function of nerve has become fully established.

The question, however, remains: Will this conductile function prove itself as tolerant towards section of the tissue as the contractile function has already proved itself to be? for, if so, any objection to the view that the passage of the *contractile* waves is due to vicarious action of rudimentary nerve-fibres will be removed. Briefly, the answer to this question is an affirmative; for I find it is quite as difficult to block the passage of stimulus waves by means of interposing cuts, as we have seen that it is to block the passage of contractile waves by the same means. For instance, here is an *Aurelia*, the bell of which has been cut into the form of a continuous parallelogram of tissue, and then submitted to the tremendously severe form of section which is depicted. Yet on very gently stimulating any point in this expanse of tissue, as at the end *a*, a tentacular wave would course all the way along the margin to *b*, thus showing that the wave of stimulation must have passed round and round the ends of all the intervening cuts. In the diagram the tentacular wave is represented as having traversed one-half of the whole distance from *a* to *b*, and near *b* there is represented a single remaining ganglion (*g*). When, therefore, the tentacular wave reaches *g*, this ganglion will shortly afterwards discharge, so giving rise to a contractile wave, which will course back from *g* to *a* in the opposite direction to that which the stimulus wave had previously pursued.

And this, I am not afraid to say, is the most important observation, both to the physiologist and to the evolutionist, that has ever been made in the whole range of invertebrate physiology. For to the physiologist this observation proves that the distinguishing function of nerve, where it first appears upon the scene of life, is a function which admits of being performed vicariously to almost any extent by all parts of the same tissue mass; while to the evolutionist the observation proves the existence of such a state of things as his theory of nervo-genesis would lead him to expect. In such a symmetrically formed animal as a Medusa, with all parts of the contractile sheet precisely resembling one another, we should expect the lines of discharge composing the hypothetical plexus to be very numerous, and all very much alike with respect to the degree of their evolution. For, as the symmetrical form of the disc does not require that any one set of lines should be used much more frequently than any other set, it follows from Mr. Spencer's theory that all the lines should more or less resemble one another as regards the extent of their differentiation.* That is to say, they should all be lines presenting

FIG 5.



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* Mr. Spencer himself observes, "The average equality of the forces to which their bodies (i. e. those of the Medusæ) are exposed all round, is unfavourable to the formation of distinct muscles, and a distinct nervous system."—('Psychology,' vol. i. p. 522.) Although this statement must now be modified so far as the ganglionic system of the Medusæ is concerned, I do not think that the anticipation which it embodies should on this account be deemed unwarrantable so far as it applies to other parts of the nervous system. For although it is true that a Medusa *as a whole* is "exposed all round" to an "average equality of forces," it

about the same degree of resistance to the passage of a stimulus wave, and therefore it should become a matter of indifference, so to speak, through which particular set of lines such a wave takes its course.

There is still another class of facts which to my mind makes very strongly in favour of Mr. Spencer's theory. Assuming, as I think we are now entitled to assume, that the contractile waves are not merely muscle-waves, but depend for their passage on the progressive passage of the stimulus waves—assuming this, and the following facts become facts of great significance. When the contractile waves in a spiral strip have become suddenly blocked by section, in many such blocking will be permanent—even though the strip be continuously stimulated, whether artificially or by a single terminal ganglion, as represented in Fig. 4. But in other cases, after a time that varies from a few minutes to a day or more, the obstruction is overcome, and the contractile waves pass forward with perfect freedom. Now, if I had time, I could prove that these facts are certainly not to be attributed to what physiologists term *shock*; and therefore it seems to me that only one hypothesis remains. What I have recently said about most of the lines of discharge in the supposed plexus being very much alike as regards the degree of their differentiation, does not, of course, mean that all the lines are *exactly* alike in this respect; for on *à priori* grounds such a state of things would be in the last degree improbable. Consequently, in conducting a spiral section, it must happen that at every snip the scissors cut through a number of lines of discharge presenting various degrees of differentiation; and, such being the case, the fact of the sudden and final blocking is presumably due to a well-differentiated line having been severed in a part of the tissue where no other line occurs of a sufficient degree of differentiation to conduct the stimulus forward. Now in most instances, as we should expect, the blocking so caused is permanent; for it is manifest that the formation of nervous channels, in the way suggested by Mr. Spencer, cannot proceed at so great a rate as to admit of *wholly* new lines of discharge being established during the lifetime of a mutilated Medusa, i. e. during the course of a few days. Nevertheless, according to the hypothesis, some small percentage of cases might be expected to occur in which such blocking of the contractile waves would only be temporary. For some cases would almost certainly occur in which the relations of the highly differentiated line just destroyed to the more slightly differentiated lines in the neighbourhood of the section,

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is not true that the *excitable* portions of a Medusa are thus equally exposed. On the contrary, the margin of the excitable sheet which lines the cavity of the bell, occupies a much more exposed position than does any other part of that sheet; and whether or not this fact has anything to do with the development of the ganglia in the only part of the excitable sheet which is thus peculiarly situated, I think it is obvious that this part of a Medusa ought to be carefully excepted in the statement which I have quoted. With regard to all other parts of the excitable sheet, however, the statement is certainly correct; and it is only to such parts that the considerations in the text apply.—G. J. R.

would happen to be such that the more slightly differentiated lines would be very nearly, though not quite, able to act vicariously for the more highly differentiated line which has just been destroyed (see Fig 4, where the deep line represents the well-differentiated line which has just been severed, and the dotted line the less differentiated one which is still intact). The contractile waves, therefore, would in the first instance become suddenly blocked at the end of the strip. But the molecular, and with them the contractile, waves still continuing to pass quite up to the end of the strip, and being there always suddenly stopped, a rude conflict of molecular forces will thus be set up in the area where these waves are impeded, and each of the forces concerned will seek for itself the line of least resistance. Hence, as the successive waves beat rhythmically on the area of obstruction, more or less of the molecular disturbance must every time be equalized through these lines of discharge which from the first have been almost sufficient to maintain the physiological continuity of the tissue. Therefore, according to the hypothesis, every wave that is blocked imposes on these particular lines of discharge a much higher degree of functional activity than they were ever before required to exercise; and this greater activity causing in its turn greater permeability, a point will sooner or later arrive at which these lines of discharge from having been *almost* become *quite* able to draft off sufficient molecular motion, or stimulating influence, to carry on the contractile waves beyond the area of previous blocking. In such instances, of course, we should expect to find, what I always observed to be the case, viz. that the first contractile waves which pass the barriers are only very feeble, the next stronger, the next still stronger, and so on, according as the new passage becomes more and more permeable by use; until at last the contractile waves pour over the original barrier without any perceptible diminution of their force. In some cases, by exploring with graduated stimuli and needle-point terminals, I was able to ascertain the precise line through which this eruption of stimulating influence had taken place; so that altogether I think these facts tend very strongly to confirm Mr. Spencer's theory regarding the genesis of nerves.* I will only add that if this interpretation of the facts is correct, we have in them a striking instance of the uniformity with which Nature works. A scientific theory concerning the evolution of nerves, which a year or two ago it seemed impossible to verify, from the fact that it seemed as though the observations which would be required to verify it would need to extend over thousands of years—this theory is now, I believe, being verified by observations which need only extend over hours and minutes. The immensely protracted history of nervo-genesis upon this planet is thus probably reproduced in a greatly foreshortened manner in the facts which I have explained; and inconceivable as is the difference between these two histories of nervo-genesis in respect

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* As additional proof that a wave of stimulation may pass over a barrier of the kind described in too small a quantity to start a wave of contraction beyond the barrier, I may mention that in some cases I have observed that the establishing of a new line of physiological connection is a more gradual process than stated in the text. To show this, I may briefly quote one very instructive case. Seven marginal bodies having been removed, the eighth one continued to originate contractile waves, which coursed round the swimming-bell as usual. I now made a radial cut half an inch on one side of the marginal body, and extending to the centre of the swimming-bell. The contractile waves were immediately blocked—thus showing, as did a somewhat similar experiment detailed in my first Royal Society paper (p. 293), "that the influence of the marginal body had previously been communicated to the swimming-bell *from one side only*." But in the case we are now considering, the discharges of the marginal body were still rendered apparent by very local contractions of a tissue area in the immediate

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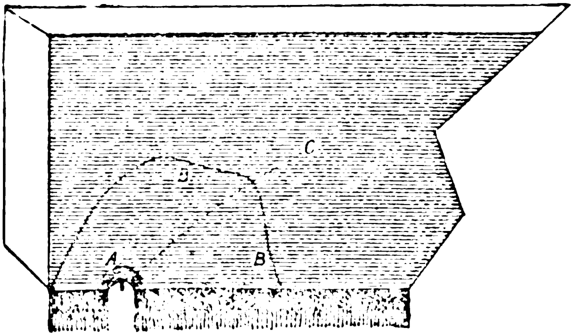
vicinity of that body, the area, namely, which in the figure representing one end of the strip is marked B B. Exploration by stimulus now showed that general contractile waves could only be started outside the area B B. In somewhat more than half an hour after the operation (during which time the area B B continued to contract rhythmically), the ganglionic influence for the first time extended from the area B B to the rest of the strip—the contraction being therefore general.

FIG. 6.

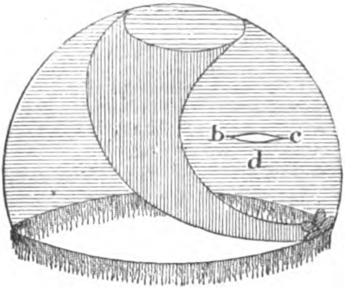


After this first eruption of contractile influence, there succeeded a period of about a minute, during which the area B B continued to contract independently as before. Then another eruption took place, followed by another period of restricted contraction, and so on. Next, these general contractions became progressively more and more frequent, and as the rhythm always continued the same, whether the contractions were local or general, the number of the latter became increased at the expense of that of the former. Thus, while at first there were twenty or thirty local contractions between every two general contractions, this proportion gradually fell to fifteen, ten, five, &c., till the numbers became equal, after which the balance began to incline in favour of the general contractions. Eventually the local contractions ceased altogether, and on now excising the marginal body and exploring by stimulus, I was able to localize very precisely the line through which physiological continuity had been established between B B and the rest of the contractile strip. This line was A C, as shown by the fact that while stimulation of any other part of the area B B was followed only by a local contraction of that area, stimulation of the line A C was always followed by a general contraction.—G. J. R.

of their duration, it is nevertheless most probably in respect of their duration alone that these two histories differ.

I will now invite your attention to another species of Medusa, which is of a somewhat more highly evolved type than *Aurelia*, and which I have called *Tiaropsis indicans*, in allusion to a highly interesting and important function which is displayed by its polypite. This function consists in that organ localizing, with the utmost precision, any point of stimulation situated in the bell. For instance, if the bell be pricked with a needle at this point (*a*), the polypite immediately moves over and touches that point, as represented in the diagram. If immediately afterwards any other part of the bell be pricked, the polypite moves over to that part, and so on. Now this, you will perceive, is a highly remarkable function; for it proves that all parts of the bell must be pervaded by lines of discharge, every one of which is capable of conveying a separate stimulus to the polypite, and so of enabling the polypite always to determine which of the whole multitude is being stimulated. This localizing function of the polypite therefore shows that the lines of discharge must be more differentiated in this species than they are in *Aurelia;* for it shows that vicarious action cannot be possible among them in so high a degree: every line of discharge must here have acquired a more specialized character, in order that the message which it conveys to the polypite when itself directly stimulated may not be confused with that which is conveyed by any other line.

FIG. 7.



Tiaropsis indicans, slightly enlarged.

Now it is easy to be wise after the event; but the state of things we here observe is just such a state of things as I think we should expect to constitute the next stage of nervo-evolution. It is no doubt a benefit to this Medusa that its polypite is able to localize a seat of stimulation in the bell; for the end of the polypite is provided with a stinging apparatus, and is besides the mouth of the animal. Consequently, when any living object touches the bell—whether it be an enemy or a creature serving as prey—it must alike be an advantage to the Medusa that its polypite is able to move over quickly to the right spot, in the one case to sting away the enemy, and in the other to capture the prey. Hence I think that natural selection would probably tend to convert lines of discharge in promiscuous directions, into lines of discharge in definite directions—thus developing the function of localization. At first, no doubt, this function would be performed only in a general and tentative manner (as, indeed, I have observed in the case of *Aurelia*); but gradually, by the combined action and mutual reaction of use and survival of the fittest, this function would come to be performed with ever-increasing precision.*

This, then, I conceive to be an important step in the evolution of nervous systems—foreshadowing as it does the principle of co-ordination among muscular movements, which in all the higher animals is effected by reflex mechanisms precisely resembling, as to their function, the primitive reflex mechanism we are considering. But now another point of interest arises. As Spencer's theory supposes a line of discharge to become more and more definite by use, if, for the maintenance of any particular function such as the one we are considering, a certain line of discharge habitually serves as a line of communication between two points of the animal tissues, it follows that this line will offer less resistance to the passage of a stimulus between these two points than would any other line in the organism. Consequently, so long as such a line remains intact, so long we should expect what we have seen to be the case, viz. that little or no vicarious action takes place between it and other lines. But let this line be severed, and let there be a number of closely adjacent lines, as there must be in this particular instance, and should we not expect, both from Spencer's theory and from our knowledge of *Aurelia*, that at some such grade of nervous evolution as *Tiaropsis* presents, the stimulus should be able to *escape* from the severed to the unsevered lines? And this I find to be the case. For if a small cut be introduced between the base of the polypite and the seat of injury in the bell, the polypite is no longer able to *localize* the seat of injury, although it still continues to perceive, so to speak, that injury is being applied *somewhere*. For instance, if a short cut be introduced as here represented at *b c*, and you prick the bell anywhere below the cut, as at *d*, the polypite, instead of immediately applying its extremity to the exact spot that is being stimulated, now actively dodges about first to one part and then to another part of the bell, as if seeking in vain for the offending body, which, however, it cannot succeed in finding. Now I explain this marked change in the behaviour of the polypite by supposing that the wave of stimulation in this case runs along the habitual line of discharge till it reaches the cut; but being there no longer able to pursue this habitual line of least resistance, the wave of stimulation escapes into the adjacent lines, and so spreads all over the bell. Hence a number of conflicting messages are simultaneously delivered to the polypite, which therefore executes the random movements I have described—each of these movements being presumably determined by the relative degree in which now one line and now another takes part in conveying the scattered stimulus.

And now for another expectation to be realized. We should expect that the higher degree of specialization, which in these lines of discharge prevents vicarious action so long as the lines are undivided, should have the effect of rendering such vicarious action as we have seen to ensue when the lines are divided, less easy than it is in *Aurelia*, where the specialization of the lines being less pronounced, vicarious action among them is presumably more habitual. And such I find to be the case; for while in *Aurelia*, as we have seen, stimulus waves continue to zig-zag round and round the ends of almost any number of overlapping cuts, in *Tiaropsis* two or three such cuts are sufficient to destroy, not only the localizing, but also the random movements of the polypite—the latter then remaining passive, because the stimulus waves are wholly blocked.

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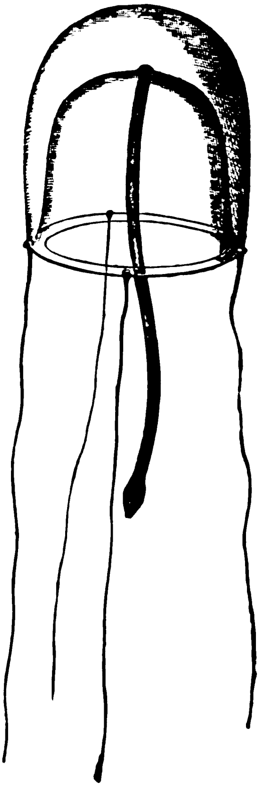
* It may be here observed that Mr. Spencer, in his theory of nervo-genesis, expressly supplements his hypothesis as to the direct influence of use, with that as to the indirect influence of natural selection. (See 'Biology,' § 164.)—G. J. R.

And lastly, before leaving the case of *Tiaropsis indicans*, I should like to mention the noteworthy fact, that although the polypite is able to perform the intricate ganglionic *function* of localizing any seat of stimulation in the bell, no signs of ganglionic *structure* can be detected with the microscope. Moreover, a portion of any size that is removed from the polypite continues to perform the localizing function, in just the same way as does the entire organ. In other words, this localizing function, which is so very efficiently performed by the polypite of this Medusa, and which, if anything resembling it occurred in the higher animals, would certainly have definite ganglia for its structural correlative, is here shared equally by all parts of the exceedingly tenuous excitable tissue that forms the outer surface of the organ. The case of the incipient ganglia of the polypite thus resembles that of the incipient nerves of the bell in this respect—that in both cases obvious signs of characteristic function are displayed before any corresponding signs of structure can be distinguished. Nerve-cells therefore, no less than nerve-fibres, are thus shown to have their first beginnings in differentiations of protoplasmic substance which are too refined for the microscope to analyze.

There is one other species of Medusa about which I should like to say a very few words, because it presents a still higher grade of nervous evolution than *Tiaropsis*. This is *Sarsia*, a Medusa in which the lines of discharge have in some places become so far differentiated as to admit of being actually seen, and are therefore entitled to be called nerves. All round the margin, and likewise along the course of the radial tubes, these, the earliest visible nerve-fibres in the animal kingdom, may be traced. And as we might anticipate, the advance of structure which is implied by an invisible "line of discharge" becoming a visible nerve-fibre, entails a corresponding advance of function. In the first place, the rate at which a stimulus travels seems to be much greater along these fully-evolved nerve-fibres than it is in the more rudimentary nerves or lines of discharge in *Aurelia*. In the next place, this greater differentiation of nerve-tissue renders the nervous connection between any two parts of the organism much more definite, and therefore vicarious action less promiscuous, than we have seen it to be in the other jelly-fishes; so that, for instance, a tentacular wave in this species may be blocked by a single short cut through the margin of the bell. Lastly, it is in this species that I was first able to perceive any unequivocal evidence of co-ordination among the marginal ganglia. In all the other species of Medusæ the marginal ganglia appear to act independently of one another; but in this species, where the marginal ganglia are first seen to be united by a visible nerve-fibre, they always act in concert. So much, indeed, is this the case, that the animal is able to steer itself in any required direction, as proved by the experiment which I described last year, whereby individuals of this species were shown to have the power of following a moving beam of light round and round the vessel in which they were contained. I may also remark that individuals of this species present much more nervous energy than those of any other species of Medusæ which I have had the opportunity of observing.

Fig. 8.



Sarsia tubulosa, × three times.

I have now, ladies and gentlemen, communicated some of the points wherein my work has tended to elucidate the early stages in the evolution of nerves and nervous systems. And these are just the stages concerning which elucidation is most required. When once nerve-fibres and nerve-cells have been fully evolved and arranged in the form of simple reflex mechanisms, the subsequent history of their evolution into compound nervous systems is readily intelligible. The principles on which this higher evolution is effected are throughout the same, and result essentially in establishing ever more and more advanced degrees of integration. Compare, for instance, these diagrams which represent severally the nervous systems of an earth-worm, a centipede, an insect, and a spider; and observe the progressive fusion of ganglia which has taken place. The progressive centralization which is thus effected is no doubt ultimately due to natural selection, if not exclusively, at any rate in large part; for this increasing consolidation of the reflex mechanisms must be of great benefit to the organisms which present it—serving as it does to render possible muscular movements ever more and more varied and combined. In the vertebrated series of animals the evolution of central nervous matter consists chiefly in adding to the size of ganglia by increasing the number of their ultimate nervous elements, nerve-cells and nerve-fibres. This progressive increase in the size of ganglia is especially remarkable in the case of the cerebral hemispheres. Now the cerebral hemispheres are the ganglia which we know to be the exclusive seat of the intellectual faculties; and their progressive increase in bulk as we ascend through the animal series, is undoubtedly to be regarded as the structural correlative of that progressive advance of the intellectual powers which is so conspicuously apparent as we ascend from the lower animals to Man.

And now, in conclusion, I should like to observe, that even in this the highest product of nervous evolution—the supreme ganglia or cerebral hemispheres of Man—not only do we still encounter the same fundamental constituents of structure as we observe in all other ganglia; but the cells and fibres in the brain of a man do not differ in any marked degree from the cells and fibres in the ganglion of an *Aurelia.* There is, however, a prodigious difference in the product of their operation. When ordinary ganglion cells discharge their influence, the result is, as we have seen, a muscular contraction; but when cerebral cells discharge their influence, we of to-day can have no doubt that the result is a mental change. And although we freely acknowledge that we are here standing on the border-land of insoluble mystery, we are not afraid to assert with confidence, that in the amazing complexity of the brain's structure—amid those millions on millions of interlacing cells and fibres—we have the physical aspect of all those relations, which on their psychical aspect we know as thoughts and feelings. Do you think that this sounds like materialism? I am not here to-night to discuss that point; but I may observe, in passing, that even were I able to tell you the particular cerebral elements which I now use in expressing this statement to you, I should be just as much or just as little on the way towards proving materialism, as I am when I tell you that a blow on the head produces insensibility. Science can never go farther than common sense in proving any *necessary* connection to subsist between mind and matter; for all that science can ever do is to ascertain numerous details with regard to such connection as undoubtedly does exist, and which, as a matter of daily experience, common-sense has already and completely recognized. However, materialism or no materialism, it is manifest that the facts being what they are, Mr. Spencer's theory as to the genesis of nerves must not be allowed to stop short just where its presence is most required. As we have seen that the cerebral hemispheres of man resemble all other ganglia in structure, we cannot hesitate in concluding, that if Mr. Spencer's theory is valid in explaining the genesis of nerves in general, in can be no less valid in explaining the genesis of these supreme ganglia in particular. And as we have every reason to believe that the functional operations of these supreme ganglia are inseparably associated with our thoughts and feelings, we are driven to the yet further conclusion, that if Mr. Spencer's theory is of any validity at all, our possible as well as our actual thoughts and feeling are determined by the strictly physical conditions under which molecular waves of stimulation course through the structure of the brain. So that in this Spencerian hypothesis of lines of discharge becoming more and more definite by use, we have a physical explanation, which is perhaps as full and as complete as such an explanation can ever be, of the genesis of Mind. From the time that intelligence first dawned upon the scene of life, whenever a new relation had to be established in the region of mind, it could only be so established in virtue of some new line of discharge being excavated through the substance of the brain. The more often this relation had to be repeated in the mind, the more often would this discharge require to take place in the brain, and so the more easy would every repetition of the process become; until at last the line of discharge grows into a nerve-fibre, and becomes the inherited property of the race. Thus it is, according to the theory, that there is always a precise proportion between the constancy with which any relations have been joined together during the history of intelligence, and the difficulty which intelligence now experiences in trying to conceive of such relations as disjoined. Thus it is that, even during the history of an individual intelligence, "practice makes perfect," by frequently repeating the needful stimulations along the same lines of cerebral discharge—so rendering the latter even more and more permeable by use. Thus it is that a child learns its lessons by frequently repeating them; and thus it is that all our knowledge is accumulated. In a word, if, as has been truly said, "man is a bundle of habits," we have in Mr. Spencer's theory of nervo-genesis a physical explanation of the fact. And forasmuch as it is upon this theory that Mr. Spencer may be said to found that great monument of modern thought—his "Principles of Psychology," I cannot but feel that one of the most important bearings which my work on the Medusæ has had, is that of supplying facts which tend to substantiate this theory—and this at a time when it seemed as though the theory could never have other than *à priori* considerations for its support. But if my interpretation of these facts is correct, this important theory is now receiving inductive verification from a most unexpected source. At first sight no two organic structures could well seem to have less in common than the swimming-bell of a Medusa and the brain of a Man; nor could anything seem more unlikely than that a great psychological theory should derive support from the study of polypes, where the very existence of a nervous system has only just been discovered. But here again, I believe, we may discern the uniformity of Nature; and while watching the passage of the waves of stimulation in the contractile strips of *Aurelia* —now passing freely, now stopped by an excess of resistance, and now again forcing a passage,—I have felt that I was probably witnessing, on the lowest plain of nervo-genesis, that very same play and counter-

play of forces, which, on the highest plain of nervo-genesis, invariably accompanies, if it does not actually cause, the most intricate reasoning of a Newton, the most sublime emotion of a Shakespeare, the most imperious will of a Napoleon, and the most transforming thought of a Darwin.*

[G. J. R.]

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* Throughout the lecture of which the above is a pretty full abstract, I have associated Mr. Herbert Spencer's theory of nervo-genesis with his name exclusively. To avoid misapprehension, therefore, I append this note to state that I am not ignorant of the fact that the theory in question has occurred to other thinkers as well as to the great English philosopher. Moreover, I am quite aware that even if this theory of nervo-genesis had never been enunciated *à priori* by any speculative thinker, some such theory would certainly have been devised *à posteriori* by any working physiologist of moderate capacity who might first happen to observe such facts as are above detailed. But considering that Mr. Spencer elaborated the theory deductively, and that he did so in a much more thorough and painstaking manner than had ever been done before; considering, too, that he has given the theory so elaborated such a prominent place in his system of objective psychology, I have no hesitation in describing this theory as pre-eminently a product of his authorship.

In now concluding this abstract I desire it to be observed, that having had so much experimental work to accomplish in connection with this research, I have not as yet had time to conduct any systematic investigation concerning the histology of the Medusæ; and that many of the above inferences must therefore be regarded as premature and uncertain so long as this part of the work remains unfinished.—G. J. R.

WEEKLY EVENING MEETING,

Friday, June 1, 1877.

WILLIAM SPOTTISWOODE, Esq. LL.D. Treas. R.S. Secretary and Vice-President, in the Chair.

OSCAR BROWNING, Esq.

The History of Education.

UP to the present time little attention has been paid in England to the science of education. In some respects no country in Europe is better fitted with appliances of instruction. Our universities are possessed of great wealth; our public schools, in their manliness and independence, are objects of admiration and envy to our neighbours; the whole country is covered with a network of educational endowments, old and new, which only require the hand of the reformer to quicken them into more abundant life. The State has taken its share in the education of the people. At the same time the *personnel* of the teaching staff in our great schools is probably unrivalled in Europe. It is true that the elements of which it is composed lack that unity and cohesion which are the conditions of a profession; teachers of various grades know little of, and perhaps care less for, the efforts of their fellow labourers in the same field. But this defect is in a fair way of being remedied; and whenever the profession of teaching is invested with the responsibilities and safeguards which belong to other learned professions, it will be second to none of them in ability and numbers. In the face of this rapid and momentous progress, it becomes of the utmost importance that theory should accompany practice, and that teachers who claim to rank with lawyers and doctors, should not commence their operations on human minds with an entire ignorance of the principles of their art. This is not a time to show how ignorance of the principles of education is one of the greatest hindrances to practical educational reform. The want of such knowledge is beginning to be felt among teachers themselves; it is recognized outside their body; chairs of education have been founded at the Scotch universities; instruction in the science is attainable in London; our English universities are being pressed to follow in the same track, and there is a fair hope that after another ten years have elapsed no master will be appointed to a large school who is not acquainted with what the best minds have thought upon the occupation of his life, and who has not undergone a formal apprenticeship for his work. In this respect we are far behind the Continent. In France, Belgium, Switzerland, Italy, pædagogics or the science of education is recognized as a definite subject of serious study. In Germany the science has assumed proportions which those who have not investigated the matter would find difficult to believe. What then is the science of education to which our neighbours are so devoted, and of which we are so ignorant? It consists mainly of two parts; first, an exposition of the general aims and principles of education, the means by which children may best be kept in order and induced to learn, and by which their characters and faculties may be most successfully developed: secondly, it comprises a history of education and of educational theories, a view of the education which has been given in different ages of the world, of the principles laid down by educational philosophers, of the lives and labours of practical teachers, showing what they were able to accomplish in their lives, and still more, what they left to be accomplished by their successors in happier times and under more toward circumstances. It is to this history of education that I propose to direct your attention this evening. I shall attempt to give a very hasty sketch of the main currents of education which have existed in the world from the earliest times to the present day. My purpose will have been fully answered if I succeed in rousing your interest to pursue the subject farther for yourselves.

The earliest education is that of the family. The child must be trained not to interfere with its parents' convenience, and to acquire those little arts which will help in maintaining the economy of the household. It was long before any attempt was made to improve generations as they succeeded each other. The earliest schools were those of the priests. As soon as an educated priesthood had taken the place of the divines and jugglers who abused the credulity of the earliest races, schools of the prophets became a necessity. The training required for ceremonials, the common life apart from the family, the accomplishments of reading and singing, afforded a nucleus for the organization of culture, and an opportunity for the efforts of a philosopher in advance of his age. Convenience and gratitude confirmed the monopoly of the clergy. The schools of Judæa and Egypt were ecclesiastical. The first had but little effect on the progress of science, but our obligations to the priests of the Nile valley are great indeed. Much of their learning is obscure to us; but we have reason to conclude that there is no branch of science which they did not attempt, and in which they did not progress at least so far as observation and careful registration of facts could carry them. They were a source of enlightenment to surrounding nations. Not only the great lawgiver of the Jews, but those who were most active in stimulating the nascent energies of Hellas, were careful to train themselves in the wisdom of the Egyptians. Greece, in giving an undying name to the literature of Alexandria, was only repaying the debt which she had incurred centuries before. Education became secular in countries where the priesthood did not exist as a separate body. At Rome, until Greece took the conqueror captive, a child was trained for the duties of life in the forum and the senate house. The Greeks were the first to develop a science of education distinct from ecclesiastical training. They divided their subjects of study into music and gymnastics, the one comprising all mental, the other all physical training. Music was at first little more than the study of the art of expression. But the range of intellectual education which had been developed by distinguished musical teachers was thus further widened by the Sophists, until it received a new stimulus and direction from the life and work of Socrates. Who can forget the picture left us by Plato of the Athenian palæstra in which Socrates was sure to find his most ready listeners and his most ardent disciples? In the intervals of running, wrestling, or the bath, the young Phædrus or Theætetus discoursed with the philosophers who had come to watch them, on the good, the beautiful, and the true. The lowest efforts of their teachers were to train them to maintain any view which they might adopt, with acuteness, eloquence, readiness, and good taste. Their highest efforts were to stimulate a craving for the knowledge of the unknowable, to raise a dissatisfaction with received opinions, and to excite a curiosity which grew stronger with the revelation of each successive mystery. Plato is the author of the first systematic treatise upon education. His proposal to entrust education to the State, has not been without imitators in our own day. If we are ever governed by the rules of the positive philosophy, a large portion of the best intellect of the age will be officially dedicated to the training of our youth. Plato lays great stress on the influence of race and blood; strong and worthy children are likely to spring from steady and worthy parents. Music and gymnastics are to develop the emotions of the young men during their earliest years, the one to strengthen their characters for the contest of life, the other to excite in them varying feelings of resentment or tenderness. Reverence, the ornament of youth, is to be called forth by well-chosen fictions; a long and rigid training in science is to precede discussion on more mature subjects. At length the goal is reached, and the ripest wisdom is ready to be applied to the most important practice. The precepts of the Republic existed in theory alone, and they differ in some respects from the rules which are expressed in the earlier dialogues. But the same spirit underlies his whole teaching. He never forgets that the beautiful is undistinguishable from the true, that the mind is best fitted to solve difficult problems which has been trained by the enthusiastic contemplation of art.

The object of the treatise of Quintilian is to show us how to form that much-abused complex, the practical man. But what a high conception of practice is his! He did not write for school-boards or competitive examinations, but for a race of rulers. It is unfortunate that the Romans understood no training except in oratory. In their eyes every citizen was a born commander, and they knew of no science of government and political economy. Cicero speaks slightingly even of jurisprudence. Anyone, he says, can make himself a jurisconsult in a week, but an orator is the production of a lifetime. No statement can be less true than that a perfect orator is a perfect man. But wisdom and philanthropy broke even through that barrier, and the training which Quintilian expounds to us as intended only for the public speaker, would in the language of Milton fit a man to perform justly, wisely, and magnanimously all the offices, both public and private, of peace and war.

Such are the ideas which the old world has left us. On one side, man beautiful, active, clever, receptive, emotional, quick to feel, to show his feeling, to argue, to refine; greedy of the pleasures of the world, perhaps a little neglectful of its duties, fearing restraint as an unjust stinting of the beauty of nature, inquiring eagerly into every secret, strongly attached to the things of this life, but elevated by an unabated striving after the highest ideal; setting no value but upon faultless abstractions, and seeing reality only in heaven, on earth mere shadows, phantoms, and copies of the unseen. On the other side, man practical, energetic, eloquent; tinged but not imbued with philosophy, trained to spare neither himself nor others, reading and thinking only with an apology, best engaged in defending a political principle, in maintaining with gravity and solemnity the conservation of ancient freedom, in leading armies through unexplored deserts, establishing roads, fortresses, settlements, the results of conquest, or in ordering and superintending the slow, certain, and utter annihilation of some enemy of Rome. Has the Christian world ever surpassed these types? Can we produce anything by education in modern times, except by combining, blending, or modifying the self-culture of the Greek or the self-sacrifice of the Roman?

The education of the middle ages was either that of the cloister or the castle. They stood in sharp contrast to each other. The object of one was to form the young monk, of the other the young knight. We should indeed be ungrateful if we forgot the services of those illustrious monasteries, Monte Cassino, Fulda, or Tours, which kept alive the path of learning throughout the dark ages, but it would be equally mistaken to attach an exaggerated value to the education of that age. Long hours were spent in the duties of the church, and in learning to take a part in elaborate and useless ceremonies. A most important department of the monastery was the writing-room, where missals, psalters, and breviaries were copied and illuminated, and too often a masterpiece of classical literature was effaced to make room for a treatise of one of the fathers or the Lenten sermons of an abbot. The discipline was harsh; the rod ruled all with indiscriminating and impartial severity. How many generations have had to suffer for the floggings of those times! Hatred of learning, antagonism between the teacher and the taught, the belief that no training could be effectual which is not repulsive and distasteful, that no subject is proper for instruction which is acquired with ease and pleasure,—all these idols of false education have their root and origin in monkish cruelty. The joy of human life would have been in danger of being stamped out if it had not been for the warmth and colour of a young knight's boyhood. He was equally well broken in to obedience and hardship, but the obedience was the willing service to a mistress whom he loved, and the hardship the permission to share the dangers of a leader whom he emulated. The seven arts of monkish and scholastic training were grammar, dialectic, rhetoric, music, arithmetic, geometry, astronomy, which together formed the *Trivium* and *Quadrivium*, the seven years' course which so profoundly affected our modern education. The seven knightly accomplishments were to ride, to swim, to shoot with the bow, to box, to hawk, to play chess, and to make verses. The verses thus made were not in Latin—bald imitations of Ovid or Horace, whose pagan beauties were wrested into the service of religion—but sonnets, ballads, and canzonets in soft Provençal or melodious Italian. In nothing, perhaps, is the difference between these two forms of education more clearly shown than in their relations to the sex who must always be the instructors of our childhood, and who seem likely to have an increasingly greater influence in raising the standard of education in future ages. A young monk was brought up to regard a woman as the worst among the many temptations of St. Antony. The page was trained to receive his best reward and worst punishment from the smile or frown of the lady of the castle, and as he grew to manhood to regard an absorbing passion as the strongest stimulus to a noble life, and the contemplation of female virtue as embodied in an Isolde or a Beatrice as the truest earnest of future immortality.

Both these forms of education disappeared before the Renaissance and the Reformation. But we must not suppose that no efforts were made to improve upon the narrowness of the schoolmen or the idleness of chivalry. Perhaps the most distinguished place among these precursors of a higher culture belongs to the Brethren of the Common Life, who were domiciled in the rich meadows of the Yssel, in the Northern Netherlands. The metropolis of their organization was Deventer, the best-known name among them that of Gerhard Groote. They devoted themselves with all humility and self-sacrifice to the education of children. Their schools were crowded. Bois-le-duc numbered 120 pupils, Zwolle 1500. For a hundred years no part of Europe shone with a brighter lustre. As the Divine Comedy represents for us the learning and piety of the middle ages in Italy, so the Imitation of Thomas-à-Kempis keeps alive for us the memory of the purity and sweetness of the Dutch community. They could not support the glare of the new Italian learning; they obtained, and I am afraid deserved, the title of obscurantists; and the wittiest squib of the middle ages, which was so true and subtle in its satire that it was hailed as a blow struck in defence of the ancient learning, consists of the lamentations of the brothers of Deventer over the new age which they could not either comprehend or withstand.

The education of the Renaissance is best represented by the name of Erasmus, that of the Reformation by the names of Luther and Melanchthon. Erasmus has left us the most minute account of his method of teaching. The child is to be formed into a good Greek and Latin scholar, and a pious man. But he fully grasps the truth that improvement must be natural and gradual. Letters are to be taught playing. The rules of grammar are to be few and short. Every means of arousing interest in the matter of the books is to be fully employed. Erasmus is no Ciceronian. Latin is to be taught so as to be of use, a living language adapted to modern wants. Children should learn an art—painting, sculpture, or architecture. Idleness is, above all things, to be avoided. The education of girls is as necessary and important as that of boys. Much depends upon home influence. Obedience must be strict, but not too severe. We must take account of individual peculiarities, and not force children into cloisters against their will; we shall obtain the best result by following nature. It is easy to see what a contrast this scheme presented to the monkish training, to the routine of useless technicalities enforced amidst the shouts of teachers and the lamentations of the taught.

Still this culture was but for the few. Luther brought the schoolmaster into the cottage, and laid the foundations of the system which is the chief honour and strength of modern Germany—a system by which the child of the humblest peasant may obtain by slow but certain gradations the best education which the country can afford. The precepts of Luther found a way to the hearts of his countrymen in short pithy sentences, like the sayings of poor Richard. Melanchthon, from his editions of school-books and his practical labours in education, earned the title of Præceptor Germaniæ. Aristotle had been dethroned from his pre-eminence in the schools, and Melanchthon attempted to supply his place. He appreciated the importance of Greek, the terror of the obscurantists; he wrote books on each department of the *Trivium*, grammar, dialectic, and rhetoric; he even attempted to deal with the *Quadrivium*, and wrote a primer of physical science. The schools of the Reformation seldom included in their curricula more than the studies of the three years' course. In nothing is the historical method of inquiry more fruitful than in education, if we wish to ascertain the real reasons for our existing arrangements. We often attempt to defend our existing institutions by philosophical arguments as the results of conscious wisdom. We do not care to ascertain that mathematics hold a subordinate place in our great schools because they were not studied in the dark ages, and because Aristotle did not write a treatise upon them.

We now come to the names of two theoretical and practical teachers who have exercised, and are still exercising, a profound effect over education. John Sturm, of Strassburg, was the friend of

Ascham, the author of the 'Scholemaster,' and the tutor of Queen Elizabeth. It was Ascham who found Lady Jane Grey alone in the library at Bradgate bending her neck over the page of Plato when all the rest of the company were following the chase. Sturm was the first great head-master, the progenitor of Busbys, if not of Arnolds. He lived and worked till the age of eighty-two. He was a friend of all the most distinguished men of the age, the chosen representative of the Protestant cause in Europe, the ambassador to foreign powers. He was believed to be better informed than any man of his time of the complications of foreign politics. Rarely did an envoy pass from France to Germany without turning aside to profit by his experience. But the chief energies of his life were devoted to teaching. He drew his scholars from the whole of Europe. Portugal, Poland, England, sent their contingent to his halls. In 1578 his school numbered several thousand students; he supplied at once the place of the cloister and the castle. What he most insisted upon was the teaching of Latin, not the conversational "*lingua franca*" of Erasmus, but pure, elegant Ciceronian Latinity. He may be called the introducer of "scholarship" into the schools, a scholarship which as yet took no account of Greek. His pupils would write elegant letters, deliver elegant Latin speeches, be familiar, if not with the thoughts, at least with the language of the ancients, would be scholars in order that they might be gentlemen. This is not the occasion to trace the course of his influence, but he is as much answerable as anyone for the euphuistic refinements which overspread Europe in the sixteenth century, and which went far to ruin and corrupt its literatures. Nowhere, perhaps, had he more effect than in England; our older public schools, on breaking with the ancient faith, looked to Sturm as their model of Protestant education; and that which was most characteristic in his methods remained essentially unchanged in them till within the memory of the present generation.

John Amos Comenius was the antithesis to Sturm. Born a poor Moravian, he passed a wandering life in poverty and obscurity, but his ideas were accepted by the most advanced thinkers of his age, notably by our own Milton; his school-books were spread throughout Europe; his works are constantly reprinted at the present day, and the system which he sketched will be found to foreshadow the education of the future. He was repelled and disgusted by the long delays and pedantries of the schools. His ardent mind conceived that if teachers would but follow nature, assist the mind in its natural development instead of forcing it against its bent, take full advantage of the innate desire for activity and growth, all men might be able to learn all things. Languages should be taught, as the mother tongue is taught, by conversations on ordinary topics; pictures, object lessons, should be freely used; teaching should never be unpleasant to the pupil; improvement should go hand in hand with a cheerful, elegant, and happy life. Comenius included in his course the teaching of the mother tongue, singing, economy, and politics, the history of the world, physical geography, and a knowledge of arts and handicrafts. Thus he was one of the first advocates of the teaching of science in schools. His kindness, gentleness, and sympathy made him the forerunner of Pestalozzi. His general principles of education would not sound strange in the treatise of Herbert Spencer.

The Protestant schools were now the best in Europe, and the monkish institutions were left to the rats which tenanted them. Catholics would have remained behind in the race if it had not been for the Jesuits. Ignatius Loyola gave this direction to the order which he founded, and his programme is in use, with certain modifications, in English Jesuit schools at the present day. In 1550 the first Jesuit school was opened in Germany; in 1700 the order possessed 612 colleges, 157 normal schools, 59 noviciates, 340 residences, 200 missions, 29 professed houses, and 24 universities. The college of Clermont had 3000 students in 1695. It would be unfair to deny the success of the education of the Jesuits. They were probably the first to bring the teacher into close connection with the taught. According to their ideal, the teacher was neither enclosed in a cloister secluded from his pupils, nor did he keep order by stamping, raving, and flogging. He was encouraged to apply his mind and soul to the mind and soul of his pupil, to study the nature, the disposition, and the parents of his scholars, to follow nature as far as possible, or rather to lie in wait for it, and discover its weak points, and where it could be most easily attacked. Doubtless the Jesuits have shown a love, devotion, and self-sacrifice in education which is worthy of the highest praise; no teacher who would compete with them can dare do less. On the other hand, they are open to grave accusation; their watchful care degenerated into surveillance, which lay schools have borrowed from them; their study of nature has led them to confession and direction. They have tracked out the soul to its recesses that they might slay it there and plant another in its place; they educated each mind according to its powers, that it might be a more subservient tool to their own purposes. They taught the accomplishments which the world loves, but their chief object was to amuse the mind and stifle inquiry; they encouraged Latin verses because they were a convenient plaything, on which powers might be exercised which could better have been employed in understanding and discussing higher subjects; they were the patrons of school-plays, of public prizes, declamations, examinations, and other exhibitions in which the parents were more considered than the boys; they regarded the claims of education not as a desire to be encouraged, but as a demand to be played with and propitiated; they gave the best education of their time in order to acquire confidence, but they became the chief obstacle to the improvement of education; they did not care for enlightenment, but only for the influence which they could derive from a supposed regard for enlightenment. Governments have done well on the whole in checking and suppressing the Colleges of Jesuits. They are not so much to be feared now-a-days; their plan of education is antiquated and difficult to alter. Till 1820 they taught philosophy out of Aristotle and Thomas Aquinas; even now they discourage the use of the vernacular.

It may be imagined that by this organization both Catholic and Protestant were apt to degenerate into pedantry both in name and purpose. The schoolmaster had a great deal too much the best of it. The Latin school was tabulated and organized until every half-hour of a boy's time was occupied; the Jesuit school took possession of the pupil, body and soul. It was therefore to be expected that a stand should be made for common sense in the direction of practice instead of theory, of wisdom instead of learning. Montaigne has left us the most delightful utterances about education. He says that the faults of modern education lie in over-estimating the intellect and rejecting morality, in exaggerating memory and depreciating useful knowledge. He recommends a tutor who is to draw out the pupil's own power and originality, to teach how to live well and to die well, to enforce a lesson by practice, to put the mother tongue before foreign tongues, to teach all manly exercises, to educate the perfect man. Away with force and compulsion, with severity and the rod!

John Locke, more than a hundred years afterwards, made a more powerful and systematic attack upon useless knowledge. His theory of the origin of ideas led him to assign great importance to education, while his knowledge of the operations of the mind lends a special value to his advice. His treatise is so well known that I need say little about it. Part of his advice is useless now, part it would be well to accept, especially his condemnation of repetition by heart and Latin themes and verses. He also sets before himself the production of the man—a sound mind in a sound body. His knowledge of medicine gives great value to his advice on the earliest education. He recommends home education without harshness or severity. Emulation is to be the chief spring of action. He considers knowledge far less valuable than a well-trained mind. He prizes that knowledge most which fits a man for the duties of the world—speaking languages, accounts, history, law, logic, rhetoric, natural philosophy. He inculcates the importance of drawing, music, dancing, riding, fencing, and trades. He is a strong advocate for home education under a private tutor, and his bitterness against public schools is as violent as that of Cowper.

Far more important in my opinion than the treatise of Locke is the 'Tractate of Education' by Milton, "the few observations," as he tells us, "which have flowered off, and are as it were the burnishing of many studious and contemplative years spent in the search of religious and civil knowledge." "I will point ye out," he says, "the right path of a virtuous and noble education, laborious indeed at first ascent, but else so smooth and green and full of godly prospect and melodious sounds on every side that the harp of Orpheus is not more charming." This is to be done between twelve and one-and-twenty, in an academy containing about a hundred and thirty scholars, which shall be at once school and university, "not needing a remove to any other house of scholarship, except it be some peculiar college of law and physics where they mean to be practitioners." The learning of things and words is to go hand in hand. The curriculum is very large. Latin, Greek, arithmetic, geometry, agriculture, geography, physiology, physics, trigonometry, fortification, architecture, engineering, navigation, anatomy, medicine, poetry, ethics, Italian, law, both Roman and English, Hebrew with Chaldee and Syriac, history, oratory, poetics. But the scholars are not to be bookworms. They are to be trained for war both on foot and on horseback, to be "practised in all the locks and gripes of wrastling." they are to "recreate and compare their travailed spirits with the divine harmonies of music heard or learnt." "In those vernal seasons of the year, when the air is calm and pleasant, it were an injury and a sullenness against nature not to go out and see her riches, and partake in her rejoicing with heaven and earth. I should not then be a persuader to them of studying much then after two or three years that they have well laid their grounds, but to ride out in companies with prudent and staid guides to all the quarters of the land." There is more wisdom in these few pages about education than in any other treatise of similar length upon the subject with which I am acquainted. Visionary as it appears at first sight, if translated into the language of our day, it will be found to be full of sound, practical advice. "Only," as Milton says in conclusion, "I believe that this is not a bow for every man to shoot in who counts himself a teacher, but will require sinews almost equal to those which Homer gave Ulysses; yet I am persuaded that it may prove much more easy in the essay than it now seems at a distance, and much more illustrious if God have so decided, and this age have spirit and capacity enough to apprehend."

While Milton was writing he might have seen an attempt to realize his ideal in Port Royal. What a charm does this name awaken! Yet how few of us have made a pilgrimage to that secluded valley. Here we find for the first time in the modern world the highest gifts of the greatest men of a country applied to the promotion of education. Arnauld, Lancelot, Nicole, Hamon did not commence by being educational philosophers; they began with a small school, and developed their method as they proceeded. Their success has seldom been surpassed. But a more lasting memorial than their pupils are the books which they sent out, and which bear the name of their cloister. The Port-Royal Logic, General Grammar, Greek, Latin, Italian, and Spanish Grammars, the Garden of Greek Roots put into French verse, which taught Greek to Gibbon, the Port-Royal Geometry, and the translations of the classics, held the first place among school-books for more than a century. The success of the Jansenists was too much for the jealousy of the Jesuits. Neither piety, nor wit, nor virtue could save them. A light was quenched which would have given an entirely different direction to the education of France and of Europe. No one can visit without emotion that retired nook which lies hidden among the forests of Versailles, where the old brick dove-cot, the pillars of the church, the trees of the desert alone remain to speak to us of Pascal, Racine, and the

Mère Angelique. The principles of Port Royal found worse supporters in a later time in the better days of French education before monarchism and militarism had sucked the life out of the nation. Rollin is never mentioned without the epithet *bon*, a testimony to his wisdom, virtue, and simplicity. Fénélon may be reckoned as belonging to the same school, but he was more fitted to mix and grapple with mankind. We must pass rapidly over succeeding years until we come to the book which has had more influence than any other on the education of the last century.

The 'Emile' of Rousseau was published in 1762, just a hundred years before the appointment of the first Public Schools Commission. It produced an astounding effect throughout Europe. Those were days when the whole cultivated world vibrated to any touch of new philosophy. French had superseded Latin as the general medium of thought. French learning stood in the same relation to the rest of Europe as German learning does now, and any discovery of D'Alembert, Rousseau, or Maupertuis travelled with inconceivable speed from Versailles to Schönbrunn, from the Spree to the Neva. Kant, in his distant home of Königsberg, for one day broke through his habits, more regular than the town clock, and stayed at home to study the new revelation. Those were happy days for philosophers and philanthropists; the enthusiasm of discovery and hope had not been checked by the disappointments of experience. The light of reason had not been quenched in blood. The burthen of Rousseau's message was nature, such a nature as never did and never will exist, but still a name for an ideal worthy of our struggles. He revolted against the false civilization which he saw around him; he was penetrated with sorrow at the shams of government and society, at the misery of the poor existing side by side with the heartlessness of the rich. The child should be the pupil of nature: he lays the greatest stress on the earliest education; the first year of life is in every respect the most important. Nature must be closely followed. The child's tears are petitions which should be granted. The naughtiness of children comes from weakness; make the child strong and he will be good. Children's destructiveness is a form of activity. Do not be too anxious to make children talk; be satisfied with a small vocabulary. Lay aside all padded caps and baby jumpers. Let children learn to walk, by learning that it hurts them to fall. Do not insist so much on the duty of obedience as on the necessity of submission to natural laws. Do not argue too much with children; educate the heart to wish for right actions; before all things study nature. The chief moral principle is, *do no one harm.* Emile is to be taught by the real things of life, by observation and experience. At twelve years old he is scarcely to know what a book is; to be able to read and write at fifteen is quite enough. We must first make him a man, and that chiefly by athletic exercises. Educate his sight to measure, count, and weigh accurately; teach him to draw; tune his ear to time and harmony; give him simple food, but let him eat as much as he likes. Thus at twelve years old Emile is a real child of nature. His carriage and bearing are firm and confident, his nature open and candid, his speech simple and to the point; his ideas are few, but clear; he knows nothing by learning, much by experience. He has read deeply in the book of nature. His mind is not on his tongue, but in his head. He speaks only one language, but knows what he is saying, and can do what he cannot describe. Routine and custom are unknown to him; authority and example affect him not; he does what he thinks right. He understands nothing of duty and obedience, but he will do what you ask him, and will expect a similar service from you. His strength and body are fully developed; he is first-rate at running, jumping, and judging distances. Should he die at this age, he will so far have lived his life. From twelve to fifteen Emile's practical education is to continue. He is still to avoid books, which teach not learning itself but to appear learned. He is to be taught and to practise some handicraft. Half the value of education is to waste time wisely, to tide over dangerous years with safety, until the character is better able to stand temptation. At fifteen a new epoch commences. The passions are awakened. The care of the teacher should now redouble; he should never leave the helm. Emile, having gradually acquired the love of himself and of those immediately about him, will begin to love his kind. Now is the time to teach him history and the machinery of society; the world as it is and as it might be. Still an encumbrance of useless and burdensome knowledge is to be avoided. Between this age and manhood Emile learns all that it is necessary for him to know.

It is perhaps strange that a book in many respects so wild and fantastic should have produced so great a practical effect. In pursuance of its precepts children went about naked, were not suffered to read, and when they grew up wore the simplest clothes, and cared for little learning except the study of nature and Plutarch. The catastrophe of the French Revolution has made the effect of 'Emile' less apparent to us. Much of the heroism of that time is doubtless due to the exaltation produced by a sweeping away of abuses and the approach of a brighter age. But we must not forget that the first generation of Emiles was just thirty years old in 1792, that many of the Girondists, the Marseillais, the soldiers and generals of Carnot and Napoleon had been bred in that hardy school. There is no more interesting chapter in the history of education than to trace back epochs of special activity to the obscure source from which they arose. Thus the Whigs of the Reform Bill sprang from the wits of Edinburgh, the heroes of the Rebellion from the divines who translated the Bible, the martyrs of the Revolution from the philosophers of the Encyclopædia.

The teaching of Rousseau found its practical expression in the *philanthropin* of Dessau, a school founded by Basedow, the friend of Goethe and Lavater, one of the prophets between whom the world-child sat bodkin. The principles of the teaching were very much those of Comenius, the combination of words and things. An amusing account of the instruction given in this school, which at the time consisted of only thirteen pupils, has come down to us. I am indebted for the translation to the excellent work of Mr. Quick on Educational Reformers. The little ones have gone through the oddest performances. They play at "word of command." Eight or ten stand in a line like soldiers, and Herr Wolke is officer. He gives the word in Latin, and they must do whatever he says. For instance, when he says Claudite oculos, they all shut their eyes; when he says Circumspicite, they look about them; Imitamini sartorem, they all sew like tailors; Imitamini sutorem, they draw the waxed thread like cobblers. Herr Wolke gives a thousand different commands in the drollest fashion. Another game, "the hiding game," I will also teach you. Some one writes a name, and hides it from the children; the name of some part of the body, or of a plant, or animal, or metal, and the children guess what it is. Whoever guesses right, gets an apple or a piece of cake. One of the visitors wrote Intestina, and told the children it was a part of the body. Then the guessing began: one guessed caput, another nasus, another os, another manus, pes, digiti, pectus, and so forth, for a long time; but one of them hits it at last. Next Herr Wolke wrote the name of a beast, a quadruped. Then came the guesses: leo, ursus, camelus, elephas, and so on, till one guessed right—it was mus. Then a town was written, and they guessed Lisbon, Madrid, Paris, London, till a child was with St. Petersburg. They had another game, which was this. Herr Wolke gave the command in Latin, and they imitated the noises of different animals, and made us laugh till we were tired. They roared like lions, crowed like cocks, mewed like cats, just as they were bid. Yet Kant found a great deal to praise in this school, and spoke of its influence as one of the best hopes of the future, and as "the only school where the teachers had liberty to work according to their own methods and schemes, and where they were in free communication both among themselves and with all learned men throughout Germany."

The work of Rousseau is the point of departure for an awakened interest in educational theories which has continued unto the present day. Few thinkers of eminence during the latter part of the last and the beginning of the present century have not written more or less directly on this subject. Poets, like Richter, Herder, and Goethe; philosophers, such as Kant, Fichte, Hegel, Schleiermacher, and even Schopenhauer; psychologists, such as Herbart and Beneke, have left directions for our guidance. Jean Paul called his book Levana, after the Roman goddess to whom the father dedicated his new-born child, in token that he intended to rear it to manhood. The second part of Wilhelm Meister is in the main a treatise upon education. Many of those whom I address must remember the mysteries of the pædagogic province, the solemn gestures of the three reverences, the long cloisters which contain the history of God's dealings with the human race, a mine of wisdom for schoolmasters, unexhausted, and I fear, alas! unworked. Perhaps the more characteristic passage is that which describes the father's return to the country of education after a year's absence. As he is riding alone, wondering in what guise he will meet his son, a multitude of horses rush by at full gallop. The monstrous hurly-burly whirls past the wanderer, a fair boy among the keepers looks at him with surprise, pulls in, leaps down, and embraces his father. He then learns that an agricultural life had not suited his son, that the superiors had found that he was fond of animals, and had set him to that occupation for which nature had destined him.

I must pass over the names of Pestalozzi and Jacotot, of whom you will find a good account in Mr. Quick's book, and I must confine myself to mentioning the name of James Mill, whose treatise on education deserves to be reprinted. The last English writers on the subject are Mr. Herbert Spencer and Mr. Alexander Bain, the study of whose works will land us in those regions of pædagogics which have been most recently explored. The point most insisted upon by Mr. Spencer is that we shall obtain the best results in education by closely studying the development of the mind, and availing ourselves of the whole amount of force which nature puts at our disposal. The mind of every being is naturally active and desirous of exercise; indeed it is never at rest. But for its healthy exercise and growth it must have something to work upon, and therefore the teacher must watch its workings with the most sympathetic care in order to supply exactly that food which it requires at that particular time. In this way a much larger cycle of attainments can be compassed than by the adoption of any programme or curriculum, however carefully drawn up. It is no good teaching what is not remembered; the strength of memory depends upon attention, and attention depends upon interest. To teach without interest is to work like Sisyphus and the Danaides. Arouse interest, if you can, rather by high means than by low means. But it is a saving of power to make use of the interest which you have already existing, and which, unless dried up or distorted by injudicious violence, would naturally lead the mind into all the knowledge which it is capable of receiving. Therefore never from the first force a child's attention; leave off a study the moment it becomes wearisome; never let the child do what it does not like; only take care that when its liking is in activity, a choice of good as well as evil should be given to it.

Mr. Bain's writings on education, which are extremely valuable, are chiefly concerned with what may be called the "*correlation of forces in man.*" He shows how emotion may be transformed into intellect, and how sensation may exhaust the brain as much as thought, and he lays down as the whole duty of a schoolmaster to stimulate the powers of each brain under his charge to the fullest activity, and to apportion them in that ratio which will best conduce to the most complete and harmonious development of the individual.

I have now come to the end of what has been of necessity a very imperfect sketch of the subject on which I proposed to discourse to you. I have been obliged to leave many important matters entirely out. I have said nothing about that magnificent system of public education in modern Germany, which must be closely studied by any nation that wishes to solve the same problem for itself. But I have said enough to make it obvious to all who hear me, that in spite of the great advances which have been made of late years, the science of education is now far in advance of the art. Our schoolmasters are still spending their best energies in teaching subjects which have been universally condemned by educational reformers for the last two hundred years. The education of every public school is a farrago of rules, principles, and customs derived from every age of teaching, from the most modern to the most remote. The philosophy of education in England may be compared to the terminal moraine of a glacier which receives upon its unsightly heaps every useless burden or defilement which has been rejected by the great central stream in its onward movement, or to the currency of those unfortunate countries who have no coinage of their own, and who pass from hand to hand the disused and depreciated tokens of the more progressive nations of the world. Legislation only too often serves to make the matter worse. It orders new subjects to be taught, but does not forbid the teaching of old subjects which have been proved to be mischievous; it loads the teacher with new duties, but does nothing to train or invigorate him for the task which it imposes. Much may be done by adopting this or that expedient, by holding conferences of teachers, by the inspection of schools, by universities or by government, by following the example of other countries who have been stimulated to improve their system of culture by their consciousness of the inferiority of their material. Still it is plain to me that the science and art of education can never truly be said to exist until it is organized on the model of the sister art of medicine. We must pursue the patient methods of induction by which other sciences have reached the stature of maturity. We must invent some means of registering and tabulating results; we must invent a phraseology and nomenclature which will enable results to be accurately recorded; we must place education in its proper position among the sciences of observation. A philosopher who should succeed in doing this, would be venerated by future ages as the creator of the art of teaching. For the avatar of such a reformer we must look to the present generation of schoolmasters. We must exhort them to keep their minds free from prejudice, and accessible to new ideas; to make themselves well acquainted with the practice of contemporary education at home and abroad; to interchange as fully as possible information about methods and results; to put every new principle to the test of careful examination; and, as part of the necessary training for their profession, in order to correct and enlarge their experience, and to keep before them a high standard of what their labours may effect, we must urge them to study with zeal and enthusiasm the lessons which are recorded in the history of education.

[O. B.]

CHRONOLOGICAL LIST
OF
THEORETICAL AND PRACTICAL TEACHERS.

Socrates	B.C. 468	B.C. 399
Plato	428	327
Aristotle	384	322
Quintilian	A.D. 42	A.D. 118
Alcuin	725	804
Gerhard Groote	1340	1384
Erasmus	1467	1536
Luther	1483	1546
Ignatius Loyola	1491	1556
Melanchthon	1497	1560
John Sturm	1507	1589
Montaigne	1533	1592
John Amos Comenius	1592	1671
Dr. Busby	1606	1695
Milton	1608	1674
Lancelot	1615	1695
Locke	1632	1704
Fénélon	1651	1715
Rollin	1661	1741
Rousseau	1712	1778
Basedow	1723	1790
Pestalozzi	1746	1827
Goethe	1749	1832
Jean Paul Richter	1763	1825
Jacotot	1770	1840
James Mill	1773	1836
Dr. Arnold	1795	1842

TRIVIUM AND QUADRIVIUM.

GRAMM(atica) loquitur, **DIA**(lectica) verba docet, **RHE**(torica) verba colorat;

MUS(ica) canit, **AR**(ithmetica) numerat, **GEO**(metria) ponderat; **AS**(tronomia) colit astra.

THE SEVEN STUDIES OF KNIGHTLY EDUCATION.

PROBITATES hæ sunt: equitare, natare, sagittare, cestibus certare, aucupare, scacis ludere, versificare.

GENERAL MONTHLY MEETING,

Monday, June 4, 1877.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Mrs. Warren De la Rue,
Lord Gordon of Drumcarn, Lord of Appeal,
William Alexander Tooke Hallowes, Esq. M.A.
Charles Rogers Heap, Esq.
Loftus Sidney Long, Esq.
Mrs. John Fletcher Moulton,

were *elected* Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Academia dei Lincei, Rome—Atti, Serie III. Transunti, Vol. I. Fasc. 5. 4to. 1877.
Actuaries, Institute of—Journal, No. 106. 8vo. 1877.
Agricultural Society, Royal—Journal; Second Series, Vol. XIII. Part 1. 8vo. 1877.
American Philosophical Society, Philadelphia—Vol. XV. Vol. XVI. No. 98. 8vo. 1876.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVII. No. 6. 8vo. 1877.
Bavarian Academy of Sciences, Royal—Abhandlungen, Band XII. 3te Abth. 4to. 1876.
Sitzungsberichte, 1876: Heft 3. 8vo. 1876.
Board of Trade—Report on Weights and Measures: Canada. 8vo. 1877.
British Architects, Royal Institute of—Sessional Papers, 1876–7. Nos. 10, 11. 4to.
Carpenter, Dr. W. B. C.B. F.R.S. (the Author)—Mesmerism, Spiritualism, &c. Historically and Scientifically considered. 16to. 1877.
Chemical Society—Journal for May, 1877. 8vo.
Cornwall Polytechnic Society—Forty-fourth Annual Report. 1876. 8vo.
Cornu, M. A. (the Author)—Détermination de la Vitesse de la Lumière d'après des Expériences exécutées en 1874 entre l'Observatoire et Montlhéry. 4to. 1876.
Coutts, John, Esq. (the Author)—Brain and Intellect. 16to. 1877.
Editors—American Journal of Science for May, 1877. 8vo.
Athenæum for May, 1877. 4to.
Chemical News for May, 1877. 4to.
Engineer for May, 1877. fol.
Journal for Applied Science for May, 1877. fol.
Nature for May, 1877. 4to.
Nautical Magazine for May, 1877. 8vo.
Pharmaceutical Journal for May, 1877. 8vo.
Telegraphic Journal for May, 1877. 8vo.
Franklin Institute—Journal, No. 617. 8vo. 1877.

Geological Survey of India—Palæontologia Indica: Ser. X. 2, Ser. XI. 1. fol. 1876.

Memoirs, Vol. XII. Parts 1, 2. 8vo. 1876.

Institution of Civil Engineers—Minutes of Proceedings, Vol. XLVIII. 8vo. 1877.

Kerslake, Thomas, Esq. (the Author)—A Primæval British Metropolis (K 101). 8vo. 1877.

Linnean Society—Proceedings, No. 68. 8vo. 1877.

Manchester Geological Society—Transactions, Vol. XIV. Parts 9, 10. 8vo. 1876–7.

Mann, R. J. M.D. M.R.I. (the Editor)—Natal: a History and Description of the Colony. By Henry Brooks. 8vo. 1876.

Medical and Chirurgical Society, Royal—Proceedings, Vol. VIII. No. 3. 8vo. 1877.

Meteorological Society—Quarterly Journal, No. 22. 8vo. 1877.

Micheli, M. Marc (the Author)—Note sur les Onagrariées du Brésil (from Archives des Sciences, 1874).

Progrès de la Physiologie Végétale, 1874–6. 8vo. 1874–7.

Munich Royal Observatory—Annalen, Band XXI. 8vo. 1876.

Meteorologische und Magnetische. Bebachtungen: 1876. 8vo. 1877.

Norfolk and Norwich Naturalists' Society—Transactions, Vol. II. Part 3. 8vo. 1877.

Philadelphia Academy of Natural Sciences—Proceedings. 1876. 8vo.

Photographic Society—Journal, New Series, Vol. I. No. 8. 8vo. 1877.

Preussische Akademie der Wissenschaften—Monatsberichte: Dec. 1876; Jan. Feb. 1877. 8vo.

Royal Irish Academy—Transactions, Vol. XXVI. Science: Parts 1–5. 4to. 1876. Proceedings: Series II. Vol. II. Nos. 4, 5, 6. 8vo. 1875.

Royal Society of London—Proceedings, No. 179. 8vo. 1877.

Royal Society of Tasmania—Papers and Proceedings for 1875. 8vo. 1876.

St. Petersburg, Académie des Sciences—Bulletins, Tome XXIII. No. 3. 4to. 1877.

Sandys, R. H. (the Author)—"In the Beginning." Part 3. 8vo. 1877.

United States Naval Observatory—Observations in 1874. 4to. 1877.

United Service Institution, Royal—Journal, No. 90. 8vo. 1877.

Van Voorst, M. J.—Laboratory Guide for Agricultural Students: by A. H. Church. 4th ed. 16to. 1877.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, Nov. Dec. 1876; April, 1877.

Yorkshire Archæological Association—Journal, Part 16. 8vo. 1877.

WEEKLY EVENING MEETING,

Friday, June 8, 1877.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

John Tyndall, LL.D. D.C.L. F.R.S.
Professor of Natural Philosophy in the Royal Institution.

Researches on the Deportment and Vital Resistance of Putrefactive and Infective Organisms, from a Physical Point of View.

(This Abstract includes a notice of the discourse given on Jan. 19, 1877.)

Portions of the autumn of 1875, and of the winter and spring of 1875–76, were devoted to the first section of these researches, and on the 21st of January, 1876, its main results were communicated orally to the Royal Institution.* The completed memoir was handed in to the Royal Society on the 6th of April: it is published in vol. 166 of the 'Philosophical Transactions.'

Many of the "closed chambers" employed in the inquiry were submitted on the 21st of January to the inspection of the Members. There had been over fifty of them in all, and several of them had been used more than once. The air in these chambers had been permitted to free itself from floating matter by self-subsidence, no artificial means of cleansing it being employed. Sterilized organic liquids and infusions of the most varied kinds freely exposed to air thus spontaneously purified were found, when tested by the microscope, to remain absolutely free from organisms of all kinds, and equally free from the turbidity, scum, and mould which to the naked eye are the infallible signs of the generation and multiplication of such organisms.

These experiments embraced, among others, the following organic liquids:—urine in its natural condition; infusions of mutton, beef, pork, hay, turnip, sole, haddock, codfish, salmon, turbot, mullet, herring, eel, oyster, whiting, liver, kidney, hare, rabbit, barndoor fowl, pheasant, and grouse.

The number of separate vessels containing these liquids which were exposed to spontaneously purified air amounted to several hundreds, and the consensus of their testimony, in the sense just indicated, was complete.

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* See p. 6 of this volume of the 'Proceedings of the Royal Institution.'

Five minutes' boiling was found in all cases sufficient to sterilize the infusions.

When, after remaining sterile for months, the doors of the chambers were opened so as to admit the uncleansed air of the laboratory, the contact of such air, or, more correctly, of the matter mechanically floating in it, infallibly produced organisms in abundance—sometimes exclusively Bacterial, sometimes exclusively fungoid, and sometimes a combination of both.

Infusions of the substances above referred to were afterwards exposed in succession to air which had been freed of its floating matter by filtration through cotton wool, also to air from which the floating matter had been removed by calcination, and finally to vacua obtained by exhausting as far as possible with an air-pump large receivers which had been previously filled with filtered air.

Boiled for five minutes and exposed to air thus treated, or to vacua thus produced, none of the infusions showed subsequently any alteration of colour or transparency to the naked eye, or to the microscope any manifestation of life.

Thus far I have summed up the results obtained with self-purified air, filtered air, calcined air, and air-pump vacua, the liquids in all cases being exposed in open test-tubes. Small retort-flasks were afterwards resorted to. Charged with the infusions, they were boiled in heated oil or brine, and sealed with exceeding care during ebullition. At the Royal Institution on January 21st, 1876, one hundred and thirty such flasks were submitted to the Members, free alike from putrefaction and from life. They embraced specimens of all the substances above mentioned and some others.

Briefly expressed, then, the evidence furnished by six months' assiduous work during the autumn, winter, and spring of 1875–76, proved conclusively that in the atmospheric conditions then existing in the laboratory of the Royal Institution, not one of the many hundred flasks and tubes experimented on failed to be sterilized by five minutes' boiling, and no countenance was given to the notion that any of these once sterilized infusions possessed the power of spontaneously generating life.

The investigation now submitted to the Members was opened in the summer of 1876 by a series of tentative experiments on turnip infusions, to which were added varying quantities of bruised or pounded cheese. Seven different kinds of cheese were employed, fifty-seven test-tubes being charged with the mixture and exposed to the self-purified air of closed chambers.

The majority of these mixtures remained unchanged; a minority became charged with organisms, which are, in my opinion, completely accounted for by reference to the protective action of the cheese. In the memoir of which this is an abstract such protective action is illustrated by the fact that when ordinary mustard seeds were tied together in a calico bag, they resisted the boiling temperature for a considerable multiple of the time which sufficed to kill them when no bag enveloped them. The bag and outside seeds protected the interior ones.

Not temperature alone, but the ability to diffuse its juices or salts, appears to be a condition of importance in the destruction of the integrity and life of a germ by boiling water. Without diffusion a germ may withstand temperatures competent to destroy it where diffusion is free. I need not remark on the imperviousness of cheese to water, and its consequent power to prevent diffusion.

These summer experiments on turnip-cheese infusions were, however, merely tentative, and I purpose completing them hereafter.

In the autumn I resumed experiments on infusions of hay, which had been purposely postponed. With this substance no difficulty was encountered in my first inquiry. Boiled for five minutes, and exposed to air purified spontaneously, or freed from its floating matter by calcination or filtration, hay infusion, though employed in multiplied experiments at various times, never showed the least competence to kindle into life. After months of transparency, I have, in a great number of cases, inoculated this infusion with minute specks of animal and vegetable liquids containing *Bacteria*, and observed twenty-four hours afterwards its colour lightened and its mass rendered opaque by the multiplication of these organisms.

But in the autumn of 1876, the substance with which I had experimented so easily and successfully a year previously appeared to have changed its nature. The infusions extracted from it bore, in some cases, not only five minutes' but fifteen minutes' boiling with impunity. On changing the hay a different result was often obtained. Many of the infusions extracted from samples of hay purchased in the autumn of 1876 behaved exactly like those extracted from the hay of 1875, being completely sterilized by five minutes' boiling.

The possible influence of age and dryness soon suggested itself, and I tested the surmise to the uttermost. Numerous and laborious experiments were executed with hay derived from different localities; and by this means, in the earlier days of the inquiry, it was revealed that the infusions which manifested this previously unobserved resistance to sterilization were, one and all, extracted from old hay, while the readily sterilized infusions were extracted from new hay, the germs adhering to which had not been subjected to long-continued desiccation.

As the inquiry proceeded the distinction between old and new hay became more and more blurred, while prolonged experiment with hay of various kinds failed to rescue the question from uncertainty. I therefore turned to substances of a succulent nature—to fungi, cucumber, melon, beetroot, and artichoke, for example, whose parasitic or epiphytic germs were unlikely to have suffered desiccation.

Boiled for periods varying from five to fifteen minutes and exposed afterwards to moteless air, in numberless experiments these infusions broke down, charging themselves throughout with organisms, and loading themselves, almost in all cases, with a soapy corrugated scum.

I then fell back upon infusions whose deportment had been previously familiar to me, and in the sterilization of which I had never experienced any difficulty. Fish, flesh, and vegetables were resubjected to trial. Though the precautions taken to avoid contamination were far more stringent than those observed in my first inquiry, and though the period of boiling was sometimes tripled in duration, these infusions, in almost every instance, broke down. Spontaneously purified air, filtered air, and calcined air (calcined, I may add, with far greater severity than was found necessary a year previously) failed, in almost all cases, to protect the infusions from putrefaction.

I was sometimes cheered by a success which, at the time of its occurrence, would seem to be the result of increased severity in the methods of experiment. But the success was subsequently so opposed by failure that it finally stood out rather as an accident than as the normal result of the inquiry.

I had the most implicit confidence in the correctness of my earlier experiments; indeed incorrectness would have led to consequences exactly opposite to those arrived at. Errors of manipulation would have filled my tubes and flasks with organisms, instead of leaving them transparent and void of life. By the unsuccessful experiments above referred to a clear issue was therefore raised:—Either infusions of fish, flesh, and vegetables had become endowed in 1876 with an inherent generative energy which they did not possess in 1875, or some new contagion external to the infusions, and of a far more obstinate character than that of 1875, had been brought to bear upon them at the later date. The scientific mind will not halt in its decision between these two alternatives.

For my own part the gradual but irresistible interaction of thought and experiment made it in the first instance probable, and at last certain, that the atmosphere in which I worked had become so virulently infective as to render utterly impotent precautions against contamination and modes of sterilization which had been found uniformly successful in a less contaminated air. I therefore removed from the laboratory, first to the top, and afterwards to the basement of the Royal Institution, but found that even here, in a multitude of cases, failure was predominant, if not uniform. This hard discipline of defeat was needed to render me acquainted with all the possibilities of infection involved in the construction of my chambers and the treatment of my infusions.

I finally resolved to break away from the Royal Institution, and to seek at a distance from it a less infective atmosphere. In Kew Gardens, thanks to Sir Joseph Hooker, the requisite conditions were found. I chose for exposure in the Jodrell laboratory the special infusions which had proved most intractable in the laboratory of the Royal Institution. The result was that liquids which in Albemarle Street resisted two hundred minutes' boiling, becoming afterwards crowded with organisms, were utterly sterilized by five minutes' boiling at Kew.

A second clear issue is thus placed before us:—Either the infusions had lost in Kew Gardens an inherent generative energy which they possessed in our laboratory, or the remarkable instances of life development, after long-continued boiling, observed in the laboratory are to be referred to the contagium of its vessels or of its air.

With a view to making nearer home experiments similar to those executed at Kew, I had a shed erected on the roof of the Royal Institution. In this shed infusions were prepared and introduced into new chambers of burnished tin, which had never been permitted to enter our laboratory. After their introduction the liquids were boiled for five minutes in an oil-bath.

The first experiment in this shed resulted in complete failure. Not one of the infusions exposed to the moteless air of the shed escaped putrefaction.

Either of two causes, or both of them combined, might, from my point of view, have produced this result. First, a flue from the laboratory was in free communication with the atmosphere not far from the shed; secondly, and this was the real cause of the infection, my assistants in preparing the infusions had freely passed between the laboratory to the shed. They had thus incautiously carried the contagium by a mode of transfer known to every physician.

The infected shed was disinfected; the infusions were again prepared, and care was taken by the use of proper clothes, to avoid the former causes of contamination. The result was similar to that obtained at Kew, viz. organic liquids which in the laboratory withstood two hundred minutes' boiling, were rendered permanently barren by five minutes' boiling in the shed.

A third clear issue is thus placed before us, which I should hardly think of formulating, were it not for the incredible confusion which apparently besets this subject in the public mind. A rod thirty feet in length would stretch from the infusions in the shed to the same infusions in the laboratory. At one end of this rod the infusions were sterilized by five minutes' boiling, at the other end they withstood two hundred minutes' boiling. As before, the choice rests between two inferences:—Either we infer that at one end of the rod animal and vegetable infusions possess a generative power which at the other end they do not possess, or we are driven to the conclusion that at the one end of the rod we have infected and at the other end disinfected air.

The second inference is that which will be accepted by the scientific mind. To what, then, is the inferred difference at the two ends of the rod to be ascribed? In one obvious particular the laboratory this year differed from that in which my first experiments were made. On its floor were various bundles of old and desiccated hay, from which, when stirred, clouds of fine dust ascended into the atmosphere. This dust proved to be both fruitful and, in the highest degree, resistant. Prior to the introduction of the hay which produced the dust, no difficulty as regards sterilization had ever been experienced; subsequent to its introduction my difficulties and defeats began.

I have twice glanced at periods of boiling amounting to two hundred minutes; for, after long and laborious trials of shorter periods I advanced to longer ones, subjecting turnip and cucumber infusions to the boiling temperature for intervals varying from five minutes to three hundred and sixty minutes. Up to a certain point these liquids maintained their power of developing life, but beyond this point complete sterility was the result. In the preliminary experiments bearing upon this question the point of sterilization lay between 180 and 240 minutes. Boiled for the former period the infusions continued fruitful; boiled for the latter period they remained permanently barren.

In these and numerous other experiments a method was followed which had been substantially employed by Spallanzani and Needham, and more recently by Wyman and Roberts, the method having been greatly refined by the philosopher last named. The flasks were partially filled with the infusions, the portions unoccupied by the liquids being taken up with ordinary unfiltered air. Now, as regards the death-point of contagia we know that in air it may be much higher than in water, the self-same temperature being fatal in the latter and sensibly harmless in the former; hence my doubt whether, in my recent experiments, the resistance of the contagium did not arise from the fact of its being surrounded, not by water but by air.

I changed the method, and made a long series of experiments with filtered air. They were almost as unsuccessful as those made with ordinary air. From time to time I succeeded in producing complete sterility by five minutes' boiling; but these successes were so checked by failures that, similar to other cases referred to, they appeared in the light of accidents. They were, however, by no means uninstructive, for they revealed the existence of breaks in the prevalence of the contagium, which, under the circumstances, might have been foreseen.

A rapid glance at the means employed to improve the method of experiment, and at the results of their employment, may be permitted here. Bulbs, exhausted by an air-pump and afterwards heated almost to redness, were filled when cool with filtered air. While being charged with the infusions the bulbs were warmed, so as to produce a gentle outflow of air, and their necks were sealed while the outflow continued. It was thus sought to avoid the contamination consequent on an indraught.

The failures resulting from this mode of experiment greatly predominated over the successes.

Employing similar bulbs, their necks in the first instance were drawn out at the ends to tubes of capillary fineness. The bulbs were then filled with one-third of an atmosphere of filtered air, and, while connected with the air-pump, were heated almost to redness. The capillary tubes were then sealed with the lamp; the sealed ends were afterwards broken off in the body of the liquid, two-thirds of each bulb being thus filled with the infusion. By great care it was found possible to reseal the capillary tubes without removing them from the liquid. The infusions were afterwards boiled from five to fifteen minutes.

Here also the fruitfulness of the boiled infusion was the rule, and its barrenness the exception.

One source of discomfort clung persistently to my mind throughout these experiments. I was by no means certain that the observed development of life was not due to germs entangled in the film of liquid adherent to the necks and higher interior surfaces of the bulbs. This film might have evaporated, and its germs, surrounded by air and vapour, instead of by water, might, on this account, have been able to withstand an ordeal to which they would have succumbed if submerged.

A plan was therefore resorted to by which the infusions were driven by atmospheric pressure through lateral channels issuing from the centres of the bulbs. As before, each bulb was filled with one-third of an atmosphere of filtered air, and afterwards heated nearly to redness. When fully charged, the infusion rose higher than the central orifice, and no portion of the internal surface was wetted save that against which the liquid permanently rested. The lateral channel was then closed with a lamp without an instant's contact being permitted to occur between any part of the infusion and the external air. It was thus rendered absolutely certain that the contagia exposed subsequently to the action of heat were to be sought, neither in the superjacent air nor on the interior surfaces of the flasks, but in the body of the infusions themselves.

By this method I tested in the first place the substance which, at an early stage of the inquiry, had excited my suspicion—without reference to which the discrepancy between the behaviour of infusions examined in the winter of 1875–76 and those examined in the winter of 1876–1877 is inexplicable, but by reference to which the explanation of the observed discrepancy is complete; I mean, the old hay which cumbered our laboratory floor.

Four hours' continuous boiling failed to sterilize bulbs charged with infusions of this hay. In special cases, moreover, germs were found so indurated and resistant, that five, six, and in one case even eight hours' boiling failed to deprive them of life.

All the difficulties encountered in this long and laborious inquiry were traced to the germs which exhibited the extraordinary powers of resistance here described. They introduced a plague into our atmosphere—the other infusions, those of fresh hay included, like a smitten population, becoming the victims of a contagium foreign to themselves.*

It is a question of obvious interest to the scientific surgeon whether those powerfully resistent germs are amenable to the ordinary processes of disinfection. It is perfectly certain that they resist to an extraordinary extent the action of heat. They have been proved competent to cause infusions, both animal and vegetable, to putrefy. How would they behave in the wards of an hospital? There are, moreover, establishments devoted to the preserving of meats and vegetables. Do they ever experience inexplicable reverses? I think it certain that the mere shaking of a bunch of desiccated hay in the air of an establishment of this character might render the ordinary process of boiling for a few minutes utterly nugatory, thus possibly entailing serious loss. They have, as will subsequently appear, one great safeguard in the complete purgation of their sealed tins of air.

A remarkable application of the germ theory is now to be mentioned. Keeping these germs and the phases through which they pass to reach the developed organism clearly in view, I have been able to sterilize the most obstinate infusions encountered in this inquiry, by heating them for a small fraction of the time above referred to as *insufficient* to sterilize them. The fully developed *Bacterium* is demonstrably killed by a temperature of 140° F. Fixing the mind's eye upon the germ during its passage from the hard and resistant to the plastic and sensitive state, it will appear in the highest degree probable that the plastic stage will be reached by different germs in different times. Some are more indurated than others, and require a longer immersion to soften and germinate. For all known germs there exists a period of incubation during which they prepare themselves for emergence as the finished organisms which have been proved so sensitive to heat. If during this period, and well within it, the infusion be boiled for even the fraction of a minute, the softened germs which are then approaching their phase of final development will be destroyed. Repeating the process of heating every ten or twelve hours, before the least *sensible* change has occurred in the infusions, each successive heating will destroy the germs then softened, until, after a sufficient number of heatings, the last living germ will disappear.

Guided by the principle here laid down, and applying the heat discontinuously, infusions have been sterilized by an aggregate period of heating, which, fifty times multiplied, would fail to sterilize them if applied continuously. Four minutes in the one case can accomplish what four hours fail to accomplish in the other.

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* A hard and wiry hay from Guildford, which I have no reason to consider old, was found extremely difficult to sterilize.

If properly followed out, the method of sterilization here described is infallible. A temperature, moreover, far below the boiling point suffices for sterilization.[*]

Another mode of sterilization, equally certain and remarkable, was forced upon me, so to speak, in the following way. In a multitude of cases a thick and folded layer of fatty scum, made up of matted *Bacteria*, gathered upon the surfaces of the infusions, the liquid underneath becoming sometimes cloudy throughout, but frequently maintaining a transparency equal to that of distilled water. The living scum-layer, as Pasteur has shown in other cases, appeared to possess the power of completely intercepting the atmospheric oxygen, appropriating the gas and depriving the germs in the liquid underneath of an element necessary to their development.

Placing the infusions in flasks, with large air-spaces above the liquids, corking the flasks, and exposing them for a few days to a temperature of 80° or 90° F., at the end of this time the oxygen of the superjacent air seems completely consumed. A lighted taper plunged into the flask is immediately extinguished. Above the scum, moreover, the interior surfaces of the bulbs used in my experiments were commonly moistened by the water of condensation. Into it the *Bacteria* sometimes rose, forming a kind of gauzy film to a height of an inch or more above the liquid. In fact, wherever air was to be found, the *Bacteria* followed it. It seemed a necessity of their existence. Hence the question, What will occur when the infusions are deprived of air?

I was by no means entitled to rest satisfied with an inference as an answer to this question; for Pasteur, in his masterly researches, has abundantly demonstrated that the process of alcoholic fermentation depends on the continuance of life without air—other organisms than *Torula* being also alleged to be competent to live without oxygen. Experiment alone could determine the effect of exhaustion upon the particular organisms here under review.

Air-pump vacua were first employed, and with a considerable measure of success. Life was demonstrably enfeebled in such vacua.

Sprengel pumps were afterwards used to remove more effectually both the air dissolved in the infusions and that diffused in the spaces above them. The periods of exhaustion varied from one to eight hours, and the results of the experiments may be thus summed up:—Could the air be completely removed from the infusions, there is every reason to believe that sterilization *without boiling* would in most, if not in all, cases be the result. But, passing from probabilities to certainties, it is a proved fact that in numerous cases unboiled infusions deprived of air by five or six hours' action of the Sprengel pump are reduced to permanent barrenness. In a great number of cases, moreover, where the unboiled infusion would have become cloudy, exposure to the boiling temperature for a single minute sufficed completely to destroy the life already on the point of being extinguished through defect of air. With a single exception, I am not sure that any infusion escaped sterilization by five minutes' boiling after it had been deprived of air by the Sprengel pump. These five minutes accomplished what five hours sometimes failed to accomplish in the presence of air.

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[*] On reading my paper, previous to its presentation to the Royal Society, Mr. Huxley suggested a continuous exposure to a temperature not much above the death-temperature of the adult *Bacterium*. I had also noted the experiment for execution myself. It was made some weeks ago with perfect success. The most obstinate infusions, maintained for a few days at a temperature of 160° F., are sterilized.—[June 21.]

The exception here referred to is old-hay infusion, which, though sterilized in less than half the time needed to kill its germs where air is present, maintained a power of developing a feeble but distinct life after having been boiled for a large multiple of the time found sufficient to render infusions of mutton, beef, pork, cucumber, turnip, beetroot, shaddock, and artichoke permanently barren.

These experiments gave me the clue to many others which might have readily become subjects of permanent misinterpretation. In the midst of a most virulently infective atmosphere, where, even after some hours' boiling, there was no escape for infusions supplied with air, the expulsion of the air by less than five minutes' boiling in properly shaped retort-flasks, and the proper sealing of the flasks during ebullition, ensured the sterility of the infusions.

The meaning of a former remark regarding the part played by boiling in establishments devoted to the preserving of meats and vegetables will be now understood.

The inertness of the germs in liquids deprived of air is not due to a mere *suspension* of their powers. The germs are *killed* by being deprived of oxygen. For when the air which has been removed by the Sprengel pump is, after some time, carefully restored to the infusion, unaccompanied by germs from without, there is no revival of life. By removing the air we stifle the life which the returning air is incompetent to restore.

These experiments on the mortality arising from a defect of oxygen are in a certain sense complementary to the beautiful results of M. Paul Bert. Applying his method to my infusions, I find them sterilized in oxygen possessing a pressure of ten atmospheres or more. Like higher organisms, our Bacterial germs are poisoned by the excess and asphyxied by the defect of oxygen. A mechanical action may also come into play.

I hardly think it necessary to summarize what has been here brought before you. In fact, the whole discourse is but a summing up of eight months of incessant labour. From the beginning to the end of the inquiry there is not, as you have seen, a shadow of evidence in favour of the doctrine of spontaneous generation. There is, on the contrary, overwhelming evidence against it; but do not carry away with you the notion sometimes erroneously ascribed to me, that I deem spontaneous generation "impossible," or that I wish to limit the power of matter in relation to life. My views on this subject ought to be well known. But possibility is one thing and proof is another, and when in our day I seek for experimental evidence of the transformation of the non-living into the living, I am led inexorably to the conclusion that no such evidence exists, and that in the lowest, as in the highest of organized creatures, the method of nature is that life shall be the issue of antecedent life.*

[J. T.]

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* Throughout these laborious researches I have been aided, with his accustomed zeal and ability, by my excellent senior assistant, Mr. John Cottrell. He has been worthily seconded by Mr. Frank Valter, and, in a humbler but still effectual way, by William Card.

GENERAL MONTHLY MEETING,

Monday July 2, 1877.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Walter Deeble Boger, Esq.
Henry Alexander Bruce, M.D.

were *elected* Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Rome—Atti, Serie III. Transunti, Vol. I. Fasc. 6. 4to. 1877.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVII. No. 7. 8vo. 1877.
Memoirs, Vol. XL. 4to. 1874.
Bessels, Dr. Emils (the Author)—Scientific Results of the United States Arctic Expedition: Steamer Polaris. Vol. I. Physical Observations. 4to. Washington, 1876.
British Architects, Royal Institute of—Sessional Papers, 1876-7. Nos. 12, 13. 4to.
British Museum Trustees—Illustrations of Lepidoptera Heterocerca. Part 1. 8vo. 1877.
Guide to Exhibition Rooms. 8vo. 1877.
Descriptive Catalogue of Playing and other Cards. 8vo. 1876.
Catalogue of British Fossil Crustacea. 8vo. 1877.
Cave, Mrs. L. T.—Colonel G. Greenwood: River Terraces. 8vo. 1877.
Editors—American Journal of Science for June, 1877. 8vo.
Athenæum for June, 1877. 4to.
Chemical News for June, 1877. 4to.
Engineer for June, 1877. fol.
Horological Journal for June, 1877. 8vo.
Journal for Applied Science for June, 1877. fol.
Nature for June, 1877. 4to.
Nautical Magazine for June, 1877. 8vo.
Pharmaceutical Journal for June, 1877. 8vo.
Telegraphic Journal for June, 1877. 8vo.
Franklin Institute—Journal, No. 618. 8vo. 1877.
Geological Society—Quarterly Journal, No. 130. 8vo. 1877.
Geological Survey of India—Records, Vol. X. Part 2. 8vo. 1877.
Glasgow Philosophical Society—Proceedings, Vol. X. No. 2. 8vo. 1876–7.
Linnean Society—Proceedings, Nos. 69, 89. 8vo. 1877.

New Church Missionary and Tract Society—Rev. Chauncey Giles: The Spiritual World and our Children there. 16to. 1875.

Dr. Theophilus Parsons: Outlines of the Religion and Philosophy of Swedenborg. 16to. 1876.

Rev. Samuel Noble: Appeal on behalf of the Views of the Eternal World and State and the Doctrines of Faith and Life held by the Body of Christians who believe that a New Church is signified (in the Revelation, chap. xxi.) by the New Jerusalem. 9th ed. 12mo. 1876.

Emanuel Swedenborg, the Spiritual Columbus. A Sketch by U. S. E. 2nd ed. 16to. 1877.

Rev. Dr. Bayley: Great Truths on Great Subjects: Six Lectures. 16to. 1877.

Photographic Society—Journal, New Series, Vol. I. No. 9. 8vo. 1877.

Rayleigh, The Lord, M.A. F.R.S. M.R.I. (the Author)—The Theory of Sound, Vol. I. 8vo. 1877.

Royal Society of London—Proceedings, No. 180. 8vo. 1877.

Sims, J. M. M.D. (the Author)—The Discovery of Anæsthesia. (K 101) 8vo. 1877.

Symons, G. J.—Monthly Meteorological Magazine, June, 1877. 8vo.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, Mai, 1877.

Victoria Institute—Transactions, No. 41. 8vo. 1877.

Zoological Society of London—Transactions, Vol. X. Part 1. 4to. 1877.

Proceedings, 1877, Part 1. 8vo. 1877.

GENERAL MONTHLY MEETING,

Monday, November 5, 1877.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Academia dei Lincei, Rome—Atti, Serie Seconda, Vol. III. Parte 1, 2, 3. 4to. 1876.
Atti, Serie III. Transunti, Vol. I. Fasc. 1, 2, 7. 4to. 1877.
Actuaries, Institute of—Journal, Nos. 107, 108. 8vo. 1877.
American Philosophical Society—Proceedings, No. 99. 8vo. 1877.
Antiquaries, Society of—Archæologia, Vol. XLV. Part 1. 4to. 1877.
Proceedings, Second Series, Vol. VII. No. 2. 8vo. 1877.
Asiatic Society, Royal—Journal, New Series, Vol. IX. Part 2. 8vo. 1877.
Asiatic Society, Royal, Bombay Branch—Journal, No. 34. 8vo. 1877.
Asiatic Society of Bengal—Journal, 1876, Part I. No. 3; Part II. No. 4. 1877, Part I. No. 1; Part II. No. 1. 8vo.
Proceedings, 1876, Nos. 9, 10; 1877, Nos. 1–5. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVII. Nos. 8, 9. 8vo. 1877.
Bagster & Co. Messrs. (the Publishers)—W. R. Cooper: Short History of Egyptian Obelisks. 16to. 1877.
Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1877, Heft 1. 8vo. 1877.
Bertier, F. M.D. (the Author)—The Spas of Aix-les-Bains and Marlioz, Savoy. 12mo. 1877.
Boston Society of Natural History—Memoirs, Vol. II. Part 4, No. 5. 4to. 1877.
Proceedings, Vol. XIII. Nos. 24–27; Vol. XIV. Nos. 1–14; Vol. XVIII. Parts 3, 4. 8vo. 1871–6.
Occasional Papers, I. H. T. W. Harris: Entomological Correspondence. 8vo. 1869.
British Association for the Advancement of Science—Report of the 46th Meeting: Glasgow, Sept. 1876. 8vo. 1877.
British Museum Trustees—Catalogue of Chinese Books. 4to. 1877.
Catalogue of Birds. Vol. III. 8vo. 1877.
Facsimiles of Ancient Charters. Part III. fol. 1877.
Buckler, George, Esq. (the Author)—Colchester Castle a Roman Building, &c. 8vo. 1876–7.
Colchester Castle, &c. By Rev. H. Jenkins. 8vo. 1869.
Royal Archæological Institute, Colchester Meeting. 12mo. 1876.
Cambridge Philosophical Society—Transactions, Vol. XI. Part 3; Vol. XII. Parts 1, 2. 4to. 1871–7.
Proceedings, Vol. III. Parts 1, 2. 8vo. 1876–7.
Canada Meteorological Office—Reports. 8vo. 1877.
Chemical Society—Journal, July, Aug. Sept. Oct. 1877. 8vo.

Civil Engineers' Institution—Minutes of Proceedings, Vol. XLIX. 8vo. 1877.

Clermont, the Lord—Supplement to Life and Works of Sir John Fortescue. 4to. 1877.

Dawson, Alfred, Esq. F.R.A.S. (the Author)—English Landscape Art in 1877. (O 17) 12mo. 1877.

Devonshire Association for the Advancement of Literature, Science, and Art—Report and Transactions, Vol. IX. 8vo. 1877.

Editors—American Journal of Science for July–Oct. 1877. 8vo.
Analyst for July–Oct. 1877. 8vo.
Athenæum for Oct. 1877. 4to.
Chemical News for July–Oct. 1877. 4to.
Engineer for July–Oct. 1877. fol.
Horological Journal for July–Oct. 1877. 8vo.
Journal for Applied Science for July–Oct. 1877. fol.
Nature for July–Oct. 1877. 4to.
Nautical Magazine for July–Oct. 1877. 8vo.
Pharmaceutical Journal for July–Oct. 1877. 8vo.
Telegraphic Journal for July–Oct. 1877. 8vo.

Franklin Institute—Journal, Nos. 619–621. 8vo. 1877.

Geographical Society, Royal—Proceedings, Vol. XXI. Nos. 4, 5, 6; and Charter. 8vo. 1877.

Geological Institute, Imperial, Vienna—Jahrbuch, 1877, Nos. 1, 2. 8vo. 1877.
Verhandlungen, 1877, Nos. 1–10. 8vo. 1877.
Abhandlungen, Band VII. 4; IX. 4to. 1877.

Geological Society—Quarterly Journal, No. 131. 8vo. 1877.

Geological Survey of India—Memoirs, Vol. XIII. Parts 1, 2. 8vo. 1877.
Palæontologia Indica, Series II. 2. fol. 1877.
Records, Vol. X. Part 3. 8vo. 1877.

Gynecological Society—Transactions, Vol. I. 8vo. 1877.

Harrison, Mr. W. H. (the Publisher)—Animal Magnetism, or Mesmerism and its Phenomena. By Wm. Gregory, M.D. Second Edition. 8vo. 1877.

Heap, C. Rogers, Esq. M.R.I. (the Author)—The Indian Famine: Water is the Best Remedy. (K 102.) 8vo. 1877.

Hildebrandsson, H. Hildebrand (the Author)—Atlas des Mouvements Supérieurs de l'Atmosphère. (M 8) 4to. 1877.

Hull Literary and Philosophical Society—Annual Report. 8vo. 1876–7.

Iron and Steel Institute—Journal, 1877, No. 1. 8vo. 1877.

Kershaw, S. W. Esq. (the Author)—Notes on Croydon Palace. 8vo. 1877.

Leeds Philosophical Society—Report for 1876–7. 8vo.

Linnean Society—Proceedings, Nos. 70, 71, 73, 90–92. 8vo. 1877.

London Corporation—Catalogue of Library. Fourteenth Supplement. 8vo. 1877.

Lunacy Commissioners—Thirty-first Report. 8vo. 1877.

Manchester Geological Society—Transactions, Vol. XIV. Parts 11, 12, 13. 8vo. 1877.

Mechanical Engineers' Institution—Proceedings, May and July, 1877. 8vo.

Medical and Chirurgical Society, Royal—Proceedings, Part 45. 8vo. 1877.

Meteorological Office—Quarterly Weather Report, 1874, Part 4. 4to. 1877.

Meteorological Society—Quarterly Journal, No. 23. 8vo. 1876.

Musical Association—Proceedings, Third Session, 1876–7. 8vo. 1877.

Nallak, Muley R. Esq. (the Author)—El Rescate de Cervantes. (K 102.) 8vo. 1877

Photographic Society—Journal, New Series, Vol. II. No. 1. 8vo. 1877.

Physical Society of London—Proceedings, Vol. II. Part 3. 8vo. 1877.

Potocki, Albert, Esq. (the Author)—Nosce te ipsum: Etudes d'après Nature. (L 17.) 8vo. 1877.

Preussische Akademie der Wissenschaften — Monatsberichte: Mai, Juni, Juli, 1877. 8vo.

Quaritch, Mr. B. (the Publisher)—General Catalogue of Books: Supplement. 8vo. 1875–7.

Radcliffe Trustees, Oxford—Catalogue of Books on Natural Science in the Radcliffe Library. 4to. 1877.

Royal Society of London—Proceedings, No. 181, 182. 8vo. 1877.

Catalogue of Scientific Papers, Vol. VII. (1864–73). 4to. 1877.

St. Bartholomew's Hospital—Statistical Tables, 1876. 8vo. 1877.

St. Petersburg, Académie des Sciences—Bulletins, Tome XXIII. No. 4. 4to. 1877.

Mémoires. 7e Série. Tome XXII. Nos. 11, 12; Tome XXIII. Nos. 2-8, 1875; Tome XXIV. Nos. 1-3, 1876-7.

Statistical Society—Journal, Vol. XL. Parts 2, 3. 8vo. 1877.

Swedenborg Society—Swedenborg's Works, 37 vols. 8vo. 1845–76.

Symons, G. J.—Monthly Meteorological Magazine, July–Oct. 1877. 8vo.

Trafford, F. W. C.—Amphiorama. New edition. (K 102.) 8vo. 1877

Trinity House Corporation—Correspondence and Reports on Electric Lights at the South Foreland. (P 12.) fol. 1877.

Tyndall, Professor, D.C.L. LL.D. F.R.S. (*the Author*)—On the Action of Rays of High Refrangibility upon Gaseous Matter. (Phil. Trans. 1870.)

On the Optical Deportment of the Atmosphere in relation to the Phenomena of Putrefaction and Infection. (Phil. Trans. 1876.)

Further Researches on the Deportment and Vital Persistence of Putrefactive and Infective Organisms from a Physical Point of View. (Phil. Trans. 1877.)

United Service Institution, Royal—Journal, Appendix to Vol. XX. Nos. 91, 92. 8vo. 1877.

Victoria Institute—Transactions, No. 42. 8vo. 1877.

Vincent, Chas. W. F.R.S.E. F.C.S. (*the Editor*)—Chemistry, Theoretical, Practical, and Analytical, as applied to the Arts and Manufactures. By Writers of Eminence. Division I. Second edition. Division V. 4to. 1877.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen. Juni, 1877. 4to.

Warden of the Standards—Eleventh Annual Report. 8vo. 1877.

Yorkshire Philosophical Society—Communications, 1876. 8vo. 1877.

Zoological Society of London—Transactions, Vol. X. Part 2. 4to. 1877.

Proceedings, 1877, Part 2. 8vo. 1877.

GENERAL MONTHLY MEETING,

Monday, December 3, 1877.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Earl Stanhope,
Mrs. Robert S. Faulconer,
Colonel Henry Macfarlane Norris,
Mrs. Henry Pollock,
J. Lyons Sampson, Esq.
Miss Elizabeth M. Steedman,

were *elected* Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Nautical Almanac for 1881. 8vo. 1877.
The Lords of the Committee of Council on Education—Catalogue of the Special Loan Collection of Scientific Apparatus at the South Kensington Museum. 3rd ed. 8vo. 1877.
Asiatic Society, Royal, Bombay Branch—Journal, Nos. 22, 23. 1865–7.
British Architects, Royal Institute of—Sessional Papers, 1877–8, Nos. 1, 2. 4to.
British Museum, Trustees—Catalogue of Additions to the MSS. 1854–75, Vol. II. 8vo. 1877.
Catalogue of the Ethiopic MSS. acquired since 1847. 4to. 1877.
Carpenter, W. B.—Temperature of the Deep-Sea Bottom. (R. Geog. Soc. Proceedings, Vol. XXI.)
Chemical Society—Journal, Nov. 1877. 8vo.
Clinical Society—Transactions, Vol. X. 8vo. 1877.
Editors—American Journal of Science for Nov. 1877. 8vo.
Analyst for Nov. 1877. 8vo.
Athenæum for Nov. 1877. 4to.
Chemical News for Nov. 1877. 4to.
Engineer for Nov. 1877. fol.
Horological Journal for Nov. 1877. 8vo.
Journal for Applied Science for Nov. 1877. fol.
Nature for Nov. 1877. 4to.
Nautical Magazine for Nov. 1877. 8vo.
Pharmaceutical Journal for Nov. 1877. 8vo.
Telegraphic Journal for Nov. 1877. 8vo.
Franklin Institute—Journal, Nos. 622, 623. 8vo. 1877.
Gairdner, W. T. M.D.—Two Lectures: 1. Lectures, Books, and Practical Teaching; 2. Clinical Teaching. 8vo. 1877.
Genève, Société de Physique—Mémoires, Tome XXV. Partie 1. 4to. 1877.

Geological Society of Ireland, Royal—Journal, New Series, Vol. IV. Parts 3, 4. 8vo. 1875–7.

Goodeve, T. M. Esq. M.A. (the Author)—The Whitworth Measuring Machine. By T. M. Goodeve and C. B. P. Shelley. 8vo. 1877.

Linnean Society—Proceedings, No. 93. 8vo. 1877.

Transactions: Second Series: Zoology, Vol. I. Parts 5, 6. 4to. 1877.

Manchester Geological Society—Transactions, Vol. XIV. Part 14. 8vo. 1877.

Medical and Chirurgical Society, Royal—Transactions, Vol. LX. 8vo. 1877.

Preussische Akademie der Wissenschaften—Monatsberichte: Aug. 1877. 8vo.

Royal Society of London—Proceedings, No. 183. 8vo. 1877.

Russell, The Lord Arthur, M.P.—Report on the State of the Royal Observatory, Edinburgh. (P 12.) fol. 1877.

Papers on the Bengal Cyclone, 31 Oct.-1 Nov. 1876; and the subsequent Cholera Epidemic. (P 12.) 1877.

Stockholm, Royal Academy of Sciences—Handlingar, Bandet XIII. and XIV. 1. 1874–5. Bihang, Bandet III. Häfte 2. 8vo. 1876.

Ofversigt, 1876. 8vo.

Symons, G. J.—Monthly Meteorological Magazine, Nov. 1877. 8vo.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen. Hefte 7, 8. 1877. 4to.

Victoria Institute—Transactions, No. 43. 8vo. 1877.

Vincent, Benjamin, Lib. R.I. (the Editor)—Dictionary of Biography, Past and Present, brought down to September, 1877. (Haydn Series.) 8vo. 1877.

The following arrangements of the Lectures before Easter, 1878, were announced:—

CHRISTMAS LECTURES.

PROFESSOR TYNDALL, D.C.L. F.R.S.—Six Lectures adapted to a Juvenile Auditory, on Heat, Visible and Invisible; on Dec. 27 (Thursday), 29, 1877: Jan. 1, 3, 5, 8, 1878.

PROFESSOR ALFRED H. GARROD, M.A. F.R.S.—Twelve Lectures on the Protoplasmic Theory of Life and its Bearing on Physiology; on Tuesdays, Jan. 22 to April 9.

JAMES DEWAR, Esq. M.A. F.R.S.—Twelve Lectures on the Chemistry of the Organic World; on Thursdays, Jan. 24 to April 11.

R. BOSWORTH SMITH, Esq. M.A.—Seven Lectures on Carthage and the Carthaginians; on Saturdays, Jan. 26 to March 9.

REV. W. HOUGHTON.—Three Lectures on the Natural History of the Ancients; on Saturdays, March 16, 23, 30.

ERNST PAUER, Esq.—Two Lectures on the Clavecinistes and their Works (England and Italy; France and Germany); with Musical Illustrations; on Saturdays, April 6, 13.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 25, 1878.

SIR W. FREDERICK POLLOCK, Bart. M.A. Manager, in the Chair.

PROFESSOR HUXLEY, LL.D. F.R.S.

William Harvey.[*]

ON the coming First of April, three hundred years will have elapsed since the birth of William Harvey, who is popularly known as the discoverer of the circulation of the blood.

Many opinions have been held respecting the exact nature and value of Harvey's contributions to the elucidation of the fundamental problem of the physiology of the higher animals; from those which deny him any merit at all—indeed, roundly charge him with the demerit of plagiarism—to those which enthrone him in a position of supreme honour among great discoverers in science. Nor has there been less controversy as to the method by which Harvey obtained the results which have made his name famous. I think it is desirable that no obscurity should hang around these questions; and I add my mite to the store of disquisitions on Harvey which this year is likely to bring forth, in the hope that it may help to throw light upon several points about which darkness has accumulated, partly by accident and partly by design.

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Aristotle's description of the heart is often cited as an example of his ignorance, but I think unjustly. However this may be, it is certain that, not long after his time, great additions were made to anatomical and physiological science. The Greek anatomists, exploring the structure of the heart, found that it contained two principal cavities, which we now call the ventricles, separated by a longitudinal partition, or septum: the one ventricle is on its left, the other on its right side. It was to the fleshy body which contains the ventricles that the ancients restricted the title of "heart." Moreover, there is another respect in which their terminology was so different from that of the moderns, that, unless we recollect that the facts may be just as accurately stated in their fashion as in ours, we are liable to fall into the mistake of supposing that they are blundering.[*] What they speak of as the auricles of the heart, we term the appendices of the auricles; and what we call the auricles are, for the ancients, on the right side, a part of the great vein or vena cava, and, on the left side, a part of the arterial system—the root, in fact, of what they termed the arteria venosa. Thus they speak of the auricles as mere appendages, or dilatations, situated upon the arterial and venous trunks respectively, close to the heart; and they always say that the vena cava and the arteria venosa open into the right and left ventricles respectively. And this was the basis of their classification of the vessels, for they held all those vessels which, in this sense, open into the right ventricle to be veins, and all those which open into the left ventricle to be arteries. But here a difficulty arose. They observed that the aorta, or stem of the arteries, and all the conspicuous branches which proceed from it to the body in general, are very different from the veins; that they have much thicker walls and stand open when they are cut, while the thin-walled veins collapse. But the "vein" which connected the right ventricle and the lungs had the thick coat of an artery, while the "artery" which connected the left ventricle and the lungs had the thin coat of a vein. Hence they called the former the *vena arteriosa*, or artery-like vein, and the latter the *arteria venosa*, or vein-like artery.

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[*] This discourse is printed in full in the 'Fortnightly Review,' for February, 1878, vol. xxiii. N. S. p. 167.

The vena arteriosa is what we call the pulmonary artery, the arteria venosa is our pulmonary vein; but in trying to understand the old anatomists it is essential to forget our nomenclature and to adopt theirs. With this precaution, and with the facts before our mind's eye, their statements will be found to be, in the main, exceedingly accurate.

About the year 300 B.C. a great discovery, that of the valves of the heart, was made by Erasistratus. This anatomist found around the opening by which the vena cava communicates with the right ventricle, three triangular membranous folds, disposed in such a manner as to allow any fluid contained in the vein to pass into the ventricle, but not back again. The opening of the vena arteriosa into the right ventricle is quite distinct from that of the vena cava; and Erasistratus observed that it is provided with three pouch-like, half-moon-shaped valves; the arrangement of which is such that a fluid can pass out of the ventricle into the vena arteriosa, but not back again. Three similar valves were found at the opening of the aorta into the left ventricle. The arteria venosa had a distinct opening into the same ventricle, and this was provided with triangular membranous valves, like those on the right side, but only two in number.

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[*] We say that the heart, in man and the higher animals, consists of two auricles and two ventricles; and that each auricle has an appendix in the form of a pouch. We term the vessel which arises from the right ventricle the pulmonary artery, because it supplies the lungs with blood. Those vessels which bring away the blood from the lungs to the left auricle, we call the pulmonary veins.

Thus the ventricles had four openings, two for each; and there were altogether eleven valves, disposed in such a manner as to permit fluids to enter the ventricles from the vena cava and the arteria venosa respectively, and to pass out of the ventricles by the vena arteriosa and the aorta respectively, but not to go the other way.

It followed from this capital discovery, that, if the contents of the heart are fluid, and if they move at all, they can only move in one way; namely, from the vena cava, through the ventricle and towards the lungs, by the vena arteriosa, on the right side; and, from the lungs, by way of the arteria venosa, through the ventricle, and out by the aorta for distribution in the body, on the left side.

Erasistratus thus, in a manner, laid the foundations of the theory of the motion of the blood. But it was not given to him to get any further. What the contents of the heart were, and whether they moved or not, was a point which could be determined only by experiment. And, for want of sufficiently careful experimentation, Erasistratus strayed into a hopelessly misleading path. Observing that the arteries are usually empty of blood after death, he adopted the unlucky hypothesis that this is their normal condition, and that during life, also, they are filled with air. And, it will be observed, that it is not improbable that Erasistratus' discovery of the valves of the heart and of their mechanical action strengthened him in this view. For, as the arteria venosa branches out in the lungs, what more likely than that its ultimate ramifications absorb the air which is inspired; and that this air, passing into the left ventricle, is then pumped all over the body through the aorta, in order to supply the vivifying principle which evidently resides in the air; or, it may be, of cooling the too great heat of the blood? How easy to explain the elastic bounding feel of a pulsating artery by the hypothesis that it is full of air. Had Erasistratus only been acquainted with the structure of insects, the analogy of their tracheal system would have been a tower of strength to him. There was no *primâ facie* absurdity in his hypothesis—and experiment was the sole means of demonstrating its truth or falsity.

More than four hundred years elapsed before the theory of the motion of the blood returned once more to the strait road which leads truthwards; and it was brought back by the only possible method, that of experiment. A man of extraordinary genius, Claudius Galenus, of Pergamos (born in 131 A.D., died in or about 201), was trained to anatomical and physiological investigation in the great schools of Alexandria, and spent a long life in incessant research, teaching, and medical practice. More than one hundred and fifty treatises from his pen, on philosophical, literary, scientific, and practical topics, are extant; and there is reason to believe that they constitute not more than a third of his works. No former anatomist had reached his excellence, while he may be regarded as the founder of experimental physiology. And, it is precisely because he was a master of the experimental method, that he was able to learn more about the motions of the heart and of the blood than any of his predecessors; and to leave to posterity a legacy of knowledge, which was not substantially increased for more than thirteen hundred years.

The conceptions of the structure of the heart and vessels, of their actions, and of the motion of the blood in them, which Galen entertained, are not stated in a complete shape in any one of his numerous works. But a careful collation of the various passages in which these conceptions are expressed, leaves no doubt upon my mind, that Galen's views respecting the structure of the organs concerned were, for the most part, as accurate as the means of anatomical analysis at his command permitted; and that he had exact and consistent, though by no means equally just, notions of the actions of these organs, and of the movements of the blood.

Starting from the fundamental facts established by Erasistratus respecting the structure of the heart and the working of its valves, Galen's great service was the proof, by the only evidence which could possess demonstrative value, namely, by that derived from experiments upon living animals, that the arteries are as much full of blood during life as the veins are, and that the left cavity of the heart, like the right, is also filled with blood.

Galen, moreover, correctly asserted, though the means of investigation at his disposition did not allow him to prove the fact, that the ramifications of the vena arteriosa in the substance of the lungs communicate with those of the arteria venosa, by direct, though invisible, passages, which he terms anastomoses; and that, by means of these communications, a certain portion of the blood of the right ventricle of the heart passes through the lungs into the left ventricle. In fact, Galen is quite clear as to the existence of a current of blood through the lungs, though not of such a current as we now know traverses them. For, while he believed that a part of the blood of the right ventricle passes through the lungs, and even, as I shall show, described at length the mechanical arrangements by which he supposes this passage to be effected, he considered that the greater part of the blood in the right ventricle passes directly, through certain pores in the septum, into the left ventricle. And this was where Galen got upon his wrong track, without which divergence a man of his scientific insight must infallibly have discovered the true character of the pulmonary current, and not improbably have been led to anticipate Harvey.

But, even in propounding this erroneous hypothesis of the porosity of the septum, it is interesting to observe with what care Galen distinguishes between observation and speculation. He expressly says that he has never seen the openings which he supposes to exist, and that he imagines them to be invisible, by reason of their small size and their closure by the refrigeration of the heart, after death. Nevertheless, he cannot doubt their existence, partly because the septum presents a great number of pits which obviously lead into its substance as they narrow, and, as he is so fond of saying, "Nature makes nothing in vain;" and, partly, because the vena cava is so large, in comparison with the vena arteriosa, that he does not see how all the blood poured into the ventricle could be got rid of, if the latter were its only channel.

Thus, for Galen, the course of the blood through the heart was—on the right side, *in* by the vena cava, *out* by the vena arteriosa and the pores of the septum; on the left side, *in* by the pores of the septum and by the arteria venosa, *out* by the aorta. What now becomes of the blood which, filling the vena arteriosa, reaches the lungs? Galen's views are perfectly definite about this point. The vena arteriosa communicates with the arteria venosa in the lungs by numerous connecting channels. During expiration the blood which is in the lungs, being compressed, tends to flow back into the heart by way of the vena arteriosa; but it is prevented from doing so, in consequence of the closure of the semilunar valves. Hence a portion of it is forced the other way, through the anastomoses into the arteria venosa; and then, mixed with "pneuma," it is carried to the left ventricle, whence it is propelled, through the aorta and its branches, all over the body.

Galen not only took great pains to obtain experimental proof that, during life, all the arteries contain blood and not air, as Erasistratus supposed; but he distinctly affirms that the blood in the left ventricle and in the arteria venosa is different from that in the right ventricle and in the veins, including the vena arteriosa; and that the difference between the two lies in colour, heat, and the greater quantity of "pneuma" contained in arterial blood. Now this "pneuma" is something acquired by the blood in the lungs. The air which is inspired into these organs is a kind of aliment. It is not taken bodily into the arteria venosa and thence carried to the left ventricle to fill the arterial system, as Erasistratus thought. On the contrary, Galen repeatedly argues that this cannot be the case, and often refers to his experimental proofs that the whole arterial system is full of blood during life. But the air supplies a material kindred to the "pneuma," out of which and the blood the "pneuma" is concocted. Hence the contents of the arteria venosa are largely composed of "pneuma," and it is out of the mixture of this with the blood which filters through the septum, that the bright "pneumatic" blood found in the arteries, and by them distributed over the body, is formed. The arteria venosa is a channel by which "pneuma" reaches the heart, but this is not its exclusive function; for it has, at the same time, to allow of the passage of certain fuliginous and impure matters which the blood contains, in the opposite direction; and it is for this reason that there are only two valves where the arteria venosa enters the ventricle. These not fitting quite tightly, allow of the exit of the fuliginous matters in question.

Modern commentators are fond of pouring scorn upon Galen, because he holds that the heart is not a muscle. But if what he says on this subject is studied with care and impartiality, and with due recollection of the fact that Galen was not obliged to use the terminology of the nineteenth century, it will be seen that he by no means deserves blame, but rather praise for his critical discrimination of things which are really unlike.

All that Galen affirms is that the heart is totally unlike one of the ordinary muscles of the body, not only in structure, but in being independent of the control of the will; and, so far from doubting that the walls of the heart are made up of active fibres, he expressly describes these fibres and what he supposes to be their arrangement and their mode of action. The fibres are of three kinds, longitudinal, transverse, and oblique. The action of the longitudinal fibres is to draw in, that of the circular fibres to expel, and that of the oblique fibres to retain the contents of the heart. How Galen supposed the oblique fibres could execute the function ascribed to them I do not know; but it is clear that he thought that the activity of the circular fibres increased, and that of the longitudinal fibres diminished, the size of the cavities which they surrounded. Nowadays we term an active fibre muscular; Galen did not, unless, in addition, it possessed the characters of voluntary muscle.

According to Galen, the arteries have a systole and diastole (that is, a state of contraction and a state of dilatation), which alternate with those of the ventricles, and depend upon active contractions and dilatations of their walls. This active faculty of the arteries is inherent in them, because they are, as it were, productions of the substance of the ventricles which possess these faculties; and it is destroyed when the vital continuity of the arteries with the heart is destroyed by section or ligature. The arteries fill, therefore, as bellows fill, not as bags are blown full.

The ultimate ramifications of the arteries open by anastomoses into those of the veins all over the body; and the vivifying arterial blood thus communicates its properties to the great mass of blood in the veins. Under certain conditions, however, the blood may flow from the veins to the arteries, in proof of which Galen adduces the fact that the whole vascular system may be emptied by opening an artery.

The two ventricles, the auricles, the pulmonary vessels, and the aorta with its branches, are conceived by the Greek anatomist to be an apparatus superadded to the veins, which he regards as the essential foundation and the most important part of the whole vascular system. No portion of Galen's doctrines has been more sharply criticised than his persistent refusal to admit that the veins, like the arteries, take their origin in the heart, and his advocacy of the view that the *fons et origo* of the whole venous system is to be sought in the liver. Here, however, I must remark that it is only those who are practically ignorant of the facts who can fail to see that Galen's way of stating the matter is not only anatomically justifiable, but that, until the true nature of the circulation was understood, and physiological considerations overrode those based upon mere structure, there was much more to be said for it than for the opposite fashion.

Remembering that what we call the right auricle was for Galen a mere part of the vena cava, it is impossible not to be struck by the justice of his striking comparison of the vena cava to the trunk of a tree, the roots of which enter the liver as their soil, while the branches spread all over the body. Galen remarks that the existence of the vena portæ, which gathers blood from the alimentary canal, and then distributes it to the liver without coming near the heart, is a fatal objection to the view of his opponents, that all the veins take their rise in the heart; and the argument is unanswerable so far as the mere anatomical facts are concerned.

Nothing could have appeared more obvious to the early anatomists than that the store of nutriment carried by the vena portæ to the liver was there elaborated into blood; and then, being absorbed by the roots of the venous system, was conveyed by its branches all over the body. The veins were thus the great distributors of the blood; the heart and arteries were a superadded apparatus for the dispersion of a "pneumatized," or vivified portion of the blood through the arteries; and this addition of "pneuma," or vivification, took place in the gills of water-breathing animals and in the lungs of air-breathers. But in the latter case the mechanism of respiration involved the addition of a new apparatus, the right ventricle, to ensure the constant flow of blood through these organs of "pneumatization."

Every statement in the preceding paragraphs can be justified by citations from Galen's works; and, therefore, it must be admitted that he had a wonderfully correct conception of the structure and disposition of the heart and vessels, and of the mode in which the ultimate ramifications of the latter communicate, both in the body generally and in the lungs; that his general view of the functions of the heart was just; and that he knew that blood passes from the right side of the heart, through the lungs, to the left side, and undergoes a great change in quality, brought about by its relation with the air in the lungs, in its course. It is unquestionable, therefore, that Galen, so far, divined the existence of a "pulmonary circulation," and that he came near to a just conception of the process of respiration; but he had no inkling even of the systemic circulation; he was quite wrong about the perforation of the septum; and his theory of the mechanical causes of the systole and diastole of the heart and arteries was erroneous. Nevertheless, for more than thirteen centuries, Galen was immeasurably in advance of all other anatomists; and some of his notions, such as that about the active dilatation of the walls of the vessels, have been debated by physiologists of the present generation. No one can read Galen's works without being impressed by the marvellous extent and diversity of his knowledge, and by his clear grasp of those experimental methods by which, alone, physiology can be advanced.

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The student of the works of the anatomists and physiologists of modern Europe in the fifteenth and the beginning of the sixteenth centuries, will find that they were chiefly occupied in learning of their own knowledge what Galen knew. It is not strange, therefore, that they were overpowered by so vast a genius, and that they allowed themselves to be enslaved by his authority, in a manner which he would have been the first to reprove. Vesalius, the great reformer of anatomy, had a bitter struggle to carry on Galen's work, by showing where he had erred in expounding the structure of the human body, on the faith of observations made on the lower animals; but it was not till the middle of the sixteenth century, that anything was done to improve on Galen's physiology, and especially to amend his doctrines concerning the movements of the heart and of the blood.

The first step in this direction is very generally ascribed to Michael Servetus, * * * who was undoubtedly well acquainted with anatomy, inasmuch as he was demonstrator to Joannes Guinterus in the School of Paris, where he had Vesalius for his colleague; and, in his later years, he practised as a physician. Hence it is not wonderful to find that the 'Christianismi Restitutio,' although essentially a farrago of scatterbrained theological speculations, contains much physiological matter. And it is in developing his conception of the relations between God and man, that Servetus wrote the well-known passages on which many have asserted his claim to the discovery of the course of the blood from the heart, through the lungs, and back to the heart; or what is now termed the pulmonary circulation. I have studied the passages in question with great care, and with every desire to give Servetus his due, but I confess I cannot see that he made much advance upon Galen. As we have seen, Galen said that some blood goes to the left side of the heart from the right side through the lungs, but that the greater part traverses the septum. Servetus appears, at first, to declare that all the blood of the right side goes through the lungs to the left side, and that the septum is imperforate. But he qualifies his assertion by admitting that some of the blood of the right ventricle may transude through the septum, and thus the question between him and Galen becomes merely one of degree. Servetus cites neither observation nor experiment in favour of the imperviousness of the septum; and the impression upon my mind is that he really knew no more than Vesalius had already published, but that the tendency to headlong speculation, which is so characteristic of the man, led him to rush in where his more thoughtful colleague held back.

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The first author who declared, without any qualification, that the septum of the ventricles is imperforate, and that all the blood of the right ventricle traverses the lungs and (except so much as may be retained for the nutrition of these organs) passes to the left ventricle, was Realdus Columbus, professor of anatomy in the famous school of Padua. The remarkable treatise, 'De Re Anatomica,' of this able anatomist, was published in 1559, or only six years after the death of Servetus, of whose notions there is no evidence that Columbus had any cognizance. Moreover, Columbus, as able an experimenter as he was a skilful dissector, deals with the question in a very different way from Servetus; so that, from his time, the existence of the pulmonary circulation, in the modern sense, may be said to have become established. Ambrose Paré, the great surgeon, writing in 1579, refers to the course of the blood through the lungs as notoriously the discovery of Columbus. And I think not only that Realdus Columbus is entitled to the whole credit of this very considerable advance upon Galen's views; but that he is the only physiologist, between the time of Galen and that of Harvey, who made any important addition to the theory of the circulation.

The claim which is put forward on behalf of the celebrated botanist, Cæsalpinus, appears to me to be devoid of any foundation. Many years after the publication of the work of Realdus Columbus, who was professor at the most famous and most frequented anatomical school of the time, and who assuredly was the last man to hide his light under a bushel, Cæsalpinus incidentally describes the pulmonary circulation in terms which simply embody a statement of Columbus's doctrine; adding nothing, and, to his credit be it said, claiming nothing. Like all the rest of the world since venesection was invented, Cæsalpinus noticed that the vein swells on the side of the ligature away from the heart; and he observes that this is inconsistent with the received views of the motion of the blood in the veins. If he had followed up the suggestion thus made to him by the needful experimental investigation, he might have anticipated Harvey; but he did not.

Again, Cannani discovered the existence of valves in some of the veins in 1547; and Fabricius rediscovered them, and prominently drew attention to their mechanism, in 1574. Nevertheless, this discovery, important as it was, and widely as it became known, had absolutely no effect in leading either the discoverers or their contemporaries to a correct view of the general circulation. In common with all the anatomists of the sixteenth century, Fabricius believed that the blood proceeded from the main trunk, or vena cava, outwards to the smallest ramifications of the veins, in order to subserve the nutrition of the parts in which they are distributed; and, instead of being led by the mechanical action of the valves to reverse his theory of the course of the venous blood, he was led by the dominant theory of the course of the blood to interpret the meaning of the valvular mechanism. Fabricius, in fact, considered that the office of the valves was to break the impetus of the venous blood, and to prevent its congestion in the organs to which it was sent; and, until the true course of the blood was demonstrated, this was as likely an hypothesis as any other.

The best evidence of the state of knowledge respecting the motions of the heart and blood in Harvey's time is afforded by those works of his contemporaries which immediately preceded the publication of the 'Exercitatio Anatomica,' in 1628.[*] And none can be more fitly cited for this purpose than the 'De Humani Corporis Fabrica, Libri decem,' of Adrian van den Spieghel, who, like Harvey, was a pupil of Fabricius of Aquapendente, and was of such distinguished ability and learning that he succeeded his master in the chair of anatomy of Padua.

Van den Spieghel, or Spigelius, as he called himself, in accordance with the fashion of those days, died comparatively young in 1625, and his work was edited by his friend Daniel Bucretius, whose preface is dated 1627. The accounts of the heart and vessels, and of the motion of the blood, which it contains, are full and clear; but, beyond matters of detail, they go further than Galen in only two points; and with respect to one of these, Spigelius was in error. The first point is the "pulmonary circulation," which is taught as Columbus taught it nearly eighty years before. The second point is, so far as I know, peculiar in Spigelius himself. He thinks that the pulsation of the arteries has an effect in promoting the motion of the blood contained in the veins which accompany them. Of the true course of the blood as a whole, Spigelius has no more suspicion than had any other physiologist of that age, except William Harvey; no rumour of whose lectures at the College of Physicians, commenced six years before Spieghel's death, was likely in those days of slow communication and in the absence of periodical publications to have reached Italy.

Now let anyone familiar with the pages of Spigelius take up Harvey's treatise and mark the contrast. The main object of the 'Exercitatio' is to put forth and demonstrate, by direct experimental and other accessory evidence, a proposition which is far from being even hinted at, either by Spigelius or by any of his contemporaries or predecessors; and which is in diametrical contradiction to the views respecting the course of the blood in the veins which are expounded in their works. From Galen to Spigelius, they one and all believed that the blood in the vena cava and its branches flows from the main trunk towards the smallest ramifications. There is a similar consensus in the doctrine, that the greater part, if not the whole, of the blood thus distributed by the veins is derived from the liver; in which organ it is generated out of the materials brought from the alimentary canal by means of the vena portæ. And all Harvey's predecessors further agree in the belief that only a small fraction of the total mass of the venous blood is conveyed by the vena arteriosa to the lungs and passes by the arteria venosa to the left ventricle, thence to be distributed over the body by the arteries. Whether some portion of the refined and "pneumatic" arterial blood traversed the anastomotic channels, the existence of which was assumed, and so reached the systemic veins; or whether, on the contrary, some portion of the venous blood made its entrance by the same passages into the arteries, depended upon circumstances. Sometimes the current might set one way, sometimes the other.

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[*] The whole title of the copy of the rare first edition in the library of the College of Physicians runs, "Exercitatio Anatomica de Motu Cordis et Sanguinis in Animalibus. Gulielmi Harvæi, Angli Medici Regii et Professoris Anatomiæ in Collegio Medicorum Londinensi. Francofurti, sumptibus Gulielmi Fitzeri. Anno MDCXXVIII." The dedications, of which that to Charles I. is pasted in, as if it had been an afterthought, extend to p. 9; the Procœmium to p. 19; while the Exercitatio itself occupies pp. 20 to 72 inclusively. There are two plates illustrative of experiments on the veins of the arm.

In direct opposition to these universally received views, Harvey asserts that the natural course of the blood in the veins is from the peripheral ramifications towards the main trunk; that the mass of the blood to be found in the veins at any moment was, a short time before, contained in the arteries, and has simply flowed out of the latter into the veins; and, finally, that the stream of blood which runs from the arteries into the veins is constant, continuous, and rapid.

According to the view of Harvey's predecessors * the veins may be compared to larger and smaller canals, fed by a spring which trickles into the chief canals, whence the water flows to the rest. The heart and lungs represent an engine set up in the principal canal to aërate some of the water and scatter it all over the garden. Whether any of this identical water came back to the engine or not would be a matter of chance, and it would certainly have no sensible effect on the motion of the water in the canals. In Harvey's conception of the matter, on the other hand, the garden is watered by channels so arranged as to form a circle, two points of which are occupied by propulsive engines. The water is kept moving in a continual round within its channels, as much entering the engines on one side, as leaves them on the other; and the motion of the water is entirely due to the engines.

It is in conceiving the motion of the blood, as a whole, to be circular, and in ascribing that circular motion simply and solely to the contractions of the walls of the heart, that Harvey is so completely original. Before him, no one, that I can discover, had ever so much as dreamed that a given portion of the blood contained, for example, in the right ventricle of the heart may, by the mere mechanical operation of the working of that organ, be made to return to the very place from which it started, after a long journey through the lungs and through the body generally. And, it should be remembered, that it is to this complete circuit of the blood alone, that the term "circulation" can, in strictness, be applied. It is of the essence of a circular motion that that which moves returns to the place from whence it started. Hence, the discovery of the course of the blood from the right ventricle, through the lungs to the left

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* See the comparison of the veins to the canals for irrigating a garden in Galen, 'De Naturalibus Facultatibus,' lib. iii. cap. xv.

ventricle was in nowise an anticipation of the discovery of the circulation of the blood. For the blood which traverses this part of its course no more describes a circle, than the dweller in a street who goes out of his own house and enters his next-door neighbour's does so. Although there may be nothing but a party wall between him and the room he has just left, it constitutes an efficient *défense de circuler*. Thus, whatever they may have known of the so-called pulmonary circulation, to say that Servetus, or Columbus, or Cæsalpinus, deserves any share of the credit which attaches to Harvey, appears to me to be to mistake the question at issue.

It must further be borne in mind, that the determination of the true course taken by the whole mass of the blood is only the most conspicuous of the discoveries of Harvey; and that his analysis of the mechanism by which the circulation is brought about is far in advance of anything which had previously been published. For the first time it is shown that the walls of the heart are active only during its systole or contraction, and that the dilatation of the heart in the diastole is purely passive. Whence it follows, that the impulse by which the blood is propelled is a *vis à tergo*, and that the blood is not drawn into the heart by any such inhalent or suctorial action, as not only the predecessors, but many of the successors of Harvey imagined it to possess.

Harvey is no less original in his view of the cause of the arterial pulse. In contravention of Galen and of all other anatomists up to his own time, he affirms that the stretching of the arteries which gives rise to the pulse is not due to the active dilatation of their walls, but to their passive distension by the blood which is forced into them at each beat of the heart; reversing Galen's dictum, he says that they dilate as bags and not as bellows. This point of fundamental, practical, as well as theoretical, importance is most admirably demonstrated, not only by experiment, but by pathological illustrations.

One of the weightiest arguments in Harvey's demonstration of the circulation is based upon the comparison of the quantity of blood driven out of the heart, at each beat, with the total quantity of blood in the body. This, so far as I know, is the first time that quantitative considerations are taken into account in the discussion of a physiological problem. But one of the most striking differences between ancient and modern physiological science, and one of the chief reasons of the rapid progress of physiology in the last half century, lies in the introduction of exact quantitative determination into physiological experimentation and observation. The moderns use means of accurate measurement, which their forefathers neither possessed nor could conceive, inasmuch as they are products of mechanical skill of the last hundred years, and of the advance of branches of science which hardly existed, even in germ, in the seventeenth century.

Having attained to a knowledge of the circulation of the blood, and of the conditions on which its motion depends, Harvey had a ready deductive solution for problems which had puzzled the older physiologists. Thus the true significance of the valves in the veins became at once apparent. Of no importance while the blood is flowing in its normal course towards the heart, they at once oppose any accidental reversal of its current, which may arise from the pressure of adjacent muscles, or the like. And, in like manner, the swelling of the veins on the further side of the ligature, which so much troubled Cæsalpinus, became at once intelligible, as the natural result of the damming up of the returning current.

In addition to the great positive results which are contained in the treatise which Harvey modestly calls an 'Exercise,' and which is, in truth, not so long as many a pamphlet about some wholly insignificant affair, its pages are characterized by such precision and simplicity of statement, such force of reasoning, and such a clear comprehension of the methods of inquiry and of the logic of physical science, that it holds a unique rank among physiological monographs. Under this aspect, I think I may fairly say that it has rarely been equalled and never surpassed.

Such being the state of knowledge among his contemporaries, and such the immense progress effected by Harvey, it is not wonderful that the publication of the 'Exercitatio' produced a profound sensation. And the best indirect evidence of the originality of its author, and of the revolutionary character of his views, is to be found in the multiplicity and the virulence of the attacks to which they were at once subjected.

Riolan, of Paris, had the greatest reputation of any anatomist of those days, and he followed the course which is usually adopted by the men of temporary notoriety towards those of enduring fame. According to Riolan, Harvey's theory of the circulation was not true; and besides that, it was not new; and, furthermore, he invented a mongrel doctrine of his own, composed of the old views with as much of Harvey's as it was safe to borrow, and tried therewith to fish credit for himself out of the business.

* * * * *

Even those who gave Harvey their general approbation and support sometimes failed to apprehend the value of some of those parts of his doctrine which are, indeed, merely auxiliary to the theory of the circulation, but are only a little less important than it. Harvey's great friend and champion, Sir George Ent, is in this case; and I am sorry to be obliged to admit that Descartes falls under the same reprehension. This great philosopher, mathematician, and physiologist, whose conception of the phenomena of life as the results of mechanism is now playing as great a part in physiological science as Harvey's own discovery, never fails to speak with admiration, as Harvey gratefully acknowledges, of the new theory of the circulation. And it is astonishing, I had almost said humiliating, to find that even he is unable to grasp Harvey's profoundly true view of the nature of the systole and the diastole, or to see the force of the quantitative argument. He adduces experimental evidence against the former position, and is even further from the truth than Galen was in his ideas of the physical cause of the circulation.

In spite of all opposition, the doctrine of the circulation propounded by Harvey was, in its essential features, universally adopted within thirty years of the time of its publication. Harvey's friend, Thomas Hobbes, remarked that he was the only man, in his experience, who had the good fortune to live long enough to see a new doctrine accepted by the world at large.

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I proposed at the outset of this discourse to say something about the method of inquiry which Harvey pursued, and which guided him throughout his successful career of discovery.

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On the faith of a conversation reported by Robert Boyle, Harvey is said to have declared that he discovered the circulation of the blood by reasoning deductively from the disposition of the valves of the veins. On this I may remark, firstly, that the words imputed to Harvey by no means warrant this conclusion; secondly, that if they did, the statement could not be true, because we have Harvey's own evidence to the contrary; and thirdly, that if the conclusion were warranted by the words reported, and were not contradicted by Harvey himself, it would still be worthless, because it is impossible to prove the circulation of the blood from any such data. What Robert Boyle says is this:—"And I remember, that when I asked our famous Harvey, in the only discourse I had with him (which was but a while before he died), what were the things that induced him to think of a circulation of the blood? he answered me, that when he took notice that the valves in the veins of so many parts of the body were so placed, that they gave free passage to the blood towards the heart, but opposed the passage of the venal blood the contrary way: he was invited to imagine that so provident a cause as nature had not so placed so many valves without design; and no design seemed more probable, than that since the blood could not well, because of the interposing valves, be sent by the veins to the limbs, it should be sent through the arteries and return through the veins, whose valves did not oppose its course that way."[*]

I have no doubt that it may be quite true, that Harvey was "induced" to "think of a circulation of the blood" by considering the disposition of the valves of the veins; just as Cæsalpinus might have been led to the same thought; and then might have found out the true state of the case, if he had taken the hints which nature gave him, and had used the proper means of investigation in order to discover whether those hints were valuable or worthless. Harvey must have learned the views of his master Fabricius; and it is likely enough that to his acute mind Fabricius's explanation of the functions of the valves seemed rather lame. But, as a matter of fact, Harvey did not reason out the circulation from the datum of the valves. On this point his own words, in the passage which contains the fullest account of the considerations which led him to the doctrine of the circulation, leave no doubt whatever:—

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[*] A Disquisition about the Final Causes of Natural Things.—Boyle's Works, vol. v. p. 427.

> Thus far I have spoken of the passage of the blood from the veins into the arteries,* and of the manner in which it is transmitted and distributed by the action of the heart; and thus far some, perhaps, moved by the authority of Galen, or of Columbus, or by the reasonings of other authors, will agree with me. But when I proceed to what remains to be said concerning the quantity and the origin of the blood thus transmitted (though it is highly worthy of consideration) it will seem so new and unheard of, that I not only fear injury to myself from the envy of a few; but I dread lest I make all mankind my enemies. So much does custom, or teaching once accepted and fixed by deep roots, weigh with all; and such is the influence of the venerable opinion of antiquity. However this may be, now that the die is cast, my hope lies in the candour of lovers of truth and of learned minds. Indeed, when I thought often and seriously upon how large the quantity [of transmitted blood] is; upon my dissections of living animals (for the purposes of experiment) and the opening of arteries and the many considerations arising therefrom; as well as upon the magnitude and the symmetry of the ventricles of the heart and of the vessels which enter and leave them (since nature makes nothing in vain, so great a size proportionally would not be given to these vessels without an object); and upon the elaborate mechanism of the valves and fibres, and of the rest of the structure of the heart; as well as of many other things; and when I long turned over in my mind, what might be the quantity of the transmitted blood; in how short a time its transmission might be effected; whether that quantity could be supplied by the juices of the food ingested; I came at length to the conclusion that the veins would become collapsed and empty, while the arteries, on the other hand, would be ruptured, by the excess of blood poured in them; unless there were some road by which the blood could at length run back from the arteries into the veins and return to the right ventricle of the heart. So I began to think whether there was a kind of motion as it were in a circle; this I afterwards found to be true.†

In all this very full and interesting account of the course of Harvey's inquiry, it will be observed that not one word is said about the valves of the veins. The valves of which he speaks are those of the heart, which had been known, as I have pointed out, ever since the days of Erasistratus.

Finally, I venture to affirm that Harvey did not deduce the circulation from the disposition of the valves of the veins, because it is logically impossible that any such conclusion should be deduced from such premises. The only conclusion which is warranted by the presence of valves in the veins is, that such valves will tend to place a certain amount of obstacle in the way of a liquid flowing in a direction opposite to that in which the valves are inclined. The amount of obstacle, from mere impediment to absolute barring of the way, will depend upon the form and disposition of the valves; upon their inertia, or stiffness of motion, in relation to the force of the current of liquid; and, above all, upon the firmness or yieldingness of the walls of the tube to which they are attached. The valve which hermetically closes the passage through an iron pipe may be of no use in an india-rubber tube. Therefore, unless the action of such valves as exist in the veins were carefully tested by experiment on the living animal, any conclusions that might be based upon their presence would be of doubtful value, and might be interpreted either in the sense of Fabricius, or in that of Harvey.

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* In the preceding chapter (vii.) Harvey has been discussing the passage of the blood through the lungs, supporting his views, among other arguments, by the authority of Galen and of Columbus; and it must be remembered that he termed the pulmonary artery *vena arteriosa*, and the pulmonary vein *arteria venosa*. Wherefore he properly speaks of the passage of the blood "from the veins into the arteries."

† Gulielmi Harveji. 'Exercitationes Anatomicæ.' Exercitatio I. cap. viii. ed. 1660.

Moreover, supposing that it could be proved that, in those veins in which valves exist, the blood can move only in one way, what is to be said about the numerous veins which have no valves? And, unless we already know upon experimental grounds that the walls of the cavities of the heart contract in a certain definite order; that the arteries are full of blood and not of air; and a number of other important facts which can only be experimentally determined; what good is it to know that there are valves in the veins? There are valves in the lymphatics as well as in the veins, and yet anyone who concluded therefrom that the lymph circulates after the manner of the blood would make a woeful mistake.

The fact is that neither in this, nor in any, physiological problem can mere deductive reasoning from dead structure tell us what part that structure plays when it is a living component of a living body. Physiology attempts to discover the laws of vital activity, and these laws are obviously ascertainable only by observation and experiment upon living things.

In the case of the circulation of the blood, as in that of all other great physiological doctrines, take away the truths which have been learned by observation and experiment on living structures, and the whole fabric crumbles away. Galen, Columbus, Harvey, were all great vivisectors. And the final ocular demonstration of the circulation of the blood by Malpighi, seven years after Harvey's death—the keystone of the fabric he raised—involved an experiment on a living frog.

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WEEKLY EVENING MEETING,

Friday, February 1, 1878.

Warren De la Rue, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

William Henry Preece, Memb. Inst. C.E. *M.R.I.* &c. &c.

The Telephone.

The telephone is an instrument constructed for the transmission of sound to a distance. The art of conveying sound to a distance is as old as the ancient Sphinx. That marvellous people the Greeks practised it; and doubtless it has served to inspire awe in many a poor pagan of simple faith as he reverently kneeled before his idol of stone or of bronze. The earliest authentic account of the germ of the telephone was within the historical period of science shadowed forth by Robert Hooke in 1667, who said:—

"'Tis not impossible to hear a whisper at a furlong's distance, it having been already done; and perhaps the nature of the thing would not make it more impossible, though that furlong should be ten times multiply'd. And though some famous Authors have affirm'd it impossible to hear through the thinnest plate of Muscovy glass; yet I know a way, by which 'tis easie enough to hear one speak through a wall a yard thick. It has not yet been thoroughly examin'd how far Otacousticons may be improv'd, nor what other wayes there may be of quickning our hearing, or conveying sound through other bodies than the Air: for that that is not the only medium I can assure the Reader, that I have, by the help of a distended wire, propagated the sound to a very considerable distance in an instant, or with as seemingly quick a motion as that of light, at least incomparably quicker than that which at the same time was propagated through the Air; and this not only in a straight line, or direct, but in one bended in many angles."

This fancy remained an idea until 1819, when Wheatstone produced his "magic lyre," which was exhibited to delighted crowds at the Adelaide Gallery, which was often used by Professor Faraday and which has frequently since been produced by Professor Tyndall at the Royal Institution. A large musical box was placed in one of the cellars of the Institution, and a light rod of deal rested upon it. No sound was heard in the theatre until a light tray or other sounding-box was placed by the writer on the rod, when its music pealed forth over the whole place. This was the first telephone, and was the precursor of those elegant toy-telephones which are now sold in the streets for a penny.

The vibrations of the musical box, with all their complexity and beauty, are imparted to the rod of wood, and are thence given up to the sounding-box. The sounding-box impresses them upon the air, and the air conveys them to the ear, whence they are transmitted to the brain, imparting those agreeable sensations called music. Sonorous vibrations, whether the result of music, of the human voice, or of mere noise, vary in pitch, in loudness, and in clangtint. The *pitch* of a note is dependent on the length of its sonorous vibration, or on the number of sound-waves which enter the ear per second; the *loudness* of a note depends on the amplitude of the air-wave, or on the length of swing to and fro of the particles of air in vibration; the *clangtint*, or quality of the note, depends upon the form or rate of motion of these particles. The limits of the ear to the reception of notes are between 16 and 38,000 vibrations per second, and the limits of the human voice are between 65 and 1044 vibrations per second. The amplitude of vibrations is very small. Lord Rayleigh has shown that a motion of only $\frac{1}{25000000}$th of an inch is sufficient to produce audible sounds.

Vibrations of matter are essential to the production of sound, and the presence of air is essential to convey it to our ears.

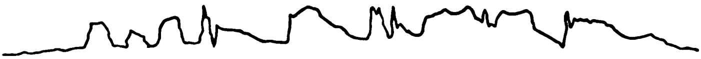
It is possible to catch up these sonorous vibrations by placing elastic matter in their path. Thus glasses can be cracked by a loud bass voice; bodies are made to rattle in a room where music is going on; and it is only necessary to sing into a piano to receive back responsive sounds. A thin copper disc held before the mouth vibrates to each sound uttered, and if a hard metallic point be adjusted near it, sounds loud enough to fill the theatre are emitted. Acting upon this, Mr. W. H. Barlow, C.E., produced before the Royal Society in 1874 his logograph, which recorded in varying lines and curves spoken language. Here is such a line, which records the pitch, loudness, and form of the sounds emitted by the lips of the speaker, and reproducing all the elements of the voice. See Fig. 1.

The transmission of sonorous vibrations by Wheatstone's telephone is very limited—the distance traversed can be measured by yards. To transmit or reproduce them at distances beyond the reach of the ear or the eye, measured by miles, we call in the aid of electricity.

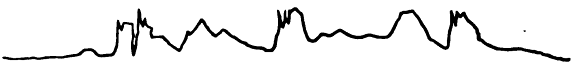
An electric current when transmitted around a bar of iron converts that bar of iron for the time being into a magnet. When a momentary current is sent around the iron it is magnetized, and then demagnetized. Page, in 1837, showed that these operations, when repeated with sufficient rapidity, produced sounds which he called "galvanic music." The bar of iron alters its form, it in fact vibrates, and these vibrations are imparted to the air.

Again, this bar of iron, when magnetized, will attract another piece of iron in its neighbourhood, and if this second bar be an armature with a certain amount of resilience or elasticity, it can be made to vibrate in a similar way and give forth musical sounds, when the currents are sent with the necessary rapidity. This was done by Elisha Gray and Alexander Graham Bell, in 1873. But electricity exerts attraction in another way. The inside and outside coating of a Leyden jar, for instance, attract each other when they are charged with electricity, but they are prevented from moving by the rigid glass between them. If this glass be supplanted by dry paper and the two coatings lie flat against the paper, one on each side, then they will move, and if the charges and discharges be rapid enough the tin-foil will vibrate and give forth sounds. This was done by Cromwell Varley, in 1870. Again, an electric current when it passes through a liquid, or an electrolyte, decomposes it into its constituent parts. If it passes through water it decomposes it into oxygen and hydrogen. If paper be damped with water, or better, with a solution of sulphate of soda, and the paper be drawn between two springs, pressing it with a certain pressure, then every time a current passes a layer of hydrogen will be deposited on one spring, the friction between that spring and the paper will be reduced, and the paper will be jerked forward. If the currents follow each other with proper rapidity these jerks will become sonorous vibrations, and loud sounds will be produced if the spring be connected with a sounding-board. This was done by Edison in 1877. Elisha Gray,

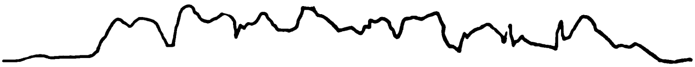
Fig. 1.



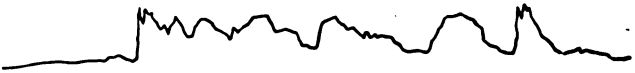
The minstrel boy to the war is gone,



In the ranks of death you'll find him;



His father's sword he has girded on,

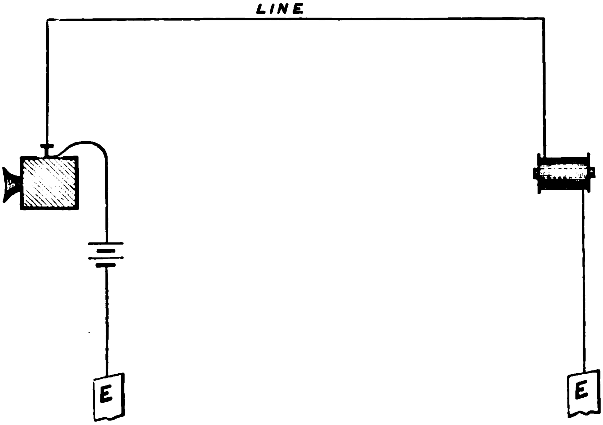


And his wild harp slung behind him.

as early as 1873, showed that similar sounds could be produced by the finger when pressed against a revolving tin disc, if currents were sent through the body in proper order. Hence sounds can be produced by the electric current in various ways.

It has already been shown how a copper disc, or, indeed, any elastic diaphragm, can be made to vibrate under the influence of sonorous vibrations, and if these vibrations be simply made to complete an electric circuit, then currents of electricity can be sent to any distance for every excursion to and fro made by the diaphragm. If these currents be sent through the primary wire of an induction coil, and the secondary wire be in connection with a vacuum tube, rotating with regular velocity, then for each note that is sounded a different figure will be found by the tube, and it will be seen that every note is distinguished by a different number of currents or by a different pitch.* On this principle Riess constructed the first electric telephone in 1861. A vibrating diaphragm transmitted its currents of electricity for each note sounded, and a magnetized and demagnetized knitting-needle repeated these notes on Page's principle. Varley in 1870, and Gray in 1873, repeated these experiments with vibrating tuning-forks and vibrating reeds. Both Edison and Bell have also produced musical telephones, but all these instruments reproduce the pitch of the note only. Something more was required to produce loudness and clangtint, and this was done by Alexander Graham Bell. He succeeded in producing currents of electricity which varied in duration, intensity, and form exactly with the length, amplitude, and form

Fig. 2.



Riess's Telephone.

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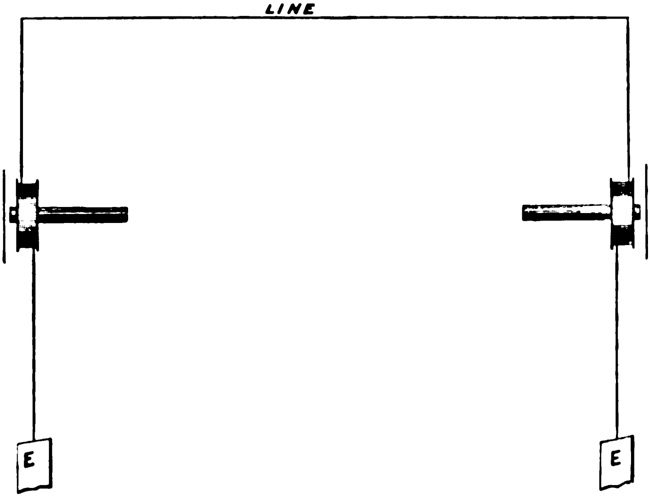
* This experiment is due to Mr. H. Edmunds, jun.

of the sonorous vibrations, and thus he succeeded in reproducing the tones of the human voice, with all their variations and peculiarities.

In nearly all the applications of electricity to useful purposes, we have to fall back upon one or other of the grand discoveries made in this very Institution by the immortal Faraday. He showed in 1831 how motion in a magnetic field produces currents of electricity. If a coil of wire be suddenly lifted off the poles of a permanent magnet, a current of electricity will be induced in that coil which can ring a bell or do other useful work. If the wire be wound permanently around the poles of the magnet, and a disc or bar of iron be moved in front of that magnet, not only will a current of electricity be induced for every motion of that disc, but the form and strength of that current will vary with the motion of the disc. This is beautifully shown with a Thomson's reflecting galvanometer. Every motion of that disc, however rapid and however minute, will call forth its corresponding current.

We have now all the requisites for what the Germans call the *Fernsprecher* or "far-speaker," or for what we know as the "articulating telephone," an instrument constructed for the *reproduction* of human speech at a distance. All that is needed is a permanent magnet, surrounded at one pole by a coil of wire and fixed in front of an elastic disc of thin soft iron. The coil of wire has one end in connection with the earth and the other in connection with the line

FIG. 3.



Bell's Telephone.

wire which is connected at the distant station with an exactly similar coil surrounding a similar magnet placed before a similar disc. There is no battery, there are no accessory apparatus; all that is needed to speak is there. The two instruments are exactly reversible —the same instrument may be used either for speaking or for listening. The one disc is held before the mouth of the speaker at one station, the other disc is held to the ear of the listener at the other station. The voice throws the air into vibration. These vibrations are imparted to the iron disc. The motion of the iron disc, by changing the strength of magnetism of the magnet, produces currents of electricity. These currents of electricity traverse the line and the coil at the distant station, and vary the strength of the magnetism of the magnet at that station. This variation of magnetism varies the attraction between the magnet and the disc. The disc is thrown into vibration, and the vibrations of the disc are imparted to the air, which conveys them to the ear and thence to the brain. Now the vibrations of the first disc, the currents of electricity, the attractions between the second magnet and the second disc, and the vibrations of the second disc, all vary exactly in a similar ratio and in a similar way. The vibrations of the second disc become an exact reproduction of the first disc—the one is in fact an echo of the other—so that whatever sound is produced in front of the first disc is exactly reproduced at the other, but with diminished power only. The first disc is only able to take up a portion of the sonorous vibrations of the speaker, and much of the actual energy of the voice is lost in overcoming the resistance of the disc and in heating the wires. Hence the sounds emitted at the second station are weaker than those at the first, but they possess the same articulation, the same tone, and the same peculiarities. The currents that perform this work are of microscopic streugth. It has been calculated that they must be less than $\frac{1}{1000000}$th of the ordinary working currents employed in telegraphy. Indeed we have no instruments at present capable of measuring them. Nevertheless the distances through which conversation has been maintained are marvellous. Professor Bell spoke from New York to Boston, 260 miles. The writer has spoken between Holyhead and Dublin, 70 miles. Conversation between Dover and Calais was maintained by Mr. Bordeaux with great ease. Captain Turner, R.E., *whispered* between London and Ipswich, 68 miles. But this assumes the lines to be clear and free from working wires. If the telephonic circuit be carried on the same poles or in the same pipes as working wires, the instrument is so delicate that it is disturbed by the induction currents that are set up in the telephonic wire by the primary currents in the working wires. This limits the distance to which conversation is possible. On busy trunk lines it has been found impossible to speak to greater distances than five miles.

It has been shown that discs vibrate under the influence of sonorous vibrations, and that these vibrations can be recorded. If these records be made on some yielding inelastic mass like tin-foil, they not only become permanent records, but they can be made to cause a similar disc at any future time to repeat or reproduce similar vibrations. Mr. T. A. Edison, of New York, has succeeded on this principle in constructing a "Phonograph," which repeats the voice of the speaker. He has crystallized into a fact the ideal of the poet who longed "for the sound of a voice that is still."

The Phonograph is the outcome of the articulating telephone. Though several have added their share in perfecting the "far-speaker," there is no name in connection with it that will shine with greater brilliancy than that of Alexander Graham Bell. His father's occupation of a vocal physiologist led him to study the vocal organs and the production of sound. Helmholtz's researches led him to investigate electricity and its application to telegraphy. The desire to increase the capacity of wires for the conveyance of messages led him to devise systems of multiplex telegraphy, and this by steady and sensible degrees led him to articulate telephony. We have here a notable example of the modern method of research where imagination suggests experiment, and experiment by evolution produces growth and perfection. Things that are new are not always accepted as true. The accounts of the telephone were received in this country with great scepticism. Many even now doubt its truth until they actually test its reality. When once, however, a new thing is shown to be true, a host of detractors delight in proving that it is not new. The inventor has to pass through the ordeal of abuse. He is shown to be a plagiarist or a purloiner, or something worse. Others are instanced as having done the same thing years ago, though perhaps their own existence apart from their ideas have never before been heard of. Professor Bell will have to go through all this; nevertheless the telephone will always be associated with his name, and it will remain one of the marvels of this marvellous age, while its chief marvel will be its beautiful and exquisite simplicity.

[W. H. P.]

GENERAL MONTHLY MEETING,

Monday, February 4, 1878.

Warren De la Rue, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Col. George Sligo Alexander Anderson,
Henry Sanders Carpenter, Esq.
Robert Burgoyne, Esq.
Lieut.-Col. Edmund Bentley Frith,
The Lord Claud Hamilton,
Edmund Haynes, Esq.
Charles Mallet, Esq.
Josiah Pierce, Esq.

were *elected* Members of the Royal Institution.

The Special Thanks of the Members were given to William Bowman, Esq. F.R.S. M.R.I. for his Present of an Ivory Bust of Professor Faraday, by the late Matthew Noble, M.R.I.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The India Office—F. Day: The Fishes of India, Vol. I. 4to. 1876.
The Lords of the Admiralty (*through the Astronomer Royal*)—Greenwich Observations for 1875. 4to. 1877.
Col. J. F. Tennant: Report on the Transit of Venus, as seen at Roorkee and Lahore, Dec. 8, 1874. 4to. 1877.
E. J. Stone: Results of Astronomical Observations at the Cape of Good Hope in 1874. 8vo. 1877.
American Academy of Arts and Sciences—Proceedings, New Series, Vol. IV.; Old Series, Vol. XIV. 8vo. 1877.
American Philosophical Society—Proceedings, No. 69. 8vo. 1863.
Asiatic Society, Royal—Journal, New Series, Vol. X. No. 1. 8vo. 1877.
Asiatic Society, Royal, Bombay Branch—Journal, No. 34a. 8vo. 1877.
Asiatic Society of Bengal—Journal, 1877, Part II. No. 2. 8vo.
Proceedings, 1877, No. 6. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVIII. Nos. 1, 2. 8vo. 1877–8.
Memoirs, Vol. XLIII. 4to. 1877.
Bavarian Academy of Sciences, Royal — Sitzungsberichte, 1877, Heft 2. 8vo. 1877.
Briggs, R. Esq. (*the Author*)—On the Relation of Moisture in the Air to Health and Comfort. (Franklin Inst. Journal, Vol. CV. 1878.) 8vo.
British Architects, Royal Institute of—Sessional Papers, 1877–8, Nos. 3, 4, 5, 6. 4to.

British Museum Trustees—Catalogue of Oriental Coins, Vol. III. 8vo. 1877.
Catalogue of Greek Coins: Tauric, Chersonese, Thrace, &c. 8vo. 1877.
Catalogue of Prints and Drawings: Division 1, Satires, Vol. III. 1734–60. 2 Parts. 8vo. 1877.

Brussels Royal Observatory—Annales Météorologiques, 1874-5–6. 4to. 1875–7.

Chemical Society—Journal, Dec. 1877 and Jan. 1878. 8vo.

Church, Professor A. H. M.A. M.R.I. (the Author)—Plain Words about Water. (K 102) 8vo. 1877.

Clinical Society—Transactions, Vol. X. 8vo. 1877.

Coutts, John, Esq. (the Author)—Philosophy of the Seven Principles found in Creation: by which Revelation and Science are found to be in Complete Harmony. 12mo. 1877.

Editors—American Journal of Science for Dec. 1877 and Jan. 1878. 8vo.
Analyst for Dec. 1877 and Jan. 1878. 8vo.
Athenæum for Dec. 1877 and Jan. 1878. 4to.
Chemical News for Dec. 1877 and Jan. 1878. 4to.
Engineer for Dec. 1877 and Jan. 1878. fol.
Horological Journal for Dec. 1877 and Jan. 1878. 8vo.
Journal for Applied Science for Dec. 1877 and Jan. 1878. fol.
Nature for Dec. 1877 and Jan. 1878. 4to.
Nautical Magazine for Dec. 1877 and Jan. 1878. 8vo.
Pharmaceutical Journal for Dec. 1877 and Jan. 1878. 8vo.
Telegraphic Journal for Dec. 1877 and Jan. 1878. 8vo.

Frankland, Professor, D.C.L. F.R.S. M.R.I. (the Author) — Experimental Researches in Pure, Applied, and Physical Chemistry. 8vo. 1877.

Franklin Institute—Journal, Nos. 624, 625. 8vo. 1877.

Geographical Society, Royal—Proceedings, Vol. XXII. No. 1. 8vo. 1877.

Geological Survey of India—Records, Vol. X. Part 4. 8vo. 1877.

Gladstone, J. H. Ph.D. F.R.S. M.R.I. (the Author)—Spelling Reform from an Educational Point of View. 16to. 1878.

Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XII. Liv. 2, 3, 4, 5. 8vo. 1877.
Natuurkundige Verhandelingen, 3de Verz. Deel II. No. 6. 4to. 1877.

Harrison, W. H. Esq. (the Author)—The Lazy Lays and Prose Imaginings. 16to. 1877.

Leeds Philosophical and Literary Society—The Archbishop of York's Address: "The Worth of Life." (L 17) 8vo. 1877.

Lindsay, The Lord, M.P. M.R.I.—Dun Echt Publications, Vol. II. Mauritius Expedition, Division 1. 4to. 1877.

Linnean Society—Proceedings, No. 94. 8vo. 1877.

London Institution—Journal, Nos. 22, 23, 24, 25, 26, 29, 30; and Selected Notes of Lectures, 1876–7. 8vo. 1873–7.

Manchester Geological Society—Transactions, Vol. XIV. Parts 15, 16. 8vo. 1877–8.

Mechanical Engineers' Institution—Proceedings, November, 1877. 8vo.

Melsens, M. (the Author)—Des Paratonnerres. 8vo. 1877.

Meteorological Office—Quarterly Weather Report, 1875, Parts 1, 2. 4to. 1877.

New Zealand Government—Statistics for 1876. fol. 1877.

North of England Institute of Mining Engineers—Transactions, Vol. XXVI. 8vo. 1877.

Photographic Society—Journal, New Series, Vol. II. Nos. 2, 3. 8vo. 1877.

Plateau, J. Hon. M.R.I. (the Author)—Bibliographie Analytique des Principaux Phénomènes Subjectifs de la Vision. (M 8) 4to. 1877.

Preussische Akademie der Wissenschaften—Monatsberichte: Sept. Oct. 1877. 8vo.

Reynolds, J. Emerson, M.D. (the Author)—Influence of Chemical Constitution on Physiological Activity. (K 102) 8vo. 1877.
Trin. Coll. Laboratory Reports, 4, 5 (Royal Dublin Society, Proceedings). 8vo. 1877.

Rossetti, Signor Carv. F. (the Author)—Di Alcuni Progressi nelle Scienze Fisiche. (K 102) 8vo. 1878.

Royal Society of Edinburgh—Transactions, Vol. XXVIII. Part 1. 4to. 1876–7. Proceedings, Nos. 96–99. 8vo. 1876–7.

Royal Society of London—Proceedings, No. 184. 8vo. 1877.

Royal Society of New South Wales—H. C. Russell: Climate of New South Wales. 8vo. 1877.

Railways of New South Wales, 1872–5. fol. 1876.

Annual Report on Department of Mines for 1876. 4to. 1877.

C. Robinson: Progress and Resources of New South Wales. (K 102) 8vo. 1877.

Journal of Proceedings of the Royal Society of New South Wales, Vol. X. 8vo. 1877.

Russell, The Lord Arthur, M.P. M.R.I.—G. von Buquoy: Skizzen zu einem Gesetzbuche der Natur. Neu Auflage, Leipzig. 1825.

St. Bartholomew's Hospital—Reports, Vol. XII. 8vo. 1877.

St. Petersburg, Académie des Sciences—Bulletins, Tome XXIV. Nos. 2, 3. 4to. 1877.

Shepherd, A. B. M.D. M.A. M.R.I. (the Author)—Goulstonian Lectures on the Natural History of Pulmonary Consumption. 8vo. 1877.

Smithsonian Institution, Washington, U.S.—Annual Report for 1876. 8vo. 1877.

Symons, G. J.—Monthly Meteorological Magazine, Dec. 1877 and Jan. 1878. 8vo.

Tennant, Professor—D. Jeffries: Treatise on Diamonds and Pearls. 4th edition. 16to. 1871.

Twining, Thomas, Esq. M.R.I.—Reference Catalogue of Current Literature. 8vo. 1875.

United Service Institution, Royal—Journal, No. 93. 8vo. 1877.

United States Geological Survey—Ninth Annual Report: by F. V. Hayden, for 1875. 8vo. 1877.

United States Government Survey—Elliott Coues: Fur-bearing Animals. 8vo. 1877.

Upsal Royal Society of Sciences—Nova Acta: In Memoriam Quattuor Seculorum ab Universitate Upsaliensi peractorum. Volumen Extra Ordinem. 4to. 1877.

Vaux, W. S. W. Esq. M.A. F.R.S. M.R.I. (the Author)—Greek Cities and Islands of Asia Minor. 16to. 1877.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, 1877: Heft 9, 10. 4to.

Wilde, M. H.—Repertorium für Meteorologie, Band V. Heft 2, and Supplementband, Erste Hälfte. 4to. 1877.

Woods, Edward, Esq. C.E. M.R.I. (the Author)—On Adequate Brake-power for Railway-trains. (K 102) 8vo. 1877.

Yorkshire Archæological and Topographical Association—Journal, Parts 17, 18. 8vo. 1877.

WEEKLY EVENING MEETING,

Friday, February 8, 1878.

THE LORD ARTHUR RUSSELL, M.P. Vice-President, in the Chair.

MATTHEW ARNOLD, Esq.

Equality.

[This Discourse is printed in full in the 'Fortnightly Review' for March, 1878, vol. xxiii. N.S. p. 313.]

WEEKLY EVENING MEETING,

Friday, February 15, 1878.

WILLIAM SPOTTISWOODE, Esq. M.A. LL.D. Tr. R.S. Secretary and Vice-President, in the Chair.

P. L. SCLATER, Esq. M.A. Ph.D. F.R.S.

Zoological Distribution, and some of its Difficulties.

AFTER pointing out that "locality" is quite as much a part of the proper characters of natural groups of animals as form and structure, the lecturer spoke of "specific" and "generic" areas, and of the doctrine of their continuity. He then treated of "representative species," and showed that, while insular representative species are usually distinct, continental representative species are not unfrequently found to be connected together by intermediate forms. The only hypothesis that would explain these and other phenomena of "distribution" was that of the derivative origin of species. But the question was, were there not exceptional cases of distribution which threw difficulties in the way of the universal adoption of this hypothesis? It must be admitted by all who had studied distribution in any group of animals that there were many such difficult cases. The lecturer then proceeded to call attention to six cases of abnormal distribution in the classes of mammals, birds, and reptiles, namely:—

1. *The Little Blue Magpie of Spain.*—The general character of the birds of Spain did not differ materially from that of the rest of Southern Europe, although a few North African species intruded into its limits. One little bird only seemed to have been introduced from afar, and disturbed the general uniformity. The little blue magpie of Spain (*Cyanopica cooki*) had not only no near relatives in the rest of Europe, but we must go to the farthest part of Siberia and Northern China before we met with its true allies. Here was found the *Cyanopica cyanea*, so closely allied to the Spanish bird as to be barely distinguishable. This was, therefore, an undoubted instance of a discontinuous generic, if not specific, area, and in order to bring it within ordinary rules it was necessary to suppose that the parent-form had been formerly existent throughout Europe and Central Asia, but had for some reason become extinct in those countries.

2. *Oxyrhamphus* and *Neomorphus*.—These two South American genera of birds offered somewhat parallel cases of broken distribution. Of the peculiar Passerine form *Oxyrhamphus*, only two very closely allied species were known, one (*O. flammiceps*) in South-eastern Brazil, and the other (*O. frater*) in Central America, the genus being quite unrepresented in the intermediate countries. In the Cuculine genus *Neomorphus*, the Central American form (*N. salvini*) was again

very nearly similar to the Brazilian (*N. geoffroyi*), whereas in the intermediate countries three other quite distinct species were known to occur.

3. *Pitta angolensis.*—Not less than from thirty to forty species of the brilliantly coloured birds of the genus *Pitta* were known to science, distributed from India, on the north, through the great Asiatic islands into Northern Australia. But one single *Pitta*, in every way typical in structure, and closely allied to an Indian species, occurred in a limited district of Western Africa, the genus being quite unknown in intermediate localities. This was a clear instance of a discontinuous generic area.

4. *The Solenodon of the Antilles.*—The insectivorous mammals, according to the best authorities, constituted ten different families, which were mostly restricted to the Palearctic, Indian, and Ethiopian regions, and were entirely unrepresented in Australia and South America. Two families only extended into the nortion portion of the New World, the moles (*Talpidæ*) and the shrews (*Sorecidæ*). But there was one very exceptional case. The genus *Solenodon*, two species of which were known from two islands in the West Indies, belonged not to the shrews or moles, but to the family *Centetidæ*, otherwise entirely confined to Madagascar. If, therefore, the descent of *Solenodon* and *Centetes* from a common ancestor were assumed, the following assumptions must also be made. First, that the West India Islands had been united by land to Africa; and secondly, that the *Centetidæ* had formerly extended all through Africa, where there were now no traces of them.

5. *The Distribution of Lemurs.*—Recalling *Solenodon* to our minds, we might well have expected that the Lemurs, one of the most prevalent and characteristic mammal groups of Madagascar, would have had allies in America, but such was not the case. The only members of this group not found in Madagascar were met with in Africa and parts of the Indian region. It was therefore manifest that, assuming the origin of the Lemurs from a common source, a continent must have formerly existed in the Indian Ocean, and formed the ancient home of the Lemurine family, of which the fragments were now so widely sundered. It would, however, be difficult to reconcile this hypothesis with that of the former land-connection of Madagascar with the Antilles through Africa, previously adverted to.

6. *The Giant Land Tortoises.*—The giant land tortoises, which had lately formed the subject of the elaborate studies of Dr. Günther, presented a still more extraordinary instance of anomalous distribution. These animals now only existed in the Galapagos Islands and on the coral reef of Aldabra, north-west of Madagascar, but a third group, which formerly inhabited the Mascarene Islands, had only recently become extinct. In order to derive these three groups of allied species from the same stock, it would be necessary to assume first that Giant Land Tortoises were formerly distributed all over South America and Africa, where no traces of them now existed; secondly,

to suppose that the Galapagos were formerly united to America; and, thirdly, that the Aldabra reef had once formed part of land that was joined to the African coast. But even then all the difficulties would not have been surmounted, for it appeared that the Mascarene form of these tortoises was more nearly allied to that of the Galapagos than to that of Aldabra. It would further have to be assumed therefore, in order to bring these facts into harmony with the usual theory, that the Mascarene Islands had remained united to the African coast after the Aldabra reef had been separated from it.

These six cases were only selected instances of the many difficulties met with in endeavouring to account for all the known facts of distribution by the hypothesis of the derivative origin of species. It would be easy for those who had studied distribution in any group of animals to add to them almost indefinitely. Two other more general phenomena of distribution, which it appeared to be difficult to reconcile with the derivative hypothesis, were also briefly adverted to, those were the existence of "tropicopolitan" forms, that is, of forms common to the tropics of both hemispheres, and the presence of several closely allied species in the same area. In the first case, it was difficult to understand where the continent could have formerly existed which afforded a home to the ancestors of the similar species now so widely separated. In the second place, it never appeared to have been explained satisfactorily how more than one form could have succeeded to a pre-existing one in the same area, and the hypothesis that allied forms had always originated in separate areas, and had come together into the same area by immigration, appeared, in some cases to be almost untenable.

These and other minor difficulties had led the author rather to question whether identity of structure must be taken, *without exception*, as an indication of immediate descent from a common parentage. At any rate, the subject seemed to be one still open for discussion, and not, as some recent writers had appeared to assume, a matter which must be regarded as fully and incontrovertibly set at rest.

[P. L. S.]

WEEKLY EVENING MEETING,

Friday, February 22, 1878.

Warren de la Rue, Esq. D.C.L. F.R.S. Vice-President,
in the Chair.

Professor Odling, M.A. F.R.S. *M.R.I.*

The New Metal, Gallium.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, March 1, 1878.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

R. Liebreich, M.D. M.R.C.S. *M.R.I.*

The Deterioration of Oil Paintings.

Oil paintings are subject to various kinds of changes, which may be considered as diseases, requiring different treatment according to their different nature. A science needs to be formed, a pathology and therapeutics of oil paintings. The pathology would have to describe and explain those diseases and their progress, and to develop the methods by which a correct diagnosis could be arrived at in each individual case. The therapeutics would teach the remedies which might be applied either to cure or to alleviate the disease, or at least to stop its progress. A hygiene would follow, which would have to teach how to avoid pernicious influences, and which, besides, while giving precepts for the technical process of painting, would have to forestall those constitutional diseases which, even in cases where no noxious influences can be traced, are the causes of decay, after a comparatively short period of existence. As medical science is above all things based on Anatomy and Physiology, so the exact knowledge of the structure of a picture would have to be acquired previously to any study of its disease. Unfortunately, direct investigation alone can procure no such exact knowledge: on the contrary, we are obliged to enter upon a minute historical investigation of the material as well as of the technical methods adopted by artists of different schools and different periods.

The excellent works of Cennino Cennini, Mérimée, Sir Charles Eastlake, Mrs. Merrifield, and others, have already furnished most valuable material; but still the field for investigation remains unlimited; for, in order to enable us to secure the conservation of each valuable painting, we ought to know exactly how it was made. The artists of the present time would spare infinite trouble to the investigators of future times, if, along with their works, they would leave the account of their practice in the case of each picture. A treatment without exact knowledge of the normal condition, as well as of the nature of the disease, is, as we shall see, as dangerous for the picture as it would be in the case of living beings.

Professional restorers of pictures admit this danger in a general way; each of them, however, is convinced that he himself, by his personal knowledge, skill, and care knows how to avoid it. The public pays too little attention to the subject, and therefore it occurred to me that it might be useful to give a short account of what we know about this question, of the changes to which oil paintings are exposed, as well as of the means either to avoid or to cure them.

We have to consider, first, the material on which the artist has painted, that is, as far as oil painting is concerned, principally wood and canvas.

Secondly, the priming, that is, the substance with which the surface was prepared in order to be made fit for painting.

Thirdly, the painting itself, that is, the pigments and vehicles used for it, and the liquids that were added during the painting, the mediums, meguilp, siccative, varnish, essential oils, &c.

Fourthly, the coat or coats of varnish spread over the picture.

The wood on which a picture has been painted may either warp, or get chinks in it, or become worm-eaten, or even altogether rotten. Against warping, the remedy usually applied is moisture. If the panel is very thick, it is first made somewhat thinner; then the back is moistened, and the picture is left to lie on its back for twelve to twenty-four hours, after which time it will be found to have become straight. Of course this must not be continued longer than necessary, otherwise the convex surface, instead of becoming plane, would become concave. When straight, the picture is kept so by beads which have to be adapted in a particular way, a certain degree of shifting being allowed for the expansion and contraction of the wood.

Cracks in the wood are drawn together by inserting pieces of wood of a special shape.

Sublimate solutions are employed to destroy worms.

Trifling losses of substance are replaced by cement. Small portions of rotten wood, not extending too near the painting, are cut out and replaced by wedge-shaped pieces. If, however, the greater part, or the whole substance of the panel, is rotten, the picture must be separated from it and transferred to new wood, or rather to canvas.

This was first tried by Hacquin in Paris, and was performed successfully upon many pictures, and, among others, upon one of Raphael's Madonnas, in the Gallery du Louvre, and upon Sebastian del Piombo's 'Resurrection of Lazarus,' now in the National Gallery. The process no longer appears so very marvellous; it is generally executed in the following way:—

First of all, the surface of the picture is pasted over with gauze and paper. After that the wood is made straight by moistening, or, if necessary, by making incisions with the saw, into which cuneiform pieces of wood are driven. By means of a tenon-saw the panel is to be sawn into little squares, which must be removed by a chisel, and in this way the thickness of the wood is reduced to half an inch; it is then planed until it becomes no thicker than paper, and the rest is removed by means of a knife and with the fingers. The painting being thus severed from its basis, it can be fixed on canvas, if the priming is sufficiently preserved. In the opposite case, a mixture made of chalk and glue, or something of the kind, must be put on first, and very evenly smoothed after being dry. This done, the new canvas has to be fixed upon it by means of a mixture of glue, varnish, and turpentine, and the substance of the picture pressed tightly and evenly against it by means of warm irons.

In order to avoid deterioration, the most minute precepts have been given for preparing the panel. It has to be taken from the best oak, or nut-trees, or cedars. The wood is to be cut into boards during winter-time, and kept till autumn before being dried; it can then be prepared only in the following spring, &c. It would certainly be preferable to give up wood panels altogether for large pictures, and only to think of means to make the canvas stronger. For small pictures, panels offer certain advantages, and can be more easily preserved from decay.

In the canvas we meet with the results of injuries or spontaneous decay. A rent may be mended by rags of linen stuck at the back of the picture. Even a hole may be filled up by pieces taken from other decayed paintings. If the picture is considerably damaged, it will be best to line it. But if the whole canvas is rotten and tattered, it will be preferable to sacrifice it by pulling off the threads one by one, after having secured the painting itself by pasting paper on the front of it. This done, the painting is transferred to another canvas in the same way as those removed from wood.

There are different modes of priming, which may be brought under two principal heads: the distemper and the oil priming.

1. The canvas is distempered by a mixture of chalk or plaster and paste, or glue, which may be laid on raw, unbleached canvas, or this latter may be beforehand prepared with glue or paste. Several coats of this mixture must be put on in succession, one being perfectly dry before the next can be applied. Many of the older oil paintings are painted on such ground. It has the advantage of being quicker prepared, of absorbing the excess of oil, of permitting the colour to enter into the priming, and to dry quicker, and, moreover, of containing a white absolutely innocuous to the other colours.

The inconveniences, on the other hand, are: that it more easily breaks, and under the influence of humidity separates from the canvas.

2. The oil priming consists of several coats of oil colours. As each of these must be perfectly dry before the next is laid on, and as, moreover, time must be given to the whole to dry completely before painting upon, in order to avoid the sinking in of the colours, the whole preparation is much slower than the distemper. Nevertheless it is now generally adopted.

Rey, in France, has pointed out a process which is a compromise between the two methods: he begins by distempering, and after several coats of distemper, having dried one after the other, he puts a coat of oil which, as it were, changes the distempered ground into an oil-colour ground.

With oil priming it is of importance that the principal colour be white-lead, to which are added comparatively small quantities of yellow, black, or other colours. For a whole century a school, that of Bologna, predominated in Italy, which abandoned this principle. During the second half of the 17th and the first half of the 18th century, most of the Italian masters of other schools followed its example. Probably for the purpose of obtaining more easily the desired effect of the chiaroscuro they painted on a brownish-red priming, which consisted of bolus mixed with umber. Not one of those pictures has kept its original colouring. Not only has the priming caused all the dark parts to grow much darker, but it has destroyed, or nearly so, all the glazing, so that only those colours can be recognized which either contain white, or are glazed on white. I can show you numerous instances of this, for, on account of the extreme fertility of this school, there is little difficulty in procuring pictures of masters of that time or of their pupils.

Wood priming does not require the same elasticity as that of the canvas, which ought to be capable of being rolled. Therefore the priming of the wood shows less variations. It is generally composed of chalk or plaster, tempered with starch, paste, size, or glue, and more or less thickly laid on. In some pictures of different centuries we find, either between the wood and the priming, or between the priming and the painting, canvas, and exceptionally even paper.

The diseases of the priming are not of a very complicated nature. They manifest themselves principally in three different ways: 1, by cracks in the priming itself: 2, by the severance of the priming from the painting; 3, by the severance of the priming from the wood or the canvas. The third disease is by far the most frequent, especially among pictures on canvas distempered with paste. If small pieces only are scaling off or blistering, they are fixed again to the ground by letting a solution of size pass between the detached part and the canvas, and pressing both gently together. If the deterioration extends over a considerable surface, the picture has to be lined. While this is being done, and while the gluing substance penetrates into the picture, the detached parts are pressed on again with slightly heated irons. If the whole priming threatens to come off, it will be better to take the picture entirely from the panel or canvas, and to transfer it to a new canvas.

I shall show you examples illustrating the before-mentioned points, and among them two pictures; one in oil, taken off from canvas, the other in tempera, taken off from wood. Both of them, strange to say, have escaped destruction without having been transferred to a new canvas, and without being covered with paper, as is usually done, before taking them off. They show you the painting by itself from both sides. I have, of course, used every precaution in bringing them safely over from Florence, where I happened to discover them carelessly stowed away among heaps of old pictures.

We come now to the most important part of the picture, the painting itself. We meet very often with the idea that the old masters had been in possession of colours, that is pigments, the knowledge of which has been lost, and that this accounts principally for the difference between the oil painting of the 15th and 16th centuries, on the one hand, and that of the 18th and 19th on the other. But this is a great mistake. We know perfectly well the pigments used by the old masters; we possess the same, and a considerable number of new ones, good as well as bad, in addition. In using the expression of good and bad, I am principally thinking of their durability. From this point of view the pigments can be placed under three headings:—

1. Those which are durable in themselves, and also agree well with the other pigments with which they have to be mixed.

2. Such as when sufficiently isolated remain unaltered; but when in contact with certain other pigments change colour, or alter the others, or produce a reciprocal modification.

3. Those which are so little durable that, even when isolated from other pigments, the mere contact of the vehicle, the air, or the light, makes them in time fade, darken, or disappear altogether.

The old masters used, without reserve, only those belonging to the first of these three categories. For those belonging to the second they imposed on themselves certain limits and precautions. Those belonging to the third they did not use at all.

That some of the modern masters have not followed these principles is not owing to a lost secret, but to the fact that they disregarded those well-known principles, and even consciously acted against them. In Sir Joshua Reynolds' diary, for instance, we read that in order to produce certain tints of flesh, he mixed orpiment, carmine-lake, and blue-black together. Now, orpiment is one of the colours of the second category, carmine-lake one of the third. That is to say: orpiment, as long as it remains isolated, keeps its brilliant yellow or reddish-orange colour; but when mixed with white-lead it decomposes, because it consists of sulphur and arsenic, and it, moreover, blackens the white-lead, because the sulphur combines with it. Carmine-lake, even if left isolated, does not stand as an oil colour, and therefore has been superseded by madder-lake.

Unfortunately some of the most brilliant colours are perishable to such a degree that they ought never to be used; yet, it seems to me, that just in one branch of art in which of late remarkable progress has been made, I mean landscape painting, the artists, in order to obtain certain effects of colour not easily to be realized, do not always resist the temptation to make use of a number of pigments the non-durability of which is proved beyond doubt. However that may be, I think it pretty certain that the pigments in themselves play only a subordinate part in the deterioration of oil paintings, and that the principal part belongs to the vehicle with which the colours are ground, and to the liquids which are added during the painting. I hope, therefore, you will excuse my making some elementary explanations about these liquids.

Oil and fat are bodies consisting of carbon, hydrogen and oxygen. They may be considered as salts in which glycerine, as a basis, is combined with different acids, stearic acid, palmic acid, oleic acid. If oil is exposed to the air, it changes; certain kinds of oil remain liquid; others become thicker and darker, and are gradually transformed into hard and opaque bodies. The drying of oils is based upon a chemical process, during which the oil oxidizes by absorbing oxygen from the air, and combining a part of it with carbon to form carbonic acid, and another part with hydrogen to form water. The different oils dry with different rapidity, but this rapidity may be modified by the presence of certain substances, or by certain treatment. Linseed oil, for instance, according to the way in which it has been pressed out of the seed, contains more or less mucilaginous substances. These latter impede the drying of the oil, and have therefore to be removed by a refining process. If linseed oil in a shallow vessel is exposed to the air and light, and especially to a green light, it soon begins to dry, and is transformed first into a kind of varnish and gradually into a solid opaque substance. The drying may be quickened by boiling, and more particularly by the addition of lead, zinc, or manganese. In this way a quick-drying oil varnish may be prepared and used as a siccative. It follows that there are certain substances which impede the drying of oils, and others which facilitate it. Amongst the pigments are some which belong to this category of bodies, white-lead, zinc-white, minium, vermilion, for instance, facilitate the drying; others, such as ivory-black, bitumen, madder-lake, will impede it. Supposing now we should add to each of the different pigments the same quantity of oil, the drying of it would progress at different rates. But in reality this difference is very greatly increased by the fact that the different pigments require very different quantities of oil, in order to be ground to the consistency requisite for painting.

Pettenkofer quotes the following figures, given to him by one of the colour manufacturers:—

100 parts	(weight)	White-lead	require	12	parts of oil.
,,	,,	Zinc-white	,,	14	,,
,,	,,	Green chromo	,,	15	,,
,,	,,	Chrome-yellow	,,	19	,,
,,	,,	Vermilion	,,	25	,,
,,	,,	Light red	,,	31	,,
,,	,,	Madder-lake	,,	62	,,
,,	,,	Yellow ochre	,,	66	,,
,,	,,	Light ochre	,,	75	,,
,,	,,	Camel's-brown	,,	75	,,
,,	,,	Brown manganese ..	,,	87	,,
,,	,,	Terre verte	,,	100	,,

100 parts (weight)	Parisian-blue	require	106	parts of oil.
" "	Burnt terre verte ..	"	112	"
" "	Berlin-blue	"	112	"
" "	Ivory-black	"	112	"
" "	Cobalt	"	125	"
" "	Florentine-brown ..	"	150	"
" "	Burnt terra sienna ..	"	181	"
" "	Raw terra sienna ..	"	240	"

According to this table a hundred parts of the quick-drying white-lead are ground with twelve parts of oil, and on the other hand, the slow-drying ivory-black requires one hundred and twelve parts of oil.

It is very important that artists should have an exact knowledge of these matters. But it seems to me that they are insufficiently known to most of them. All, of course, know perfectly how different the drying quality of different colours is. But that these different colours introduce into the picture so different a quantity of oil, and how large this quantity is in the colours they buy, and further, that the oil as well as the mediums or siccatives they add to dry the colours, are gradually transformed into a caoutchouc-like opaque substance, which envelops and darkens the pigments; and moreover, that the oil undergoes—not in the beginning, but much later on when it is already completely dry—changes of volume, and so impairs the continuity of the picture,—all this is not sufficiently known. Otherwise, the custom of painting with the ordinary oil colours to be bought at any colourman's, would not have been going on for nearly a hundred years in spite of all the clearly shown evil results; results due, chiefly, TO THE PRINCIPAL ENEMY OF OIL PAINTING, THAT IS TO SAY, THE OIL.

That the masters of the 15th and 16th centuries did not use colours prepared in this way, you may consider as absolutely certain; and if we hear the lost secret spoken of, and if we read that the pupils of the old masters had to pledge themselves to keep the secret, we may be sure that it is neither the method of painting nor the pigment used for it which is concerned in that secret, but exclusively the way of preparing the colours. The preparation was a very complicated one, varying with the different pigments; and we know that the pupils passed six years, that is half of the apprenticeship, in grinding the colours for the master.

And therefore it is to this very point that everyone who wishes to study the method of the old masters must first of all direct his attention. I, too, was led by the study of this question, to analyze and restore old pictures. The possibility of making such analysis we owe to the relation between the old masters and their pupils. Of course we could not dissect or chemically analyze works of Titian or Raphael. But fortunately the pupils painted with the same material and by the same method as the masters, and thousands of pictures by the pupils, *well preserved* or in different stages of decay, may be easily procured.

I have myself, from among a very great number of such pictures, selected about one hundred specimens, part of which I have brought before you. As their artistic value is not, as you perceive, of the highest description, we need not feel any scruple in experimenting upon or even destroying them, if we can thereby gain any valuable information.

If we compare the pictures of the Italian and Dutch schools of the 15th, 16th, and 17th centuries, with those of the French and English schools of the last hundred years, we are struck by the great difference in the nature of their diseases. We may divide those diseases into *constitutional* ones—that is to say, such as are based on the method and the material used for painting, and into those produced by external influences.

The Dutch pictures of the 15th, 16th, and 17th centuries, and the Italian pictures of the 15th and 16th centuries, seem to me perfectly free from constitutional diseases. It is only in the 17th century that the Italian pictures show a special constitutional alteration, caused by the practice of the Bologna school.

The pictures of the last hundred years of the French school, of a part of the English school, and some painters of other schools, have been attacked by a constitutional disease perfectly defined and characteristic of this period.

Among external influences injurious to oil painting, we have to consider dampness, heat, bad air, dust, smoke, mechanical injuries, and last, not least, the destructive, or "altering" hand of the picture-restorer.

Pettenkofer's scientific researches first clearly defined the influence of humidity on oil paintings, showing that it produced a discontinuity of the molecules of the vehicle and the resinous substances. As glass, when pulverized and thereby mixed with air, loses its transparency, and water, when mixed with oil, becomes of a milky aspect, so the oily and resinous substances contained in paintings will become dim as soon as air penetrates between their particles. The picture thus assumes a greyish, dim appearance, and the pigments seem to have been fading. That this is not really the case has been proved by the influence of a process invented by Pettenkofer, which he calls regeneration. In a flat box the picture is exposed to air impregnated with alcohol. Of this latter, the resinous elements of the picture absorb a certain quantity, swell and fill up the interstices between the separated particles so as to reunite them into an optically homogeneous, transparent substance.

The alcohol does not affect in the same way the hardened oil. If the interstices between its particles are not filled up by the swelling resin, it becomes necessary to introduce a new substance into the picture, and this is called nourishing a picture.

Pettenkofer has the great merit of having clearly proved that the nourishing of a picture with oils, as the custom was formerly, and still is to some degree, is a very objectionable proceeding, as it has the effect of darkening the colours for ever. He recommends, instead of oil, balsam of copaiva, which has since become an invaluable means for preserving and restoring oil paintings, and will be more and more extensively used.

I have frequently applied Pettenkofer's method, and with very beneficial effect; but whenever I mentioned it to professional picture-restorers, here as well as on the Continent, I always found them to reject it, either *à priori*, or after experiments incorrectly made.

In Munich, it seems, the pictures of all periods and of all schools have had to suffer under local influences and through the changes in the humidity of the air. This accounts for Pettenkofer having principally described this, so to say, endemical disease. In other galleries this affection does not appear so frequently, and Pettenkofer's method, therefore, will not find everywhere the same extensive application as at Munich. I think, however, that with some modifications it may be employed against some other alterations. I have, for instance, found it efficacious with paintings which had been injured by exposure to great heat. I shall show you a small picture which had been hanging for a long time so near a gas flame that it was almost completely scaling off, and so entirely faded that it scarcely looked like an oil painting at all. In that state it was exposed to alcoholized air, then nourished with balsam, and its back slightly varnished; and the scales starting from the canvas were refixed by pressure. And now it appears fresh in colour, firm in substance, and perfectly smooth on its surface. The old, cracked varnish, melted together by the alcohol, looks as if fresh laid on.

Humidity sometimes favours the development of fungus. The round, black, small spots which pass through the canvas and the painting of these two pictures are produced by the same little plant which Professor Tyndall showed you when he spoke on the highly interesting subject of spontaneous generation.

Oil and water, so injurious to oil paintings, enter both into the material used for lining. Anxious to exclude these sources of danger, and to simplify the whole process, I have endeavoured to replace it by a new method which I shall submit to you this evening.

How paintings may be disfigured by restorers you see in this picture, which was renovated with oil colours according to the practice only abandoned about thirty years ago, when it was advantageously replaced by the use of varnish colours.

The amount of external injury oil paintings sometimes endure and stand is perfectly amazing. Pictures in the course of centuries, during the destructive fury of wars and revolutions, may have been torn out of their frames, rescued from below the ruins of burned monasteries, may subsequently have passed from one bric-à-brac shop to another, where they have been piled up, to be pulled about at each new inspection, and literally trodden under foot, whereby they have finally been reduced to a state of colourless, greyish, or black rags. Still such pictures may not unfrequently be awakened, as it were, to new life, to their original brilliancy of colour; if, with all necessary care, their injured limbs are put together again, their wounds are healed, and fresh nourishment, air, and thorough cleansing, are administered to their lacerated bodies.

A sound constitution is, of course, a necessary condition for obtaining any such result, without it we can only obtain a partial cure. We see this with reference to the Bologna school of the 17th century. The pictures which you see here are instances of this. From the state of rags to which they were reduced they have passed, by appropriate treatment, into the state of firm, even, well-conditioned, and clean pictures. The constitutional alteration characteristic of their time and school, however, could not be cured. You will, therefore, perceive that the contrast is too great between light and shade, that the half tones are too weak, and that the glazings spread on dark ground, which certainly existed formerly, have been destroyed by the growing of bolus and umber of the priming. That this is not the fault of the method of restoration is clearly proved by the state in which you will find all the pictures of this school, even those best preserved in the best galleries of all countries.

The constitutional diseases of pictures belonging to the French and to the English school of the last hundred years are of still more serious nature, and much more difficult to cure. Many of them, though they were never exposed to any injury whatever, nor are likely ever to be so in our present state of civilization, cannot be guarded from premature decay in spite of all possible care with which they are kept.

The principal symptoms of their bad constitution are:—

1. Darkening of the opaque bright colours.
2. Fading of the transparent brilliant colours.
3. Darkening, and above all, cracking of the transparent dark colours.

The best opportunity to study these several appearances is given us in the Museum of the Louvre, which contains a great number of such pictures in the section occupied by the French school. I have paid particular attention to the cracks in these pictures, as I find that in shape, in size, in position, as well as in relation to the various colours, they differ distinctly from the cracks in older pictures and in those of other schools. This, of course, is of importance, not only for the explanation of the reasons which produced them, but as a symptom which, in a given case, might determine the diagnosis, whether a picture be an original or only a copy. The special characteristics of these cracks are the following:—

They are all but exclusively found in the thickly laid on transparent dark colours, and they are the deeper and the more gaping in proportion to the thickness of the layer of the colour and the extent of the dark surface. The chief cracks run parallel to the outlines of surfaces painted with bright opaque colours, such, for instance, as are used for the flesh tints, and which are more or less thickly laid on.

But there is generally a slight distance between the bright colours and the cracks.

Lateral branches of these cracks pass into the white, but they do not gape, provided the white colours had been laid on directly upon the priming, and not upon a layer of dark transparent and not sufficiently dried colour.

This examination of the cracks of pictures has sometimes afforded me a peculiar insight into the practice used for the picture. In the well-known picture, for instance, by Guèricault, of "The Wreck of the Medusa," in the Gallery of the Louvre, the cracks follow exactly the outlines of the bright flesh-tints. The arm of one of the dead bodies hanging in the water is so covered by planks and water that nothing of the forearm is to be seen. It is, however, very easy to prove that originally that arm was painted in all its length, for the cracks do not only follow the outline of the visible upper arm, but also the no longer visible forearm, and all the five fingers. This proves that the fore-part of the arm and the hand were originally painted in flesh tints before they were covered over by the planks, and the water painted afterwards. In Ingres' portrait of Cherubini, the face of the latter is beautifully preserved, whilst that of the Muse, as well as her drapery, is covered with cracks. In the depth of the cracks of the white drapery, an intense blue tint is to be seen. Mr. Henri Lehmann, of Paris, the favourite pupil of Ingres, who knows the history of this picture as an eye-witness, and whom I consulted about this very striking appearance, gave me the following information:—Ingres painted the head of Cherubini in Paris, and then took it with him to Rome. There it was pieced into a new canvas and lined. Then the Muse was painted, and before the colours were perfectly dry, another model was chosen, and a new Muse painted over the old one. The colour of the drapery was likewise altered, and this explains the cracks in the white colour, and explains also why the blue appears in the depth of the cracks of the drapery.

Among the English artists of the last hundred years, some have painted with the same material and by the same process as their French contemporaries, and consequently with the same unfortunate results. Others avoided these by using the same material with more precautions. Others, again, and among them Sir Joshua Reynolds, have in their different works followed various practices, and consequently had varied results. Thus some of Sir Joshua's pictures have kept perfectly sound. Others are cracked in the characteristic way just mentioned. Others, again, are cracked in an absolutely irregular way. We can easily form an idea of it, if we read in his 'Diary Notes,' for instance, the way in which he painted the portrait of Miss Kirkman, which he began with whiting and gum tragacanth, then covered it successively with wax, then white of eggs, and then varnished it.

The study of the alterations already fully developed in pictures painted within the last hundred years only, and their comparison with the works of the old masters, would suggest the following rules for the process of painting:—

1. The oil should in all colours be reduced to a minimum, and under no form should more of it than absolutely necessary be introduced into a picture.

2. All transparent colours which dry very slowly, should be ground not with oil at all, but with a resinous vehicle.

3. No colour should be put on any part of a picture which is not yet perfectly dry; and, above all, never a quick-drying colour upon a slowly drying one, which is not yet perfectly dry.

4. White and other quick-drying opaque colours may be put on thickly. On the contrary, transparent and slowly drying colours should always be put on in thin layers.

If the effect of a thick layer of these latter is required, it must be produced by laying one thin layer over another, taking care to have one completely dry before the next is laid on. If transparent colours are mixed with sufficient quantity of white-lead, they may be treated like opaque ones.

We come now to the last layer of the picture, to that one which is spread over its surface in order to equalize optical irregularities, and to protect it at the same time from the air. I mean the varnish.

The varnish may crack or get dim, then it should be treated by Pettenkofer's method; but it may become dark yellow, brown and dirty, and so hide the picture that it becomes necessary to take it off and to replace it by a thin layer of new varnish. It is here that picture restorers, or we may say picture cleaners, display their beneficial skill, and also their very destructive activity.

If a picture is throughout painted in oil, if its substance has remained sound and even, and varnished with an easily soluble mastich or dammar varnish, then there will be neither difficulty nor danger in removing the varnish. This can, in such a case, be done either by a dry process, that is, by rubbing the surface with the tips of the fingers, and thus reducing the varnish by degrees to a fine dust, or by dissolving the varnish by application of liquids, which, when brought only for a short time into contact with the oil painting, will not endanger it. We have, however, seen that the works of the old masters are not painted with oil colours like those used by modern painters, but, on the contrary, that certain pigments, and especially the transparent colours used for glazing, were ground only with resinous substances. These latter have, in the course of time, been so thoroughly united with the layer of varnish spread over the surface of the picture, that there no longer exists any decided limit between the picture and the varnish. It is in such pictures that a great amount of experience, and knowledge of the process used for the picture, as well as precaution, are required in order to take away from the varnish as much only as is indispensable, and without interfering with the picture itself. Numberless works of art have been irreparably injured by restorers, who, in their eagerness to remove dirt and varnish, attacked the painting itself. They then destroyed just that last finishing touch of the painting, without which it is no longer a masterpiece.

The difficulty and danger are much greater in cleaning those pictures which have not been varnished with the ordinary easily-dissolved mastich or dammar varnish, but have been painted over with oil, oil-varnish, or oleo-resinous varnish. It seems incredible that these substances should ever be used for such purposes; it is, however, a fact that there are still people who fancy that it will contribute to the good preservation of their pictures to brush from time to time a little of those liquids over their surface. They recognize too late that the varnish becomes more and more dark, of a brownish colour, and opaque. If such varnish has afterwards to be removed, then we meet with the great difficulty, that this can be done only with substances which would just as easily dissolve the whole picture as the hardened layers spread over it.

This shows what can be the value of those universal remedies which from time to time appear, and are praised for the innocuous way in which pictures by their means may be cleaned.

There is at this moment a great discussion going on in Italy about Luporini's method. Luporini is a painter and picture-restorer in Pisa, who believes himself to have invented a new means of cleaning pictures without any danger. Some months ago, in Florence, I examined a large number of pictures cleaned by him. Those of the Gallery of St. Donato, belonging to Prince Demidoff, mostly Flemish and Dutch landscapes, are cleaned very well and without any injury to the painting. On the contrary, the St. John, by Andrea del Sarto, one of the finest pictures of the Palazzo Pitti, I found very much altered by the restoration of Luporini. I had studied that picture very closely the year before, and should now sooner believe it to be a modern copy than the cleaned original. It has lost all softness of outline, and the characteristic expression of the face. The change in the flesh tints can scarcely be explained otherwise than by an entire removal of the glazing.

I think it is taking a heavy responsibility to allow a new experiment to be tried upon such an invaluable work of art. Even private persons, who are fortunate enough to be in possession of such treasures, ought to feel responsible for the good preservation of masterpieces, which are, it is true, their material property, but which intellectually belong to the whole civilized world of the present and of the future.

[R. L.]

GENERAL MONTHLY MEETING,

Monday, March 4, 1878.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Earl of Denbigh,
The Lady Louisa Mills,
Charles Aldin, Esq.
William Aldin, Esq.
John Ashby, Esq.
The Lady Belcher,
Mrs. Burton Borough,
Henry Edmunds, jun. Esq.
Joseph Findlater, Esq.
Mrs. Goodall,
Thomas Gregory, Esq.
Francis Harris, M.D. F.L.S.
Adolphus F. Janvrin, Esq.
Mrs. W. S. Kirkes,
Charles J. Longman, Esq.
Charles Marett, Esq.
William Henry Michael, Esq.
Mrs. Richard Cornwallis Moore,
Thomas Morson, Esq.
Rudolf Neele, Esq.
Mrs. Charlotte May Norman,
William Robert Phelips, Esq.
Arthur Rokeby Price, Esq.
Rev. Alexander Taylor, M.A.
Walter Weldon, Esq. F.R.S.E. F.C.S.
Miss Florence Caroline Wheatstone,

were *elected* Members of the Royal Institution.

The Special Thanks of the Members were given to **Warren De la Rue**, Esq. D.C.L. for his Donation of Fifty Pounds for the benefit of the Chemical Laboratory, and of Apparatus for the Liquefaction of Gases for the Physical Laboratory.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Lords of the Committee of Council on Education—Conferences held in connection with the Special Loan Collection of Scientific Apparatus, 1876. 2 vols. 8vo. 1877.

Academia dei Lincei, Rome—Atti, Serie III. Transunti, Vol. II. Fasc. 1, 2. 4to. 1878.

Agricultural Society of England, Royal—Journal, Second Series, Vol. XIII. Part 2. 8vo. 1877.

American Philosophical Society—Proceedings, No. 88. 8vo. 1872.

Astronomical Society, Royal—Monthly Notices, Vol. XXXVIII. No. 3. 8vo. 1878.

British Architects, Royal Institute of—Sessional Papers, 1878. No. 7. 4to.

British Museum Trustees—Catalogue of Spanish MSS. Vol. II. 8vo. 1877.
Catalogue of Gigantic Land Tortoises. 4to. 1877.

Cox, Edward W. Esq. S.L. M.R.I. (the Author)—Monograph on Sleep and Dream. 8vo. 1878.

Editors—American Journal of Science for 1878. 8vo.
Analyst for Feb. 1878. 8vo.
Athenæum for Feb. 1878. 4to.
Chemical News for Feb. 1878. 4to.
Engineer for Feb. 1878. fol.
Horological Journal for Feb. 1878. 8vo.
Iron for Feb. 1878. 4to.
Journal for Applied Science for Feb. 1878. fol.
Nature for Feb. 1878. 4to.
Nautical Magazine for Feb. 1878. 8vo.
Pharmaceutical Journal for Feb. 1878. 8vo.
Telegraphic Journal for Feb. 1878. 8vo.

Franklin Institute—Journal, No. 626. 8vo. 1877.

Gray's Inn, Honourable Society of—Supplement to Catalogue of the Library. 8vo. 1878.

Harrison, W. H. Esq. (the Editor)—'Rifts in the Veil': Inspirational Poems and Essays; by Spiritualists. 12mo. 1878.

Meteorological Society—Quarterly Journal, No. 24. 8vo. 1878.

Pharmaceutical Society of Great Britain—Catalogue of Museum, 1878. 8vo.

Photographic Society—Journal, New Series, No. 4. 8vo. 1878.

Preussische Akademie der Wissenschaften—Monatsberichte: Nov. 1878. 8vo.

Royal Observatory, Edinburgh—Astronomical Observations, Vol. XIV. 1870–7. 4to. 1877.

Royal Society of London—Philosophical Transactions for 1877. Vol. CLXVII. Part 1. 4to. 1877.

Statistical Society—Journal, Vol. XL. Part 4. 8vo. 1877.

Telegraph Engineers, Society of—The Telegraph Pocket-Book for 1878. 12mo.

United States Coast Survey—Meteorological Researches, Part 1. 4to. 1877.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, 1878: Heft 1. 4to.

WEEKLY EVENING MEETING,

Friday, March 8, 1878.

THE DUKE OF NORTHUMBERLAND, D.C.L. President,
in the Chair.

PROFESSOR GOLDWIN SMITH.

The Influence of Geographical Circumstances on Political Character.

Two instances were taken as illustrations, one from ancient and the other from modern history—from ancient history Rome, and from modern history England.

In the case of Rome, the influence of geographical circumstance was traced in her selection by nature as the principal seat of military power, of political organization and of law. The speaker then proceeded to England.

We have two large islands close to a continent, and that continent which hitherto by natural election has been the chief seat of civilization. One island is much larger than the other, and the larger island lies between the continent and the smaller. The larger island is so placed as to receive primæval immigration from three quarters: from France; from the coast of Northern Germany and the Low Countries; and from Scandinavia, the transit being rendered easier in the last case by the prevailing winds and by the position of the Shetlands and the Orkneys. The smaller island can hardly receive immigrants except through the larger. The western parts of the larger island are mountainous, and it is divided into two very unequal parts by the Cheviots and the mosses of the Border. The larger island has extensive districts well adapted for grain: the climate of a great part of the smaller island is too wet for grain, and good only for pasture. The larger island is full of minerals and coal: of which the smaller island is almost destitute. These are the most salient features of the scene of English history.

What, politically speaking, are the special attributes of an island? In the first place it is likely to be settled by a bold and enterprising race. Migration by land, under the pressure of hunger, calls for no great effort of courage or intelligence on the part of the nomad. Migration by sea does: we can hardly realize now the daring which was needed to put forth on a strange element, and especially to go out of sight of land. Of the two elements which make up our nationality the Celtic had only to pass the Channel which you can see across; but the Teutonic, which is the dominant element and the basis of our character and institutions, had to pass an ocean which you cannot see across. From Scandinavia especially England received, under the form of freebooters, who afterwards became conquerors and settlers, the very core and sinews of her maritime population, the progenitors of the Blakes and Nelsons. The Northman, like the Phœnician, had a country too narrow for him, and timber for ship-building at hand. But the land of the Phœnician was a lovely land which bound him to itself; and wherever he roved, his heart was still in the pleasant abodes of Lebanon and the sunlit quays of Tyre. Thus he became a merchant, and the father of all who have made the estranging sea a bond and highway between nations. The land of the Scandinavian was not a lovely land; he was glad to exchange it for a sunnier dwelling place, and he became the founder of Norman kingdoms. One can hardly help lingering over these primæval mariners, for there is no romance in history equal to theirs. In our days science has gone before the most adventurous keel, limiting possibilities, and disenchanting the universe of Ulysses and Sindbad; but the Phœnician and the Northman put off into a really unknown world. Not only is a race which comes by sea likely to be peculiarly vigorous, but the very process of maritime migration would intensify the spirit of freedom and independence. Timur or Genghis Khan, sweeping from land to land with the vast human herd under his command, becomes more despotic as the herd becomes larger and the area of its conquests is increased. But a maritime migration is a number of little jointstock enterprises, implying common councils and a good deal of equality. It naturally results also in a number of small and separate settlements, which again is favourable to local self-government.

Another attribute of an island is freedom from invasion, which brings with it comparative immunity from standing armies. And perhaps we have owed more politically to this comparative immunity, than it is flattering to our self-esteem to suppose. Charles I. had no standing army, and the nation was then able to hold its own against the Crown; but the later Stuarts had learned wisdom from Charles's example; they had a standing army, and then the nation had to call in the Prince of Orange. To go back to an earlier period, John and his mercenaries would have beaten the Barons and annulled the Great Charter, if England had not been rescued at the critical moment by the ambition of the French prince. Navies are not political, they do not overturn constitutions.

A third consequence of insular position is isolation, especially in early times. An extreme case of isolation is Egypt, which is in fact a great oasis or island in the desert. The extraordinary fertility of the valley of the Nile produced an early development which was afterwards arrested by its isolation, artificially intensified by the jealousy of a powerful priesthood; and the great monuments of the early development remained unapproached, enigmatic, and mysterious for ever. England, by her insularity, escaped all permanent trace of Roman conquest; Scotland and Ireland escaped the conquest altogether, for the tide of invasion, which for a moment flowed to the foot of the Grampians, soon ebbed to the line of the great wall, the western wing of a system of defences which once guarded civilization from the Solway to the Euphrates. Instead of being a land of Roman provincials with a ruling Teutonic race, like France or Spain, England became purely Teutonic, and for a time was lost to Christendom and civilization. The missionary monk recovered what the legionary had lost. When Ethelbert advanced to meet Augustine on the beach of Kent, Teutonism was united to Christendom, to ecclesiastical, and to what remained of ancient Rome. The fifth great element of our political character, classical republicanism, came in at a later day. Still after the conversion, the English Church remained national, and to the eye of a Roman High Churchman half schismatic: to bring her into perfect union and obedience was the object of Rome in seconding the enterprise of the Norman Conqueror. Into much closer union and much stricter obedience she was brought; but she did not lose her insular character; and the Reformation, as a national, though not as a doctrinal revolt from Rome, commenced in England as early as the thirteenth century. In the sixteenth century the breaking up of the great federation of Christendom threw England back into an isolation tempered by her connection with the Protestant party on the Continent. Since that time the growth of European interests, of commerce, of international law, of international intercourse of every kind, of the community of intellect and science, have been building up again, on a sounder foundation than that of the Latin Church, the federation of Europe, or rather the federation of mankind. Of late the sympathy of England with European politics has been increasing in a marked way. But, in the past, England, while too close to the continent of Europe not to be a part of the European system, has been a peculiar and semi-independent part of it. She has acted as a sort of balancing and moderating power. She has been the asylum of vanquished parties and ideas. In the seventeenth century, when the Catholic and absolutist reaction prevailed on the Continent, she was the chief refuge of Protestantism and political freedom. When the French Revolution swept over the Continent, she threw herself into the reactionary scale. The tricolor has gone nearly round the world, at least nearly round Europe; but on the flag of England still remains the religious symbol of the pre-revolutionary era.

There are certain features of the insular character which we hear of sometimes from our neighbours. Their influence may be traced all through the history of our international relations—notably in the desperate effort to be rid of us which has immortalized Joan of Arc. They have made enemies to us even of those who have received liberation or other benefits at our hands. Among them is an insular incapacity, very unfortunate in the case of an imperial nation, for mingling with people of other races, or even for living on kindly terms with them. We have been the most beneficent of conquerors, and the least beloved.

It is needless to point out the effect of our insular position in developing maritime enterprise and trade with the corresponding elements of political character. We shall touch on this point presently in another connection. These islands lie most favourably for maritime enterprise and trade, not only with respect to Europe and the East, but also with respect to the Western hemisphere. It may be said that from its cradle the English race looked unconsciously across the Atlantic to an immense heritage in a then unknown world.

Let us now look at the divisions of the islands. The hills of Devon and Cornwall, the mountains of Wales, those of Westmoreland and Cumberland, those of the Highlands of Scotland, in the age of the steam engine are the asylum of natural beauty, poetry, and the hearts which seek repose from the din and turmoil of commercial life. In earlier days they, with seagirt Ireland, were the asylum of the weaker race. Here the Celt found refuge when Saxon invasion swept him from England and the Scotch Lowlands. Here he was preserved, with his own language, with his own Church—the Church of Britain before Augustine—with his un-Teutonic gifts and weaknesses, his quickness of intellect, his lively, social, sympathetic nature, his religious enthusiasm, his superstition, his clannishness, his devotion to personal leaders, his comparative indifference to institutions, and want of natural aptitude for self-government.

We always talk of Anglo-Saxons, and identify the extension of our empire with that of the Anglo-Saxon race. But taking all the elements of Celtic population in the two islands together, they must bear a very considerable proportion to the Teutonic element. That large Irish settlements are being formed in the cities of Northern England is seen from election addresses, to which we allude only in an ethnological point of view. In the competition of races on the American continent the Irish more than holds its own; and it is but fair to it to say that it owes its large increase not merely, as its detractors say, to economic recklessness, but to its morality and to the qualities of its women as mothers.

The Teutonic element has been the governing and moulding element, and has supplied, as I said before, the basis of our character and institutions; but the Celtic element is not to be left out of sight, especially since political enfranchisement has called it to the active exercise of power. The Celts of Devon and Cornwall were soon subdued, and were then gradually assimilated. The Celts of Wales have not yet been assimilated, and it was not till the reign of Edward I. that they were subdued. In their independent state, as enemies of the English monarchy, they allied themselves with all insurrections in England, and thus unintentionally rendered some service to English liberty in its perilous struggles with the power of the Crown. They lent aid to the Barons of Runnymede, and the Great Charter accordingly contains an article in their favour. In another form the influence of their separate action may even yet be felt in English politics. The Teutonic monarchy of England was powerful enough to subjugate the Celts of Wales; the Teutonic monarchy of Scotland was not strong enough to subjugate, or even to make any serious attempt to subjugate, the Celts of the Scotch Highlands. Beyond the Grampians the dominion of the Celt, with his language, habits, and institutions, remained unimpaired. The clan retained its pristine form. The isolation of the glens preserved its separate existence, and the nature of the country precluded the use of the horse, which by giving birth to a mounted class would have led to the growth of a nobility and destroyed the fraternal equality of the clansmen. The horse is an animal whose political significance is embodied in the terms for aristocracy, both ancient and modern. To show how this preservation of the clans has affected the course of English history we have only to mention the names of Montrose, Dundee, and the Pretender. As Macaulay truly says, these were clan movements, not dynastic rebellions, so far, at least, as the masses were concerned. Like all clan movements, they were little more than raids, and their effect was consequently transient and superficial.

Far deeper and more abiding has been the effect produced by the geographical boundary which ultimately cut off the Saxon settlements in the Lowlands of Scotland from the kindred settlements in England. One direct consequence of this was the weakness of the Scotch monarchy, which made the history of Scotland a perpetual tale of feudal anarchy, and, though Scotch historians do not much like to confess it, precluded the real development of Parliamentary institutions. Another was border war, which retarded civilization in the whole of Scotland and over the northern part of England, whence issued, in the Wars of the Roses, the host of barbarous marauders brought down upon the more civilized South by the partisans of Margaret of Anjou. But the most momentous consequence was a separate Scotch Reformation. In England, the Reformation being made by the Crown, Episcopacy was retained as congenial to monarchy. In Scotland, the monarchy being at that moment practically in abeyance, the Reformation was made by the people. Hence it assumed the more republican form of Presbyterianism. In Scotland, being made by the people, it had, together with a certain intellectual narrowness, the force and intensity of popular conviction. In England it was far weaker, and might have succumbed, with the political principles which were bound up with it, to the reaction under Charles and Laud. But Scotland stood firm and turned the day. The resistance of her Covenanting peasantry to the later Stuarts again largely contributed to the victory of Protestantism and liberty in 1688. Even down to the present day she has retained a political as well as a religious character of her own, and has exercised a special and a very marked influence on the course of political events.

Then as to the influence of the geographical position of England

and of its productions on the pursuits of the people and on their political character as it is formed by their pursuits. Commercial and manufacturing cities, as a rule, are proverbially the homes of the activity of mind and the quickness of sympathy which supply the element of progress. Even in the country men who work in gangs show something of the same spirit. Agriculture, on the other hand, is conservative, especially where the estates are large. Now, the maritime position and the mineral products of England have together called into existence, notably within the last century, great masses of commercial and manufacturing population with the progressive tendencies in politics which belong to it. On the other hand, we have also a large agricultural country, not, as Pericles called Attica, a mere garden and pleasure-ground of the commercial city, but sufficient to constitute not only a substantial but a preponderating interest; and this, as a rule, has laid its weight in the other scale. Thus there has been, not an equilibrium, but an alternate action of political forces which has kept the country, on the whole, in the line of sure and steady progress. In the great feudal kingdoms of Europe, where the cities were weak, the element of progress was wanting. In the commercial cities of Italy the conservative element failed; popular liberty sprang up like a gourd, and like a gourd it withered. In English politics ground has been slowly gained, but it has seldom been lost again; there have been violent oscillations, especially when political and religious struggles have come together; but there has been no final victory of reaction. We are speaking only of the past; as to the present and future, if we were not afraid of approaching any controverted question, we might perhaps be able to show that agencies are at work which will probably change the scene.

The very obvious facts just stated might be copiously illustrated, if illustration were needful, by reference to the history of party struggles in England. In the Middle Ages London was the heart of the reforming and progressive party. At the time of the Wars of the Roses, though there was not much principle on either side, of the two parties the Yorkists were the popular reformers, while Margaret of Anjou and her favourites were absolutists and reactionists; the banners of all the great cities were in the Yorkist ranks, while the Lancastrians drew their forces mainly from the North, which was then, economically and socially, the very opposite of what it is now. In the time of Charles I. the South and East were still the great seats of commerce, manufacturing industry, and wealth; and these districts were on the side of the Parliament. The King was strong in the North and West. The North was under the influence of the Royalist Marquis of Newcastle, in whom the powerful feudal lord of the Middle Ages was blended with the elegant grandee of the Renaissance, who raised regiments among his own retainers, and whose department was rather that of an independent prince than that of a subject. But Bradford and one or two other towns, in which manufactures were just beginning to rise, were Parliamentarian, and followed the standard of the Fairfaxes. In the West also commercial cities, such as Gloucester and Bristol, though completely within the King's country, were Parliamentarian. If a city was Royalist, it was under ecclesiastical influence. It is true that the most ardent supporters and the best soldiers of the Parliament were farmers; but those men were not tenants at will, they were small freeholders or leaseholders, called into existence to a great extent by the great territorial revolution which accompanied the Reformation; and moreover they, like their leader, were set in motion not so much by political zeal as by religion. Men of all classes indeed, even wealthy and titled landowners, were in that age raised to a remarkable extent by religious enthusiasm above material and social interests. There was here an important limitation to the range of physical influences, at all events to the range of physical influences of a direct kind; and it is the more to be noted, because the colony, the production of which was to humanity at large the most momentous result of the Revolution, was distinctly a religious colony, and succeeded on that very account, when gold-seeking and commercial colonies, notwithstanding all that maritime enterprise and everything which we can directly trace to physical influences could do for them, had failed.

It is needless to say how completely the political topography of England had been changed between the reign of Charles I. and the reign of William IV., or how great a part in that change had been played by coal.

The speaker concluded with an application of the same principles to the case of Ireland, showing that the calamities of her history were traceable to nature as much as to man.

[G. S.]

WEEKLY EVENING MEETING,

Friday, March 15, 1878.

The Duke of Northumberland, D.C.L. President, in the Chair.

The Lord Rayleigh, M.A. F.R.S. *M.R.I.*

The Explanation of certain Acoustical Phenomena.

Musical sounds have their origin in the vibrations of material systems. In many cases, e. g. the pianoforte, the vibrations are free, and are then necessarily of short duration. In other cases, e. g. organ pipes and instruments of the violin class, the vibrations are maintained, which can only happen when the vibrating body is in connection with a source of energy, capable of compensating the loss caused by friction and generation of aerial waves. The theory of free vibrations is tolerably complete, but the explanations hitherto given of maintained vibrations are generally inadequate, and in most cases altogether illusory.

In consequence of its connection with a source of energy, a vibrating body is subject to certain forces, whose nature and effects are to be estimated. These forces are divisible into two groups. The first group operate upon the periodic time of the vibration, i. e. upon the pitch of the resulting note, and their effect may be in either direction. The second group of forces do not alter the pitch, but either encourage or discourage the vibration. In the first case only can the vibration be maintained; so that for the explanation of any maintained vibration, it is necessary to examine the character of the second group of forces sufficiently to discover whether their effect is favourable or unfavourable. In illustration of these remarks, the simple case of a common pendulum was considered. The effect of a small periodic horizontal impulse is in general both to alter the periodic time and the amplitude of vibration. If the impulse (supposed to be always in the same direction) acts when the pendulum passes through its lowest position, the force belongs to the second group. It leaves the periodic time unaltered, and encourages or discourages the vibration according as the direction of the pendulum's motion is the same or the opposite of that of the impulse. If, on the other hand, the impulse acts when the pendulum is at one or other of the limits of its swing, the effect is solely on the periodic time, and the vibration is neither encouraged nor discouraged. In order to encourage, i. e. practically in order to maintain a vibration, it is necessary that the forces should not depend solely upon the position of the vibrating body. Thus, in the case of the pendulum, if a small impulse in a given direction acts upon it every time that it passes through its lowest position, the vibration is not maintained, the advantage gained as the pendulum makes a passage in the same direction as that in which the impulse acts being exactly neutralized on the return passage, when the motion is in the opposite direction.

As an example of the application of these principles the maintenance of an electric tuning fork was discussed. If the magnetic forces depended only upon the position of the fork, the vibration could not be maintained. It appears therefore that the explanations usually given do not touch the real point at all. The fact that the vibrations are maintained is a proof that the forces do not depend solely upon the position of the fork. The causes of deviation are two—the self-induction of the electric currents, and the adhesion of the mercury to the wire whose motion makes and breaks the contact. On both accounts the magnetic forces are more powerful in the latter than in the earlier part of the contact, although the position of the fork is the same; and it is on this *difference* that the possibility of maintenance depends. Of course the arrangement must be such that the retardation of force *encourages* the vibration, and the arrangement which in fact encourages the vibration would have had the opposite effect, if the nature of electric currents had been such that they were more powerful during the earlier than during the later stages of a contact.

In order to bring the subject within the limits of a lecture, one class of maintained vibrations was selected for discussion, that, namely, of which *heat* is the motive power. The best understood example of this kind of maintenance is that afforded by Trevelyan's bars, or rockers. A heated brass or copper bar, so shaped as to rock readily from one point of support to another, is laid upon a cold block of lead. The communication of heat through the point of support expands the lead lying immediately below in such a manner that the rocker receives a small impulse. During the interruption of the contact the communicated heat has time to disperse itself in some degree into the mass of lead, and it is not difficult to see that the impulse is of a kind to encourage the motion. But the most interesting vibrations of this class are those in which the vibrating body consists of a mass of air more or less completely confined.

If heat be periodically communicated to, and abstracted from, a mass of air vibrating (for example) in a cylinder bounded by a piston, the effect produced will depend upon the phase of the vibration at which the transfer of heat takes place. If heat be given to the air at the moment of greatest condensation, or taken from it at the moment of greatest rarefaction, the vibration is encouraged. On the other hand, if heat be given at the moment of greatest rarefaction, or abstracted at the moment of greatest condensation, the vibration is discouraged. The latter effect takes place of itself, when the rapidity of alternation is neither very great nor very small, in consequence of radiation; for when air is condensed it becomes hotter, and communicates heat to surrounding bodies. The two extreme cases are exceptional, though for different reasons. In the first, which corresponds to the suppositions of Laplace's theory of the propagation of sound, there is not sufficient time for a sensible transfer to be effected. In the second, the temperature remains nearly constant, and the loss of heat occurs during the *process* of condensation, and not when the condensation is effected. This case corresponds to Newton's theory of the velocity of sound. When the transfer of heat takes place at the moments of greatest condensation or of greatest rarefaction, the pitch is not affected.

If the air be at its normal density at the moment when the transfer of heat takes place, the vibration is neither encouraged nor discouraged, but the pitch is altered. Thus the pitch is *raised*, if heat be communicated to the air a quarter period *before* the phase of greatest condensation, and the pitch is *lowered* if the heat be communicated a quarter period *after* the phase of greatest condensation.

In general both kinds of effects are produced by a periodic transfer of heat. The pitch is altered, and the vibrations are either encouraged or discouraged. But there is no effect of the second kind if the air concerned be at a loop, i. e. a place where the density does not vary, nor if the communication of heat be the same at any stage of rarefaction, as in the corresponding stage of condensation.

The first example of aerial vibrations maintained by heat was found in a phenomenon which has often been observed by glass-blowers, and was made the subject of a systematic investigation by Dr. Sondhauss. When a bulb about three-quarters of an inch in diameter is blown at the end of a somewhat narrow tube, 5 or 6 inches in length, a sound is sometimes heard proceeding from the heated glass. It was proved by Sondhauss that a vibration of the glass itself is no essential part of the phenomenon, and the same observer was very successful in discovering the connection between the *pitch* of the note and the dimensions of the apparatus. But no explanation (worthy of the name) of the production of sound has been given.

For the sake of simplicity, a simple tube, hot at the closed end and getting gradually cooler towards the open end, was first considered. At a quarter of a period *before* the phase of greatest condensation (which occurs almost simultaneously at all parts of the column) the air is moving inwards, i. e. towards the closed end, and therefore is passing from colder to hotter parts of the tube; but the heat received at this moment (of normal density) has no effect either in encouraging or discouraging the vibration. The same would be true of the entire operation of the heat, if the adjustment of temperature were instantaneous, so that there was never any sensible difference between the temperatures of the air and of the neighbouring parts of the tube. But in fact the adjustment of temperature takes *time*, and thus the temperature of the air deviates from that of the neighbouring parts of the tube, inclining towards the temperature of that part of the tube *from* which the air has just come. From this it follows that at the phase of greatest condensation heat is received by the air, and at the phase of greatest rarefaction is given up from it, and thus there is a tendency to maintain the vibrations. It must not be forgotten, however, that apart from transfer of heat altogether, the condensed air is hotter than the rarefied air, and that in order that the whole effect of heat may be on the side of encouragement, it is necessary that previous to condensation the air should pass not merely towards a hotter part of the tube, but towards a part of the tube which is hotter than the air will be when it arrives there. On this account a great range of temperature is necessary for the maintenance of vibration, and even with a great range the influence of the transfer of heat is necessarily unfavourable at the closed end, where the motion is very small. This is probably the reason of the advantage of a bulb. It is obvious that if the *open* end of the tube were heated, the effect of the transfer of heat would be even more unfavourable than in the case of a temperature uniform throughout.

The sounds emitted by a jet of hydrogen, burning in an open tube, were noticed soon after the discovery of the gas, and have been the subject of several elaborate inquiries. The fact that the notes are substantially the same as those which may be elicited from the tube in other ways, e. g. by blowing, was announced by Chladni. Faraday proved that other gases were competent to take the place of hydrogen, though not without disadvantage. But it is to Sondhauss that we owe the most detailed examination of the circumstances under which the sound is produced. His experiments prove the importance of the part taken by the column of gas in the tube which supplies the jet. For example, sound cannot be obtained with a supply tube which is plugged with cotton in the neighbourhood of the jet, although no difference can be detected by the eye between the flame thus obtained and others which are competent to excite sound. When the supply tube is unobstructed, the sounds obtainable are limited as to pitch, often dividing themselves into detached groups. In the intervals between the groups no coaxing will induce a maintained sound, and it may be added that, for a part of the interval at any rate, the influence of the flame is inimical, so that a vibration started by a blow is damped more rapidly than if the jet were not ignited.

Partly in consequence of the peculiar behaviour of flames and partly for other reasons, the thorough explanation of these phenomena is a matter of some difficulty; but there can be no doubt that they fall under the head of vibration
communicated periodically to th
tube at a place where, in the cou
Although some authors have sh
the effects of the current of air

can readily be produced, not only when there is no through draught, but even when the flame is so situated that there is no sensible periodic motion of the air in its neighbourhood. In the course of the lecture a globe intended for burning phosphorus in oxygen gas was used as a resonator, and, when excited by a hydrogen flame well removed from the neck, gave a pure tone of about 95 vibrations per second.

In consequence of the variable pressure within the resonator, the issue of gas, and therefore the development of heat, varies during the vibration. The question is under what circumstances the variation is of the kind necessary for the maintenance of the vibration. If we were to suppose, as we might at first be inclined to do, that the issue of gas is greatest when the pressure in the resonator is least, and that the phase of greatest development of heat coincides with that of the greatest issue of gas, we should have the condition of things the most unfavourable of all to the persistence of the vibration. It is not difficult, however, to see that both suppositions are incorrect. In the supply tube (supposed to be unplugged, and of not too small bore) stationary, or approximately stationary, vibrations are excited, whose phase is either the same or the opposite of that of the vibration in the resonator. If the length of the supply tube from the burner to the open end in the gas-generating flask be less than a quarter of the wave length in hydrogen of the actual vibration, the greatest issue of gas *precedes* by a quarter period the phase of greatest condensation; so that, if the development of heat is *retarded* somewhat in comparison with the issue of gas, a state of things exists *favourable* to the maintenance of the sound. Some such retardation is inevitable, because a jet of inflammable gas can burn only at the outside; but in many cases a still more potent cause may be found, in the fact that during the retreat of the gas in the supply tube small quantities of air may enter from the interior of the resonator, whose expulsion must be effected before the inflammable gas can again begin to escape.

If the length of the supply tube amounts to exactly one quarter of the wave length, the stationary vibration within it will be of such a character that a node is formed at the burner, the variable part of the pressure just inside the burner being the same as in the interior of the resonator. Under these circumstances there is nothing to make the flow of gas, or the development of heat, variable, and therefore the vibration cannot be maintained. This particular case is free from some of the difficulties which attach themselves to the general problem, and the conclusion is in accordance with Sondhauss' observations.

When the supply tube is somewhat longer than a quarter of the wave, the motion of the gas is materially different from that first described. Instead of preceding, the greatest outward flow of gas *follows* at a quarter period interval the phase of greatest condensation, and therefore if the development of heat be somewhat retarded, the whole effect is unfavourable. This state of things continues to prevail, as the supply tube is lengthened, until the length of half a wave is reached, after which the motion again changes sign, so as to restore the possibility of maintenance. Although the size of the flame and its position in the tube (or neck of resonator) are not without influence, this sketch of the theory is sufficient to explain the fact, formulated by Dr. Sondhauss, that the principal element in the question is the length of the supply tube.

The next example of the production of sound by heat, shown in the lecture, was a very interesting phenomenon discovered by Rijke. When a piece of fine metallic gauze, stretching across the lower part of a tube, open at both ends and held vertically, is heated by a gas flame placed under it, a sound of considerable power, and lasting for several seconds, is observed almost immediately *after* the removal of the flame. Differing in this respect from the case of sonorous flames, the generation of sound was found by Rijke to be closely connected with the formation of a through draught, which impinges upon the heated gauze. In this form of the experiment the heat is soon abstracted, and then the sound ceases; but by keeping the gauze hot by the current from a powerful galvanic battery, Rijke was able to obtain the prolongation of the sound for an indefinite period. In any case from the point of view of the lecture the sound is to be regarded as a *maintained* sound.

In accordance with the general views already explained, we have to examine the character of the variable communication of heat from the gauze to the air. So far as the communication is affected directly by variations of pressure or density, the influence is unfavourable, inasmuch as the air will receive less heat from the gauze when its own temperature is raised by condensation. The maintenance depends upon the variable transfer of heat due to the varying *motions* of the air through the gauze, this motion being compounded of a uniform motion upwards with a motion, alternately upwards and downwards, due to the vibration. In the lower half of the tube these motions conspire a quarter period *before* the phase of greatest condensation, and oppose one another a quarter period after that phase. The rate of transfer of heat will depend mainly upon the temperature of the air in contact with the gauze, being greatest when that temperature is lowest. Perhaps the easiest way to trace the mode of action is to begin with the case of a simple vibration without a steady current. Under these circumstances the whole of the air which comes in contact with the metal, in the course of a complete period, becomes heated; and after this state of things is established, there is comparatively little further transfer of heat. The effect of superposing a small steady upwards current is now easily recognized. At the limit of the inwards motion, i. e. at the phase of greatest condensation, a small quantity of air comes into contact with the metal, which has not done so before, and is accordingly cool; and the heat communicated to this quantity of air acts in the most favourable manner for the maintenance of the vibration.

A quite different result ensues if the gauze be placed in the *upper* half of the tube. In this case the fresh air will come into the field at the moment of greatest rarefaction, when the communication of heat has an unfavourable instead of a favourable effect. The principal note of the tube therefore cannot be sounded.

A complementary phenomenon discovered by Bosscha and Riess may be explained upon the same principles. If a current of *hot* air impinge upon *cold* gauze, sound is produced; but in order to obtain the principal note of the tube the gauze must be in the upper, and not as before in the lower, half of the tube. An experiment due to Riess was shown in which the sound is maintained indefinitely. The upper part of a brass tube is kept cool by water contained in a tin vessel, through the bottom of which the tube passes. In this way the gauze remains comparatively cool, although exposed to the heat of a gas flame situated an inch or two below it. The experiment sometimes succeeds better when the draught is checked by a plate of wood placed somewhat closely over the top of the tube.

Both in Rijke's and Riess' experiments the variable transfer of heat depends upon the motion of vibration, while the effect of the transfer depends upon the variation of pressure. The gauze must therefore be placed where both effects are sensible, i. e. neither near a node nor near a loop. About a quarter of the length of the tube, from the lower or upper end, as the case may be, appears to be the most favourable position.

[Rayleigh.]

WEEKLY EVENING MEETING,

Friday, March 22, 1878.

Warren De la Rue, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Professor Tyndall, D.C.L. LL.D. F.R.S. *M.R.I.*

Recent Experiments on Fog-signals.

The care of its sailors is one of the first duties of a maritime people, and one of the sailor's greatest dangers is his proximity to the coast at night. Hence the idea of warning him of such proximity by beacon-fires placed sometimes on natural eminences and sometimes on towers built expressly for the purpose. Close to Dover Castle, for example, stands an ancient Pharos of this description.

As our marine increased greater skill was invoked, and lamps reinforced by parabolic reflectors poured their light upon the sea. Several of these lamps were sometimes grouped together so as to intensify the light, which at a little distance appeared as if it emanated from a single source. This "catoptric" form of apparatus is still to some extent employed in our lighthouse-service, but for a long time past it has been more and more displaced by the great lenses devised by the illustrious Frenchman, Fresnel.

In a first-class "dioptric" apparatus the light emanates from a lamp with several concentric wicks, the flame of which, being kindled by a very active draught, attains to great intensity. In fixed lights the lenses refract the rays issuing from the lamp so as to cause them to form a luminous sheet which grazes the sea-horizon. In revolving lights the lenses gather up the rays into distinct beams, resembling the spokes of a wheel, which sweep over the sea and strike the eye of the mariner in succession.

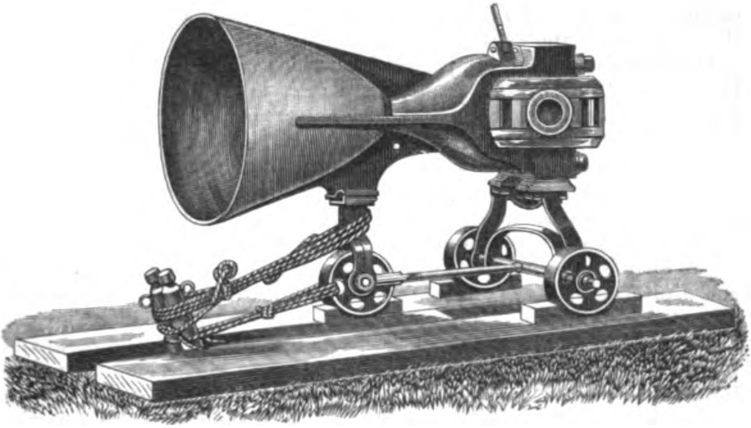
It is not for clear weather that the greatest strengthening of the light is intended, for here it is not needed. Nor is it for densely foggy weather, for here it is ineffectual. But it is for the intermediate stages of hazy, snowy, or rainy weather, in which a powerful light can assert itself, while a feeble one is extinguished. The usual first-order lamp is one of four wicks, but Mr. Douglass, the able and indefatigable engineer of the Trinity House, has recently raised the number of the wicks to six, which produce a very noble flame. To Mr. Wigham, of Dublin, we are indebted for the successful application of gas to lighthouse illumination. In some lighthouses his power varies from 28 jets to 108 jets, while in the lighthouse of Galley Head three burners of the largest size can be employed, the maximum number of jets being 324. These larger powers are invoked only in case of fog, the 28-jet burner being amply sufficient for clear weather. The passage from the small burner to the large, and from the large burner to the small, is made with ease, rapidity, and certainty. This employment of gas is indigenous to Ireland, and the Board of Trade has exercised a wise liberality in allowing every facility to Mr. Wigham for the development of his invention.

The last great agent employed in lighthouse illumination is electricity. It was in this Institution, beginning in 1831, that Faraday proved the existence and illustrated the laws of those induced currents which in our day have received such astounding development. In relation to this subject Faraday's words have a prophetic ring. "I have rather," he writes in 1831, "been desirous of discovering new facts and new relations dependent on magneto-electric induction than of exalting the force of those already obtained, being assured that the latter would find their full development hereafter." The labours of Holmes, of the Paris Alliance Company, of Wilde, and of Gramme, constitute a brilliant fulfilment of this prediction.

But, as regards the augmentation of power, the greatest step hitherto made was independently taken a few years ago by Dr. Werner Siemens and Sir Charles Wheatstone. Through the application of their discovery a machine endowed with an infinitesimal charge of magnetism may, by a process of accumulation at compound interest, be caused so to enrich itself magnetically as to cast by its performance all the older machines into the shade. The light now before you is that of a small machine placed downstairs, and worked there by a minute steam-engine. It is a light of about 1000 candles; and for it, and for the steam-engine that works it, our members are indebted to the liberality of Dr. William Siemens, who in the most generous manner has presented the machine to this Institution. After an exhaustive trial at the South Foreland, machines on the principle of Siemens, but of far greater power than this one, have been recently chosen by the Elder Brethren of the Trinity House for the two lighthouses at the Lizard Point.

Our most intense lights, including the six-wick lamp, the Wigham gas-light, and the electric light, being intended to aid the mariner in heavy weather, may be regarded, in a certain sense, as fog-signals. But fog, when thick, is intractable to light. The sun cannot penetrate it, much less any terrestrial source of illumination. Hence the necessity of employing sound-signals in dense fogs. Bells, gongs, horns, whistles, guns, and syrens have been used for this purpose; but it is mainly, if not wholly, with explosive signals that we have now to deal. The gun has been employed with useful effect at the North Stack, near Holyhead, on the Kish Bank near Dublin, at Lundy Island, and at other points on our coasts. During the long, laborious, and I venture to think memorable series of observations conducted under the auspices of the Elder Brethren of the Trinity House at the South Foreland in

1872 and 1873, it was proved that a short 5½-inch howitzer, firing 3 lbs. of powder, yielded a louder report than a long 18-pounder firing the same charge. Here was a hint to be acted on by the Elder Brethren. The effectiveness of the sound depended on the shape of the gun, and as it could not be assumed that in the howitzer we had hit accidentally upon the best possible shape, arrangements were made with the War Office for the construction of a gun specially calculated to produce the loudest sound attainable from the combustion of 3 lbs. of powder. To prevent the unnecessary landward waste of the sound, the gun was furnished with a parabolic muzzle, intended to project the sound over the sea, where it was most needed. The construction of this gun was based on a searching series of experiments executed at Woolwich with small models, provided with muzzles of various kinds. A drawing of the gun is before you. It was constructed on the principle of the revolver, its various chambers being loaded and brought in rapid succession into the firing position. The performance of the gun proved the correctness of the principles on which its construction was based.



Breech-loading Fog-signal Gun, with Bell Mouth,* proposed by Major Maitland, R.A. Assistant Superintendent.

* The carriage of this gun has been modified in construction since this drawing was made.

An incidental point of some interest was decided by the earliest Woolwich experiments. It had been a widely spread opinion among artillerists, that a bronze gun produces a specially loud report. I doubted from the outset whether this would help us; and in a letter dated 22nd April, 1874, I ventured to express myself thus:—"The report of a gun, as affecting an observer close at hand, is made up of two factors—the sound due to the shock of the air by the violently expanding gas, and the sound derived from the vibrations of the gun, which, to some extent, rings like a bell. This latter, I apprehend, will disappear at considerable distances." The result of subsequent trial, as reported by General Campbell, is, "that the sonorous qualities of bronze are greatly superior to those of cast iron at short distances, but that the advantage lies with the baser metal at long ranges." *

Coincident with these trials of guns at Woolwich, gun-cotton was thought of as a probably effective sound-producer. From the first, indeed, theoretic considerations caused me to fix my attention persistently on this substance; for the remarkable experiments of Mr. Abel, whereby its rapidity of combustion and violently explosive energy are demonstrated, seemed to single it out as a substance eminently calculated to fulfil the conditions necessary to the production of an intense wave of sound. What those conditions are we shall now more particularly inquire, calling to our aid a brief but very remarkable paper, published by Professor Stokes in the 'Philosophical Magazine' for 1868.

The explosive force of gunpowder is known to depend on the sudden conversion of a solid body into an intensely heated gas. Now the work which the artillerist requires the expanding gas to perform is the displacement of the projectile, besides which it has to displace the air in front of the projectile, which is backed by the whole pressure of the atmosphere. Such, however, is not the work that we want our gunpowder to perform. We wish to transmute its energy not into the mere mechanical translation of either shot or air, but into vibratory motion. We want *pulses* to be formed which shall propagate themselves to vast distances through the atmosphere, and this requires a certain choice and management of the explosive material.

A sound-wave consists essentially of two parts—a condensation and a rarefaction. Now air is a very mobile fluid, and if the shock imparted to it lack due promptness, the wave is not produced. Consider the case of a common clock pendulum, which oscillates to and fro, and which might be expected to generate corresponding pulses in the air. When, for example, the bob moves to the right, the air to the right of it might be supposed to be condensed, while a partial vacuum might be supposed to follow the bob. As a matter of fact, we have nothing of this kind. The air particles in front of the bob retreat so rapidly, and those behind it close so rapidly in, that no sound-pulse is formed. The mobility of hydrogen, moreover, being far greater than that of air, a prompter action is essential to the formation of sonorous waves in hydrogen than in air. It is to this rapid power of readjustment, this refusal, so to speak, to allow its atoms to be crowded together or to be drawn apart, that Professor Stokes, with admirable penetration, refers the damping power, first described by Sir John Leslie, of hydrogen upon sound.

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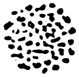
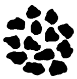
* General Campbell assigns a true cause for this difference. The ring of the bronze gun represents so much energy withdrawn from the explosive force of the gunpowder. Further experiments would, however, be needed to place the superiority of the cast-iron gun at a distance beyond question.

A tuning-fork which executes 256 complete vibrations in a second, if struck gently on a pad and held in free air, emits a scarcely audible note. It behaves to some extent like the pendulum bob just referred to. This feebleness is due to the prompt "reciprocating flow" of the air between the incipient condensations and rarefactions, whereby the formation of sound-pulses is forestalled. Stokes, however, has taught us that this flow may be intercepted by placing the edge of a card in close proximity to one of the corners of the fork. An immediate augmentation of the sound of the fork is the consequence.

The more rapid the shock imparted to the air, the greater is the fractional part of the energy of the shock converted into wave motion. And as different kinds of gunpowder vary considerably in their rapidity of combustion, it may be expected that they will also vary as producers of sound. This theoretic inference is completely verified by experiment. In a series of preliminary trials conducted at Woolwich on the 4th of June, 1875, the sound-producing powers of four different kinds of powder were determined. In the order of the size of their grains they bear the names respectively of Fine-grain (F. G.), Large-grain (L. G.), Rifle Large-grain (R. L. G.), and Pebble-powder (P.). (See annexed figures.) The charge in each case amounted to 4½ lbs.; four 24-lb. howitzers being employed to fire the respective charges. There were eleven observers, all of whom, without a single dissentient, pronounced the sound of the fine-grain powder loudest of all. In the opinion of seven of the eleven the large-grain powder came next; seven also of the eleven placed the rifle large-grain third on the list; while they were again unanimous in pronouncing the pebble-powder the worst sound-producer. These differences are entirely due to differences in the rapidity of combustion. All who have witnessed the performance of the 80-ton gun must have been surprised at the mildness of its thunder. To avoid the strain resulting from quick combustion, the powder employed is composed of lumps far larger than those of the pebble-powder above referred to. In the long tube of the gun these lumps of solid matter gradually resolve themselves


F. G.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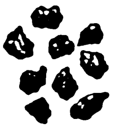
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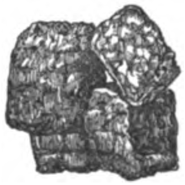
P.

into gas, which on issuing from the muzzle imparts a kind of push to the air, instead of the sharp shock necessary to form the condensation of an intensely sonorous wave.

These are some of the physical reasons why gun-cotton might be regarded as a promising fog-signal. Firing it as we have been taught to do by Mr. Abel, its explosion is more rapid than that of gunpowder. In its case the air particles, alert as they are, will not, it might be presumed, be able to slip from condensation to rarefaction with a rapidity sufficient to forestall the formation of the wave. On *à priori* grounds then, we are entitled to infer the effectiveness of gun-cotton, while in a great number of comparative experiments, stretching from 1874 to the present time, this inference has been verified in the most conclusive manner.

As regards explosive material, and zealous and accomplished help in the use of it, the resources of Woolwich Arsenal have been freely placed at the disposal of the Elder Brethren. General Campbell, General Younghusband, Colonel Fraser, Colonel Maitland, and other officers, have taken an active personal part in the investigation, and in most cases have incurred the labour of reducing and reporting on the observations. Guns of various forms and sizes have been invoked for gunpowder, while gun-cotton has been fired in free air and in the foci of parabolic reflectors.

On the 22nd of February, 1875, a number of small guns, cast specially for the purpose—some with plain, some with conical, and some with parabolic muzzles—firing 4 oz. of fine-grain powder, were pitted against 4 oz. of gun-cotton detonated both in the open, and in the focus of a parabolic reflector.* The sound produced by the gun-cotton, reinforced by the reflector, was unanimously pronounced loudest of all. With equal unanimity, the gun-cotton detonated in free air was placed second in intensity. Though the same charge was used throughout, the guns differed notably among themselves, but none of them came up to the gun-cotton, either with or without the reflector. A second series, observed from a different distance on the same day, confirmed to the letter the foregoing result.

As a practical point, however, the comparative cost of gun-cotton and gunpowder has to be taken into account, though considerations of cost ought not to be stretched too far in cases involving the safety of human life. In the earlier experiments, where quantities of equal price were pitted against each other, the results were somewhat fluctuating. Indeed, the perfect manipulation of the gun-cotton required some preliminary discipline—promptness, certainty, and effectiveness of firing, augmenting as experience increased. As 1 lb. of gun-cotton costs as much as 3 lbs. of gunpowder, these quantities were compared together on the 22nd of February. The guns employed to discharge the gunpowder were a 12-lb. brass howitzer, a 24-lb. cast-iron howitzer, and the long 18-pounder employed at the South Foreland. The result was, that the 24-lb. howitzer, firing 3 lbs. of gunpowder, had a slight advantage over 1 lb. of gun-cotton detonated in the open; while the 12-lb. howitzer and the 18-pounder were both beaten by the gun-cotton. On the 2nd of May, on the other hand, the gun-cotton is reported as having been beaten by all the guns.

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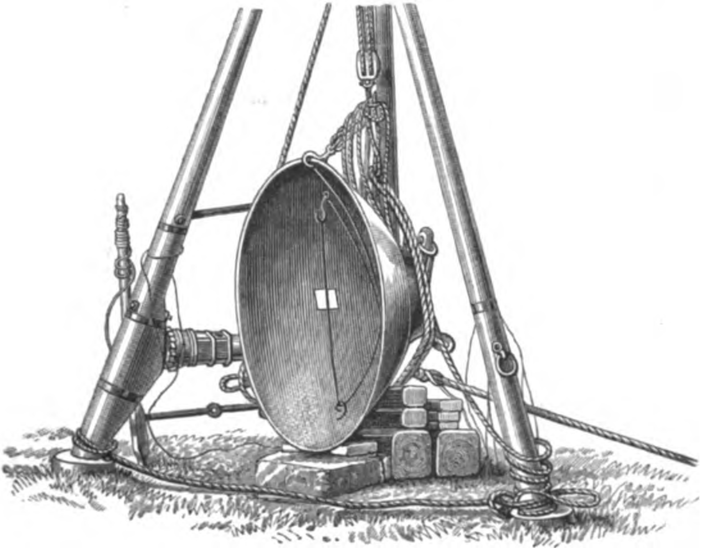
* For charges of this weight the reflector is of moderate size, and may be employed without fear of fracture.

Meanwhile, the parabolic-muzzle gun, expressly intended for fog-signalling, was pushed rapidly forward, and on the 22nd and 23rd of March, 1876, its power was tested at Shoeburyness. Pitted against it were a 16-pounder, a 5½-inch howitzer, 1½ lb. of gun-cotton detonated in the focus of a reflector (see annexed figure), and 1½ lb. of gun-cotton detonated in free air. On this occasion nineteen different series of experiments were made, when the new experimental gun, firing a 3-lb. charge, demonstrated its superiority over all guns previously employed to fire the same charge. As regards the comparative merits of the gun-cotton fired in the open, and the gunpowder fired from the new gun, the mean values of their sounds were the same. Fired in the focus of the reflector, the gun-cotton clearly dominated over all the other sound-producers.*



Gun-cotton Slab (1½ lb.) Detonated in the Focus of a Cast-iron Reflector.

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* The reflector was fractured by the explosion, but it did good service afterwards.

The whole of the observations here referred to were embraced by an angle of about 70°, of which 50° lay on the one side and 20° on the other side of the line of fire. The shots were heard by eleven observers on board the 'Galatea,' which took up positions varying from 2 miles to 13½ miles from the firing-point. In all these observations, the reinforcing action of the reflector, and of the parabolic muzzle of the gun, came into play. But the reinforcement of the sound in one direction implies its withdrawal from some other direction, and accordingly it was found that at a distance of 5¼ miles from the firing-point, and on a line, including nearly an angle of 90°, with the line of fire, the gun-cotton in the open beat the new gun; while behind the station, at distances of 8½ miles and 13½ miles respectively, the gun-cotton in the open beat both the gun and the gun-cotton in the reflector. This result is rendered more important by the fact that the sound reached the Mucking Light, a distance of 13½ miles, against a light wind which was blowing at the time.

Most, if not all, of our ordinary sound-producers send forth waves which are not of uniform intensity throughout. A trumpet is loudest in the direction of its axis. The same is true of a gun. A bell, with its mouth pointed upwards or downwards, sends forth waves far denser in the horizontal plane passing through the bell than at an angular distance of 90° from that plane. The oldest bellhangers must have been aware of the fact that the sides of the bell, and not its mouth, emitted the strongest sound, their practice being probably determined by this knowledge. Our slabs of gun-cotton also emit waves of different densities in different parts. It has occurred in the experiments at Shoeburyness that when the broad side of a slab was turned towards the suspending wire of a second slab six feet distant, the wire was cut by the explosion, while when the edge of the slab was turned to the wire this never occurred. To the circumstance that the broadsides of the slabs faced the sea is probably to be ascribed the remarkable fact observed on the 23rd of March, that in two directions, not far removed from the line of fire, the gun-cotton detonated in the open had a slight advantage over the new gun.

Theoretic considerations rendered it probable that the shape and size of the exploding mass would affect the constitution of the wave of sound. I did not think large rectangular slabs the most favourable shape, and accordingly proposed cutting a large slab into fragments of different sizes, and pitting them against each other. The differences between the sounds were by no means so great as the differences in the quantities of explosive material might lead one to expect. The mean values of eighteen series of observations made on board the 'Galatea,' at distances varying from 1¾ mile to 4·8 miles, were as follows:—

Weights	4-oz.	6-oz.	9-oz.	12-oz.
Value of sound	3·12	3·34	4·0	4·03

These charges were cut from a slab of dry gun-cotton about

1¼ inch thick: they were squares and rectangles of the following dimensions:—4 oz., 2 inches by 2 inches; 6 oz., 2 inches by 3 inches; 9 oz., 3 inches by 3 inches; 12 oz., 2 inches by 6 inches.

The numbers under the respective weights express the recorded value of the sounds. They must be simply taken as a ready means of expressing the approximate relative intensity of the sounds as estimated by the ear. When we find a 9-oz. charge marked 4, and a 12-oz. charge marked 4·03, the two sounds may be regarded as practically equal in intensity, thus proving that an addition of 30 per cent. in the larger charges produces no sensible difference in the sound. Were the sounds estimated by some physical means, instead of by the ear, the values of the sounds would not, in my opinion, show a greater advance with the increase of material than that indicated by the foregoing numbers. Subsequent experiments rendered still more certain the effectiveness, as well as the economy, of the smaller charges of gun-cotton.

It is an obvious corollary from the foregoing experiments that on our "nesses" and promontories, where the land is clasped on both sides for a considerable distance by the sea—where, therefore, the sound has to propagate itself rearward as well as forward—the use of the parabolic gun, or of the parabolic reflector might be a disadvantage rather than an advantage. Here gun-cotton, exploded in the open, forms the most appropriate source of sound. This remark is especially applicable to such lightships as are intended to spread the sound all round them as from central foci. As a signal in rock lighthouses, where neither syren, steam-whistle, nor gun could be mounted; and as a handy fleet-signal, dispensing with the lumber of special signal-guns, the gun-cotton will prove invaluable. But in most of these cases we have the drawback that local damage may be done by the explosion. The lantern of the rock lighthouse might suffer from concussion near at hand, and though mechanical arrangements might be devised, both in the case of the lighthouse and of the ship's deck, to place the firing-point of the gun-cotton at a safe distance, no such arrangement could compete, as regards simplicity and effectiveness, with the expedient of *a gun-cotton rocket*. Had such a means of signalling existed at the Bishop's Rock lighthouse, the ill-fated 'Schiller' might have been warned of her approach to danger ten, or it may be twenty, miles before she reached the rock which wrecked her. Had the fleet possessed such a signal, instead of the ubiquitous but ineffectual whistle, the 'Iron Duke' and 'Vanguard' need never have come into collision.

It was the necessity of providing a suitable signal for rock lighthouses, and of clearing obstacles which cast an acoustic shadow, that suggested the idea of the gun-cotton rocket to Sir Richard Collinson, Deputy Master of the Trinity House. His idea was to place a disk or short cylinder of gun-cotton in the head of a rocket, the ascensional force of which should be employed to carry the disk to an elevation of 1000 feet or thereabouts, where by the ignition of a fuse associated with a detonator, the gun-cotton should be fired, sending its sound in all directions vertically and obliquely down upon earth and sea. The first attempt to realize this idea was made on the 18th of July, 1876, at the fire-work manufactory of the Messrs. Brock, at Nunhead. Eight rockets were then fired, four being charged with 5 oz. and four with 7½ oz. of gun-cotton. They ascended to a great height, and exploded with a very loud report in the air. On the 27th of July, the rockets were tried at Shoeburyness. The most noteworthy result on this occasion was the hearing of the sounds at the Mouse Lighthouse, 8½ miles E. by S., and at the Chapman Lighthouse, 8½ miles W. by N.; that is to say, at opposite sides of the firing-point. It is worthy of remark that, in the case of the Chapman Lighthouse, land and trees intervened between the firing-point and the place of observation. "This," as General Younghusband justly remarked at the time, "may prove to be a valuable consideration if it should be found necessary to place a signal station in a position whence the sea could not be freely observed." Indeed, the clearing of such obstacles was one of the objects which the inventor of the rocket had in view.

With reference to the action of the wind, it was thought desirable to compare the range of explosions produced near the surface of the earth with others produced at the elevation attainable by the gun-cotton rockets. Wind and weather, however, are not at our command; and hence one of the objects of a series of experiments conducted on the 13th of December, 1876, was not fulfilled. It is worthy, however, of note that on this day, with smooth water and a calm atmosphere, the rockets were distinctly heard at a distance of 11·2 miles from the firing-point. The quantity of gun-cotton employed was 7½ oz. On Thursday the 8th of March, 1877, these comparative experiments of firing at high and low elevations were pushed still further. The gun-cotton near the ground consisted of ½-lb. disks, suspended from a horizontal iron bar about 4½ feet above the ground. The rockets carried the same quantity of gun-cotton in their heads, and the height to which they attained, as determined by a theodolite, was from 800 to 900 feet. The day was cold, with occasional squalls of snow and hail, the direction of the sound being at right angles to that of the wind. Five series of observations were made on board the 'Vestal,' at distances varying from 3 to 6 miles. The mean value of the explosions in the air exceeded that of the explosions near the ground by a small but sensible quantity. At Windmill Hill, Gravesend, however, which was nearly to leeward, and 5½ miles from the firing-point, in nineteen cases out of twenty-four the disk fired near the ground was loudest; while in the remaining five the rocket had the advantage.

Towards the close of the day the atmosphere became very serene. A few distant cumuli sailed near the horizon, but the zenith and a vast angular space all round it were absolutely free from cloud. From the deck of the 'Galatea' a rocket was discharged, which reached a great elevation, and exploded with a loud report. Following this solid nucleus of sound was a continuous train of echoes, which retreated to a continually greater distance, dying gradually off into silence after seven seconds' duration. These echoes were of the same character as those so frequently noticed at the South Foreland in 1872-73, and called by me "aerial echoes."

On the 23rd of March the experiments were resumed, the most noteworthy results of that day's observations being that the sounds were heard at Tillingham, 10 miles to the N.E.; at West Mersea, 15¾ miles to the N.E. by E.; at Brightlingsea, 17½ miles to the N.E.; and at Clacton Wash, 20½ miles to the N.E. by ½ E. The wind was blowing at the time from the S.E. Some of these sounds were produced by rockets, some by a 24-lb. howitzer, and some by an 8-inch Maroon.

In December, 1876, Mr. Gardiner, the managing director of the Cotton-powder Company, had proposed a trial of this material against the gun-cotton. The density of the cotton he urged was only 1·03, while that of the powder was 1·70. A greater quantity of explosive material being thus compressed into the same volume, Mr. Gardiner thought that a greater sonorous effect must be produced by the powder. At the instance of Mr. Mackie, who had previously gone very thoroughly into the subject, a Committee of the Elder Brethren visited the cotton-powder manufactory, on the banks of the Swale, near Faversham, on the 16th of June, 1877. The weights of cotton-powder employed were 2 oz., 8 oz., 1 lb., and 2 lbs., in the form of rockets and of signals fired a few feet above the ground. The experiments throughout were arranged and conducted by Mr. Mackie. Our desire on this occasion was to get as near to windward as possible, but the Swale and other obstacles limited our distance to 1½ mile. We stood here E.S.E. from the firing-point while the wind blew fresh from the N.E.

The cotton-powder yielded a very effective report. The rockets in general had a slight advantage over the same quantities of material fired near the ground. The loudness of the sound was by no means proportional to the quantity of the material exploded, 8 oz. yielding very nearly as loud a report as 1 lb. The "aerial echoes," which invariably followed the explosion of the rockets, were loud and long-continued.

On the 17th of October, 1877, another series of experiments with howitzers and rockets was carried out at Shoeburyness. The charge of the howitzer was 3 lbs. of L. G. powder. The charges of the rockets were 12 oz., 8 oz., 4 oz., and 2 oz. of gun-cotton respectively. The gun and the four rockets constituted a series, and eight series were fired during the afternoon of the 17th. The observations were made from the 'Vestal' and the 'Galatea,' positions being successively assumed which permitted the sound to reach the observers with the wind, against the wind, and across the wind. The distance of the 'Galatea' varied from 3 to 7 miles, that of the 'Vestal,' which was more restricted in her movements, being 2 to 3 miles. Briefly summed up, the result is that the howitzer, firing a 3-lb. charge, which it will be remembered was our best gun at the South Foreland, was beaten by the 12-oz. rocket, by the 8-oz. rocket, and by the 4-oz. rocket. The 2-oz. rocket alone fell behind the howitzer.

It is worth while recording the distances to which some of the sounds were heard on the day now referred to:—

1. Leigh	6½ miles	W.N.W...	24 out of 40 sounds heard.
2. Girdler Light-vessel	12 „	S.E. by E.	5 „
3. Reculvers	17½ „	S.E. by S.	18 „
4. St. Nicholas	20 „	S.E. ..	3 „
5. Epple Bay	22 „	S.E. by E.	19 „
6. Westgate	23 „	S.E. by E.	9 „
7. Kingsgate	25 „	S.E. by E.	8 „

The day was cloudy, with occasional showers of drizzling rain; the wind about N.W. by N. all day; at times squally, rising to a force of 6 or 7 and sometimes dropping to a force of 2 or 3. The station at Leigh excepted, all these places were to leeward of Shoeburyness. At four other stations to leeward, varying in distance from 15½ to 24½ miles, nothing was heard, while at eleven stations to windward, varying from 8 to 26 miles, the sounds were also inaudible. It was found, indeed, that the sounds proceeding directly against the wind did not penetrate much beyond 3 miles.

On the following day, viz. the 18th October, we proceeded to Dungeness with the view of making a series of strict comparative experiments with gun-cotton and cotton-powder. Rockets containing 8 oz., 4 oz., and 2 oz. of gun-cotton had been prepared at the Royal Arsenal; while others, containing similar quantities of cotton-powder, had been supplied by the cotton-powder company at Faversham. With these were compared the ordinary 18-pounder gun, which happened to be mounted at Dungeness, firing the usual charge of 3 lbs. of powder, and a syren.

From these experiments it appeared that the gun-cotton and cotton-powder were practically equal as producers of sound.

The effectiveness of small charges was illustrated in a very striking manner, only a single unit separating the numerical value of the 8-oz. rocket from that of the 2-oz. rocket. The former was recorded as 6·9 and the latter as 5·9, the value of the 4-oz. rocket being intermediate between them. These results were recorded by a number of very practised observers on board the 'Galatea.' They were completely borne out by the observations of the Coastguard, who marked the value of the 8-oz. rocket 6·1, and that of the 2-oz. rocket 5·2. The 18-pounder gun fell far behind all the rockets, a result, probably, to be in part ascribed to the imperfection of the powder. The performance of the syren was, on the whole, less satisfactory than that of the rocket. The instrument was worked, not by steam of 70 lbs. pressure, as at the South Foreland, but by compressed air, beginning with 40 lbs. and ending with 30 lbs. pressure. The trumpet was pointed to windward, and in the axis of the instrument the sound was about as effective as that of the 8-oz. rocket. But in a direction at right angles to the axis, and still more in the rear of this direction, the syren fell very sensibly behind even the 2-oz. rocket.

These are the principal comparative trials made between the gun-cotton rocket and other fog-signals; but they are not the only ones. On the 2nd of August, 1877, for example, experiments were made at Lundy Island with the following results. At 2 miles distant from the firing-point, with land intervening, the 18-pounder, firing a 3-lb. charge, was quite unheard. Both the 4-oz. rocket and the 8-oz. rocket, however, reached an elevation which commanded the acoustic shadow, and yielded loud reports. When both were in view, and heard, the rockets were still superior to the gun. On the 6th of August, at St. Ann's, the 4-oz. and 8-oz. rockets proved superior to the syren. On the Shambles Light-vessel, when a pressure of 13 lbs. was employed to sound the syren, the rockets proved greatly superior to that instrument. Proceeding along the sea margin at Flamboro Head, Mr. Edwards states that at a distance of 1¼ mile, with the 18-pounder previously used as a fog-signal hidden behind the cliffs, its report was quite unheard, while the 4-oz. rocket, rising to an elevation which brought it clearly into view, yielded a powerful sound in the face of an opposing wind.

On the evening of February 9th, 1877, a remarkable series of experiments were made by Mr. Prentice at Stowmarket with the gun-cotton rocket. From the report with which he has kindly furnished me I extract the following particulars. The first column in the annexed statement contains the name of the place of observation, the second its distance from the firing-point, and the third the result observed:—

Stoke Hill, Ipswich ..	10 miles	Rockets clearly seen and sounds distinctly heard 53 seconds after the flash.
Melton	15 „	Signals distinctly heard. Thought at first that sounds were reverberated from the sea.
Framlingham	18 „	Signals very distinctly heard, both in the open air and in a closed room. Wind in favour of sound.
Stratford. St. Andrews	19 „	Reports loud; startled pheasants in a cover close by.
Tuddenham. St. Martin	10 „	Reports very loud; rolled away like thunder.
Christ Church Park ..	11 „	Report arrived a little more than a minute after flash.
Nettlestead Hall	6 „	Distinct in every part of observer's house. Very loud in the open air.
Bildestone	6 „	Explosion very loud, wind against sound.
Nacton	14 „	Reports quite distinct—mistaken by inhabitants for claps of thunder.
Aldboro	25 „	Rockets seen through a very hazy atmosphere; a rumbling detonation heard.
Capel Mills	11 „	Reports heard within and without the observer's house. Wind opposed to sound.
Lawford	15½ „	Reports distinct: attributed to distant thunder.

In the great majority of these cases, the direction of the sound enclosed a large angle with the direction of the wind. In some cases, indeed, the two directions were at right angles to each other. It is needless to dwell for a moment on the advantage of possessing a signal commanding ranges such as these.

The explosion of substances in the air, after having been carried to a considerable elevation by rockets, is a familiar performance. In 1873, moreover, the Board of Trade proposed a light-and-sound rocket as a signal of distress, which proposal was subsequently realized, but in a form too elaborate and expensive for practical use. The idea of a gun-cotton rocket fit for signalling in fogs is, I believe, wholly due to the Deputy Master of the Trinity House. Thanks to the skilful aid given by the authorities of Woolwich, by Mr. Prentice, and Mr. Brock, that idea is now an accomplished fact; a signal of great power, handiness, and economy, being thus placed at the service of our mariners. Not only may the rocket be applied in association with lighthouses and lightships, but in the Navy also it may be turned to important account. Soon after the loss of the 'Vanguard' I ventured to urge upon an eminent naval officer the desirability of having an organized code of fog-signals for the fleet. He shook his head doubtingly, and referred to the difficulty of finding room for signal guns. The gun-cotton rocket completely surmounts this difficulty. It is manipulated with ease and rapidity, while its discharges may be so grouped and combined as to give a most important extension to the voice of the admiral in command. It is needless to add that at any point upon our coasts, or upon any other coast, where its establishment might be desirable, a fog-signal station might be extemporised without difficulty.

I have referred more than once to the train of echoes which accompanied the explosion of gun-cotton in free air, speaking of them as similar in all respects to those which were described for the first time in my Report on Fog-signals, addressed to the Corporation of Trinity House in 1874.* To these echoes I attached a fundamental significance. There was no visible reflecting surface from which they could come. On some days, with hardly a cloud in the air and hardly a ripple on the sea, they reached a magical intensity. As far as the sense of hearing could judge, they came from the body of the air in front of the great trumpet which produced them. The trumpet blasts were five seconds in duration, but long before the blast had ceased the echoes struck in, adding their strength to the primitive note of the trumpet. After the blast had ended the echoes continued, retreating further and further from the point of observation, and finally dying away at great distances. The echoes were perfectly continuous as long as the sea was clear of ships, "tapering" by imperceptible gradations into absolute silence. But when a ship happened to throw itself athwart the course of the sound, the echo from the broadside of the vessel was returned as a shock which rudely interrupted the continuity of the dying atmospheric music.

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* See also 'Philosophical Transactions' for 1874, p. 183.

These echoes have been ascribed to reflection from the crests of the sea-waves. But this hypothesis is negatived by the fact, that the echoes were produced in great intensity and duration when no waves existed —when the sea, in fact, was of glassy smoothness. It has been also shown that the direction of the echoes depended not on that of waves, real or assumed, but on the direction of the axis of the trumpet. Causing that axis to traverse an arc of 210°, and the trumpet to sound at various points of the arc, the echoes were always, at all events in calm weather, returned from that portion of the atmosphere towards which the trumpet was directed. They could not, under the circumstances, come from the glassy sea; while both their variation of direction and their perfectly continuous fall into silence, are irreconcilable with the notion that they came from fixed objects on the land. They came from that portion of the atmosphere into which the trumpet poured its maximum sound, and fell in intensity as the direct sound penetrated to greater atmospheric distances.

The day on which our latest observations were made was particularly fine. Before reaching Dungeness, the smoothness of the sea and the serenity of the air caused me to test the echoing power of the atmosphere. A single ship lay about half a mile distant between us and the land. The result of the proposed experiment was clearly foreseen. It was this. The rocket being sent up, it exploded at a great height; the echoes retreated in their usual fashion, becoming less and less intense as the distance of the surfaces of reflection from the observers increased. About five seconds after the explosion, a single loud shock was sent back to us from the side of the vessel lying between us and the land. Obliterated for a moment by this more intense echo, the aerial reverberation continued its retreat, dying away into silence in two or three seconds afterwards.

I have referred to the firing of an 8-oz. rocket from the deck of the 'Galatea' on the 8th of March, 1877, stating the duration of its echoes to be 7 seconds. Mr. Prentice, who was present at the time, assured me that in his experiments similar echoes had been frequently heard of more than twice this duration. The ranges of his sounds alone would render this result in the highest degree probable.

To attempt to interpret an experiment which I have not had an opportunity of repeating, is an operation of some risk; and it is not without a consciousness of this that I refer here to a result announced by Professor Joseph Henry, which he considers adverse to the notion of aerial echoes. He took the trouble to point the trumpet of a syren towards the zenith, and found that when the syren was sounded no echo was returned. Now the reflecting surfaces which give rise to these echoes are for the most part due to differences of temperature between sea and air. If, through any cause, the air above be chilled, we have descending streams—if the air below be warmed, we have ascending streams as the initial cause of atmospheric flocculence. A sound proceeding vertically does not cross the streams, nor impinge upon the reflecting surfaces, as does a sound proceeding horizontally across them. Aerial echoes, therefore, will not accompany the vertical sound as they accompany the horizontal one. The experiment, as I interpret it, is not opposed to the theory of these echoes which I have ventured to enunciate. But, as I have indicated, not only to see but to vary such an experiment is a necessary prelude to grasping its full significance.

In a paper published in the 'Philosophical Transactions' for 1876, Professor Osborne Reynolds refers to these echoes in the following terms:—"Without attempting to explain the reverberations and echoes which have been observed, I will merely call attention to the fact that in no case have I heard any attending the reports of the rockets,* although they seem to have been invariable with the guns and pistols. These facts suggest that the echoes are in some way connected with the direction given to the sound. They are caused by the voice, trumpets, and the syren, all of which give direction to the sound; but I am not aware that they have ever been observed in the case of a sound which has no direction of greatest intensity." The reference to the voice, and other references, cause me to think that in speaking of echoes, Professor Osborne Reynolds and myself are dealing with different phenomena. Be that as it may, the foregoing observations render it perfectly certain that the condition as to direction here laid down is not necessary to the production of the echoes.

There is not a feature connected with the aerial echoes which cannot be brought out by experiments in the open air of the laboratory. I have recently made the following experiment:—A rectangle, 22 inches by 12, is crossed by twenty-three brass tubes, each having a slit along it from which gas can issue. In this way twenty-three low flat flames are obtained. A sounding reed fixed in a short tube is placed at one end of the rectangle, and a "sensitive flame" at some distance beyond the other end. When the reed sounds, the flame in front of it is violently agitated, and roars boisterously. Turning on the gas, and lighting it as it issues from the slits, the air above the flames becomes so heterogeneous that the sensitive flame is instantly stilled by the aerial reflection, rising from a height of 6 inches to a height of 18 inches. Here we have the acoustic opacity of the air in front of the South Foreland strikingly imitated. Turning off the gas, and removing the sensitive flame to some distance behind the reed, it burns there tranquilly, though the reed may be sounding. Again lighting the gas as it issues from the brass tubes, the sound reflected from the heterogeneous air throws the sensitive flame into violent agitation. Here we have imitated the aerial echoes heard when standing behind the syren-trumpet at the South Foreland. The experiment is extremely simple, and in the highest degree impressive.

[J. T.]

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* These carried 12 oz. of gunpowder, which has been found by Col. Fraser to require an iron case to produce an effective explosion.

WEEKLY EVENING MEETING,

Friday, March 29, 1878.

Warren De la Rue, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Professor Dewar, M.A. F.R.S.

The Chemical Actions of Light and their Electrical Relations.

[Abstract deferred.]

GENERAL MONTHLY MEETING,

Monday, April 1, 1878.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Francis Maule Campbell, Esq.
Alfred Wing Everest, Esq.
Lancelot Fielding Everest, Esq.
Charles Cubitt Gooch, Esq.
Charles Hawksley, Esq.
Tom Simpson Jay, Esq.
Hugh Parnell, Esq. M.A.
George W. Smalley, Esq.

were *elected* Members of the Royal Institution.

The Special Thanks of the Members were given to **Lord Arthur Russell**, M.P. *M.R.I.* for his present of Eighty Volumes of Ersch and Gruber's "Encyclopädie der Wissenschaften," and to Dr. **William Siemens**, D.C.L. F.R.S. *M.R.I.* for his present of a Dynamo-Magneto-Electric Machine, and a small Steam-engine.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

American Academy of Arts and Sciences, Boston—Proceedings, Vol. XIII. Part 1. 8vo. 1877.

Astronomical Society, Royal—Monthly Notices, Vol. XXXVIII. No. 4. 8vo. 1878.

Basel Naturforschender Gesellschaft—Verhandlungen, 6te Theil, 3tes Heft. 8vo. 1878.

British Architects, Royal Institute of—Sessional Papers, 1878. Nos. 8, 9. 4to.

Chemical Society—Journal for Feb. and March, 1878. 8vo.

De la Rue, Warren, Esq. D.C.L. F.R.S. &c. M.R.I. (the Author)—Experimental Researches on the Electrical Discharge with the Chloride of Silver Battery. Part 1. (Phil. Trans. R.S. Vol. 169.)

Editors—American Journal of Science for March, 1878. 8vo.
  Analyst for March, 1878. 8vo.
  Athenæum for March, 1878. 4to.
  Chemical News for March, 1878. 4to.
  Engineer for March, 1878. fol.
  Horological Journal for March, 1878. 8vo.
  Iron for March, 1878. 4to.
  Journal for Applied Science for March, 1878. fol.
  Nature for March, 1878. 4to.
  Nautical Magazine for March, 1878. 8vo.
  Pharmaceutical Journal for March, 1878. 8vo.
  Telegraphic Journal for March, 1878. 8vo.
Franklin Institute—Journal, No. 627. 8vo. 1877.
Godman, Frederick Du Cane, Esq. F.L.S. F.Z.S. M.R.I. (*the Author*)—Natural History of the Azores or Western Islands. 8vo. 1870.
Harrison, W. H. Esq.—Psychography: By "M.A. Oxon." 12mo. 1878.
Linnean Society—Proceedings, Nos. 72, 95. 8vo. 1878.
Liverpool Literary and Philosophical Society—Proceedings, Vol. XXXI. 8vo. 1876-7.
Perry, Rev. S. J. F.R.S. (*the Editor*)—Results of Meteorological and Magnetical Observations at Stonyhurst, 1877. 16to. 1878.
Preussische Akademie der Wissenschaften—Monatsberichte: Dec. 1878. 8vo.
Rossetti, Professor F.—Various Papers. 8vo. 1877.
Royal Society of London—Proceedings, No. 185. 8vo. 1878.
Russell, Lord Arthur, M.P. M.R.I.—Ersch und Gruber: Allgemeine Encyclopädie der Wissenschaften:
  1st Series, A to G. 40 vols. 4to. 1818–44.
  2nd Series, H to N. 23 vols. 4to. 1827–44.
  3rd Series, O to Z. 19 vols. 4to. 1830–44.
Scott, R. H. Esq. F.R.S. (*the Editor*)—The American Storm Warnings. (K 102) 8vo. 1878.
Tuson, Professor R. V. (*the Editor*)—Cooley's Cyclopædia of Practical Receipts. Sixth edition. Part 1. 8vo. 1878.
Victoria Institute—Transactions, No. 45. 8vo. 1878.
Vincent, Chas. W. F.R.S.E. F.C.S. (*the Editor*)—Chemistry, Theoretical, Practical, and Analytical, as applied to the Arts and Manufactures. By Writers of Eminence. Division I. Second edition. Division VI. 4to. 1878.
Zoological Society of London—Transactions, Vol. X. Parts 3, 4, 5. 4to. 1877.
  Proceedings, 1877, Parts 3, 4. 8vo. 1877.

The following Arrangements for the Lectures after Easter were announced:—

W. T. THISELTON DYER, M.A. B.Sc. F.L.S. Assistant Director, Royal Gardens, Kew.—Five Lectures on Some Points in Vegetable Morphology; on Tuesday, April 30 to May 28.

LORD RAYLEIGH, M.A. F.R.S.—Four Lectures on Colour; on Thursdays, May 2 to 23.

PROFESSOR HENRY MORLEY.—Four Lectures on Richard Steele; on Saturdays, May 4, 11, 18, 25.

REV. W. H. DALLINGER, F.R.M.S.—Three Lectures on Researches in Minute and Low Forms of Life; on Tuesdays, June 4, 11, 18.

PROFESSOR F. GUTHRIE, F.R.S.—Three Lectures on Studies in Molecular Physics; on Thursdays, May 30, June 6, and June 13.

JAMES SULLY, Esq.—Three Lectures on the Psychology of Art; on Saturdays June 1, 8, 15.

WEEKLY EVENING MEETING,

Friday, April 5, 1878.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

William Spottiswoode, Esq. M.A. LL.D. Tr.R.S. Sec.R.I.

Quartz: an Old Chapter re-written.

It is well known that difficult subjects are often made much more intelligible by being presented to the mind in more than one way. Under such a category the operation of rock crystal, or quartz, upon polarised light may be fairly classed. And although its fundamental laws are well known to students of optics, they are perhaps sufficiently intricate to justify their being brought under a new experimental form before the members of the Royal Institution.

If a Nicol's prism be used as a polariser and a sphere of Iceland spar as an analyser: then, when the sphere is so placed that its axis coincides with that of the beam of incident light, two images are formed on the screen. If the aperture be a circle, or a circular ring, the two images will be concentric circles; the ordinary image being the larger, and the extraordinary the smaller. The light in both images is polarised, but the polarisation in one is radial, in the other tangential. Hence each ring will be broken by two dark intervals opposite to one another, viz. at points where the vibrations would be perpendicular to those transmitted by the polariser; and as the vibrations at each point of one ring are perpendicular to those at the corresponding point of the other, the dark intervals in one ring are in quadrants at right angles to those in the other. If, however, the sphere be turned round (say about a vertical axis), various results will be noticed; first, the large, or ordinary, image will remain fixed, and consequently the rays forming it follow the ordinary laws of refraction; secondly, the smaller, or extraordinary, image will shift both its position and form, showing that the rays forming it follow a more complicated law; thirdly, when the angle of turning has reached 90°, the polarisation which was radial for the first position of the sphere, will now be plane. The change of figure depends upon the fact that the angle of refraction in the extraordinary image varies with the angle of inclination of the incident ray to the axis of the crystal, and is greatest when that angle is 90°: while the change in the polarisation is a result of the general law that the plane of polarisation in the ordinary ray passes through, while that of the extraordinary is perpendicular to the axis of the crystal.

Returning to the original position of the sphere, if monochromatic light be used, the same effect is seen as with white light; but if a plate of quartz cut perpendicularly to the axis be inserted between the Nicol and the sphere, the dark interval will be shifted to the right or to the left according as a right-handed or a left-handed quartz is used. The amount of displacement will depend, for light of a given colour, on the thickness of the quartz; and, for a given thickness of quartz, on the colour, or, more strictly speaking, on the wave length. In this way the rotation of the plane of polarisation by the quartz plate is brought visibly before the eye.

In this experiment, however, the various stages of rotation for the rays of various colours are brought successively into view; but by the following arrangement they may be all exhibited simultaneously. If, while white light is used, and spectra of the ring images be formed, then, on re-introducing the quartz plate, the spectrum of one ring (the ordinary, or the extraordinary, according to the position of the polariser) will be seen to be crossed by a spiral band of darkness, sweeping obliquely from red to blue, or *vice versâ*, according as the quartz is right-handed or left-handed. This shows that the various prismatic colours in continuous succession, are extinguished at various points of the ring; or, in other words, that the plane of polarisation varies continuously in angular position with the wave length. This fact explains the effect seen when the image is formed on the screen without dispersion; viz. the complete image consists of two concentric rings coloured with the residual tints (arising from the extinction of the prismatic colours in succession). These tints run through their cycle once in each semi-circumference; and the tints in each semicircle of one image are complementary to those in the corresponding semicircle of the other. If a biquartz be used, and the line of division between the right-handed and left-handed portions pass through the junction of the red and violet parts of either of the rings, the order of colours in the two semicircles will be reverse to one another.

If with the same arrangement as last described the sphere be turned round through an axis parallel to the line of division of the biquartz, the following results may be observed: first, each of the two images will be tinted by the biquartz in two compartments, the tints of which depend upon the position of the Nicol; secondly, the two images will, as usual, present tints complementary to one another; thirdly, the parts where the complementary tints overlap will, as usual, appear white; fourthly, the parts where the same tints overlap will be more brightly illuminated than the parts which do not so overlap. The interest of the last feature consists in showing, as first noticed by Helmholtz, that the low-tint colours, russet, brown, drab, &c., are really subjective effects due to red, orange, yellow, &c., when feebly illuminated in comparison with some other brighter part of the field.

The effect of right and left-handed quartz in combination is ordinarily shown, and turned to useful account, by a combination of plates, as in Savart's bands, or of wedges, as in Babinet's compensator; but it may also be well illustrated by the following arrangement. A solid cone, having a very obtuse vertical angle, and its axis parallel to that of the crystal, is cut from a right-handed quartz; a second cone, in all other respects similar to the first excepting that it is hollow, is cut from a left-handed quartz; and the two are cemented, one inside the other, by balsam, so as to form a compound plate. When the polariser and analyser are crossed, the following phenomena are observed: The coloured bands are arranged in circles about the centre. Midway between the centre and circumference, where the thicknesses of right-hand and left-hand quartz are equal, a black ring is seen. Inside this the colours are arranged in the order due to right-handed quartz; outside it they are arranged in the order due to left-handed quartz. On turning the analyser round, the middle ring changes from black to white, and the other rings, within and without the middle ring respectively, change their colours in opposite orders. But owing to the fact that the colours are originally ranged in opposite orders, the last-mentioned change gives rise to an optical illusion, in virtue of which the coloured rings seem to flow inwards or outwards throughout, according to the direction in which the analyser is turned.

Another useful combination of quartz may be mentioned. Two series of plates cut perpendicularly to the axis, and in the form of sectors, of various thicknesses, were arranged so as to form circular discs. One of the discs was cut from right-handed, the other from left-handed quartz, and one which was placed in front of the other, was capable of revolving about its centre. When sectors of equal thickness coincided in position, they neutralized one another, and no colour was seen. When, however, by the revolution of the movable disc, plates of different thickness were brought opposite one another, colours were seen due to plates whose thickness was equal to that difference. The vividness of the colours increased to a maximum when the revolution amounted to a right angle, after which it diminished until at two right angles, or half an entire revolution, it again disappeared.

[W. S.]

WEEKLY EVENING MEETING.

Friday, March 29, 1878.

Warren de La Rue, Esq. M.A. D.C.L. F.R.S. Vice-President, in the Chair.

Professor James Dewar, M.A. F.R.S.

Experiments in Electro-Photometry.

(*Abstract.*)

Edmund Becquerel, in the year 1839, opened up a new field of chemical research through the discovery that electric currents may be developed during the production of chemical inter-actions excited by solar agency.

Hunt, in the year 1840, repeated, with many modifications, Becquerel's experiments, and confirmed his results.

Grove, in 1858, examined the influence of light on the polarized electrode, and concluded that the effect of light was simply an augmentation of the chemical action taking place at the surface of the electrodes.

Becquerel, in his well-known work, 'La Lumière,' published in 1868, gives details regarding the construction of an electro-chemical actinometer formed by coating plates of silver with a thin film of the sub-chloride, and subsequent heating for many hours to a temperature of 150° C.

Egeroff, in 1877, suggested the use of a double apparatus of Becquerel's form, acting as a differential combination, the plates of silver being coated with iodide instead of chloride.

The modifications of the halogen salts of silver when subjected to the action of light have up to the present time been used most successfully in the production of electric currents, and although mixtures of photographically sensitive salts have been shown by Smee to produce currents of a similar kind, yet no attempt has been made to examine the proper form of instrument required for the general investigation of the electrical actions induced by light on fluid substances.

This subject has occupied my attention for some time, and when the investigation is completed I shall deal with the results in greater detail. In the meantime the following description will give an idea of the method of investigation.

A little consideration shows that the amount of current produced by a definite intensity and quality of light acting during a short period of time on a given sensitive substance in solution, is primarily a function of the nature, form, and position of the poles in the cell relatively to the direction in which the light enters, and the selective absorption, concentration, and conductivity of the fluid.

The diffusive action taking place in such cells complicates the effects and is especially intricate when insoluble substances are formed. In order to simplify the investigation in the first instance, poles that are not chemically acted upon, and a sensitive substance yielding only soluble products on the action of light, were employed. For this purpose platinum poles and chlorous acid or peroxide of chlorine were selected.

The best form of cell had one of the poles made of fine platinum wire fixed as closely as possible to the inner surface where the light enters, the other pole being made of thicker wire placed deeper in the fluid.

As the action is confined to a very fine film where the light enters, the maximum amount of current is obtained when the composition of the fluid is modified deep enough to isolate temporarily the front pole in the modified medium. Under these conditions the formation of local currents is avoided, and the maximum electromotive force obtained.

In cells of this construction the amount of current is independent of the surface of the fluid acted upon by light, so that a mere slit, sufficient to expose the front poles, acts as efficiently as a larger surface. This prevents the unnecessary exhaustion of material and enables the cell to be made of very small dimensions. By means of such an apparatus the chemical actions of light and their electrical relations may be traced in many new directions.

The amount and direction of the current in the case of chlorous acid is readily modified by the addition of certain salts and acids, and thus electrical variations may be produced, resembling the effects observed during the action of light on the eye.

Certain modifications taking place in the chlorous acid by exposure to light increase its sensibility, and as a general result it is found that the fluid through these alterations increases in resistance. We have thus an anomalous kind of battery where the available electromotive force increases with the resistance. The addition of neutral substances which increase the resistance without producing new decompositions improves the action of the cell.

Care has to be taken in these experiments to use the same apparatus in a series of comparative experiments, as infinitesimal differences in the contact of the active pole render it difficult to make two instruments giving exactly the same results. Cells have been constructed with two, three, and four poles, and their individual and combined action examined. Quartz surfaces have also been employed instead of glass, thus enabling the chemical opacity of different substances to be determined.

The electrical currents derived through the action of light on definite salts are strong in the case of ferro- and ferri-cyanide of potassium, but remarkably so in the case of nitroprusside of sodium.

Of organic acids the tartrate of uranium is one of the most active. A mixture of selenious acid and sulphurous acid in presence of hydrochloric acid yields strong currents when subjected to light in the form of cell described. The list of substances that may be proved to undergo chemical decomposition by the action of light is very extensive; full details will be found in the completed paper.

[J. D.]

WEEKLY EVENING MEETING,

Friday, April 12, 1878.

The Duke of Northumberland, D.C.L. LL.D. Lord Privy Seal, President, in the Chair.

Sir Joseph D. Hooker, C.B. K.S.I. M.D. D.C.L. President R.S. *M.R.I.* &c.

The Distribution of the North American Flora.

Whatever countries beyond the seas we may visit, in the temperate regions of the globe, we find that their vegetation has been invaded, and in many cases profoundly modified, by immigrant-plants from other countries, and these are in almost all cases natives of North-western Europe. Nearly forty years ago I arrived at night at the Falkland Islands, when a boat was sent ashore to communicate the ship's arrival to the Governor; and, being eager to know something of the vegetation of the islands, I asked the officer in charge of the boat to pluck for me any plants he could feel for, as it was too dark to see anything, and the armful he brought to me consisted of nothing but the English shepherds' purse. On another occasion, landing on a small uninhabited island,* nearly at the Antipodes, the first evidence I met with of its having been previously visited by man, was the English chickweed; and this I traced to a mound that marked the grave of a British sailor, and that was covered with the plant, doubtless the offspring of seed that had adhered to the spade or mattock with which the grave had been dug.

It was hence no surprise to me to find myself, on landing at Boston last summer, greeted by Western European plants that had established themselves as colonists in New England. Of these the first was the wild chicory, growing far more luxuriantly than I ever saw it do elsewhere, forming a tangled mass of stems and branches, studded with turquoise-blue blossoms, and covering acres of ground; the very next plants that attracted my attention were the Ox-eye daisy and Mayweed, which together whitened the banks in some places, and which I subsequently tracked more than half-way across the continent.

These, and more than 250 other Old England plants, which are now peopling New England, were for the most part fellow emigrants and fellow colonists with the Anglo-Saxon, having (as seeds) accompanied him across the Atlantic, and having, like him, asserted their supremacy over and displaced a certain number of natives of the soil.

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* Lord Auckland's Island, south of New Zealand.

Turning to the hotter parts of North America, the same process of invasion by natives of the old world is going on; a British-Indian plant * has established itself in the streets of Savannah, and so entirely simulated the habit of a native weed, that American botanists gave it a new name, regarding it as indigenous; and one of the most curious cases of plant invasion known to me is that of the mango tree in Jamaica, which reminds one of the accounts of captured tribes, which after being carried into their conqueror's country, have so increased and multiplied, as eventually to dispossess and supplant their captors. In 1782 Admiral Rodney took a French ship, bound for St. Domingo from Bourbon, with living plants of the cinnamon, jack-fruit, and mango, sent to the Botanical Gardens of the former island by that of the latter. These undistinguished prizes the Admiral presented to the Jamaica Botanical Gardens. There the cinnamon was carefully fostered, but proved to be (as it is to this day) difficult of culture in the island; whilst the mango, which was neglected, became in eleven years as common as the orange, spreading over lowlands and mountains from the sea level to 5000 feet above it. On the abolition of slavery immense tracts of land, especially coffee estates, relapsed to a state of nature, and the mango being a favourite fruit with the blacks, its stones were flung about everywhere, giving rise to groves along the roadsides and settlements; and the fruit of these again, rolling down hill, gave rise to forests in the valleys and on their slopes. The effect of this spread of the mango has been to cover hundreds of thousands of acres, and to ameliorate the climate of what were dry and barren districts, by producing moisture and shade, and by retaining the rainfalls that had previously evaporated; besides the affording food for several months of the year to both negroes and horses. It may well be, that by future generations in Jamaica, Admiral Rodney will be known less for his victory over Count Grasse, and being the first to "break the enemy's line," than as the capturer of the mango tree in the Spanish Main.

And it is the same in all countries colonized by the Anglo-Saxon, so firmly have the plants he has brought with him established their foot- or rather root-hold in the soil, that were he and all other evidence of his occupation to disappear from North America, these his fellow emigrants would remain as witnesses of his former presence, not only on the shores and in the forests of the older States, but in the interior prairie and the newly settled valleys of the Rocky Mountains themselves.

Time does not permit me to dwell longer upon this subject of immigration during the historic period. I must now hasten to consider the flora of North America, as it was for an indefinite period

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* *Fragaria indica*, Andr. (*Potentilla Durandii*, Torr. and Gr.)

before the arrival of the Anglo-Saxon, embracing prehistoric and geological epochs: we have to regard this flora as a whole, and as subdivisible into local floras, characterized by the prevalence of certain assemblages of plants; to connect these local floras with the geographical features of the areas they occupy; to account for their position and composition by a reference to the countries from which their components may have been derived, and to the means of communication which exist, or may in former times have existed with these countries.

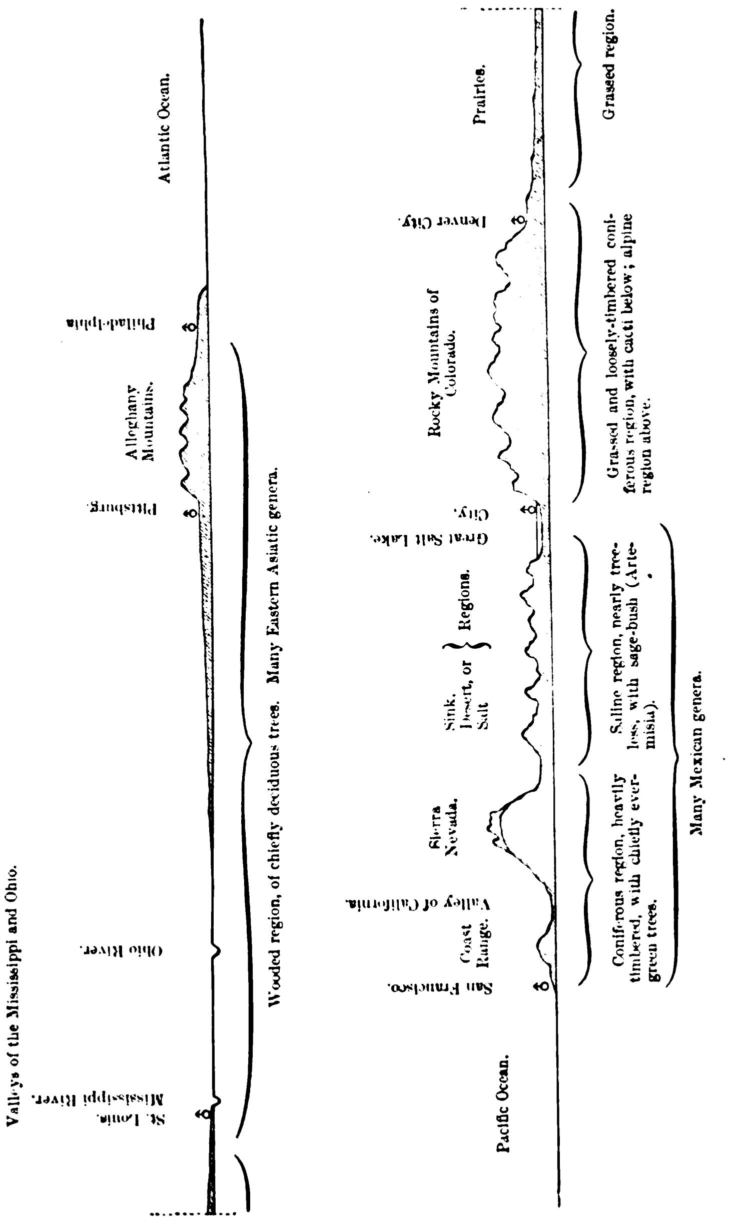
Before proceeding with this inquiry I will indicate, with the aid of the map, those prominent features of North American geography, which have regulated the distribution of its plants.

In the Arctic regions the three northern continents approach, and the hydrography and geography of those regions favour the assumption that in former times they may have been connected. Next we observe that in the American continent (unlike the European and Asiatic), the great obstacles to the intermingling of floras, the mountain chains, are longitudinal; as are the principal valleys, which are the great aids to their diffusion. If we now run a section across the continent along its principal parallel (that near 40°), which approximately coincides with the isotherm of 55°, we find that such a section represents tolerably well any other parallel to it in those meridians in which there is the greatest development of a temperate vegetation. Commencing on the east, there is first the Atlantic seaboard, bounded to the westward by mountain ranges of moderate elevation (rarely attaining 6000 feet), which under various names extend from New Brunswick, in lat. 48°, to Alabama and Georgia, in lat. 34° (and which have been collectively called the Appalachian chain). Westward of this chain are the broad, low, well-watered valleys of the Ohio, Mississippi, and Missouri, the latter at its intersection with our principal parallel being nearly midway across the continent and 1300 miles from the Atlantic. From the Missouri the ascent is very gradual to the elevated region of the Rocky Mountains, which consist of a complicated series of rocky ridges rarely exceeding 14,000 feet elevation, occupying a belt 300 miles broad from east to west. These ridges enclose very large, well-watered, open grassy valleys, called Parks, the rivers from which usually discharge from the range through narrow gorges, called Cañons.

The parks and valleys to the east of the mountainous belt present the grey-green (grassy) vegetation of the prairie, those on the west, the hoary sage-bush (Artemisia) vegetation of the dry country to the westward; and these often intersect, so that a transverse ridge may separate a green and well-watered park from a hoary and dry one.

The descent from the Rocky Mountains on the west, is on to a tract elevated upwards of 4000 feet above the sea, extending for 400 miles to the foot of the Sierra Nevada. This tract is intersected by several short ranges 8000 feet high and upwards; its climate is dry, its soil saline, and many of the rivers lose themselves in salt lakes and

SECTION OF THE NORTH AMERICAN CONTINENT IN ABOUT LAT. 40°.


Valleys of the Mississippi and Ohio.
St. Louis.
Mississippi River.
Ohio River.
Pittsburg.
Allegheny Mountains.
Philadelphia.
Atlantic Ocean.
Wooded region, of chiefly deciduous trees. Many Eastern Asiatic genera.
Pacific Ocean.
San Francisco.
Coast Range.
Valley of California.
Sierra Nevada.
Sink, Desert, or Salt Regions.
Great Salt Lake City.
Rocky Mountains of Colorado.
Denver City.
Prairies.
Coniferous region, heavily timbered, with chiefly evergreen trees.
Saline region, nearly treeless, with sage-bush (Artemisia).
Grassed and loosely-timbered coniferous region, with cacti below; alpine region above.
Grassed region.
Many Mexican genera.

marshes, whence the local names of Great basin, and of the Sink, Salt-lake, and Desert regions. The Sierra Nevada succeeds, rising steeply to an elevation of 12,000 and sometimes of 15,000, feet. Under various names it traverses America, with little interruption, from Alaska to Southern California, at a distance of 100 to 150 miles from the Pacific; but its breadth is nowhere so great as that of the Rocky Mountains. The descent from it to the westward is into the great valley of California, whose floor is raised but little above the sea level, and between which and the Pacific are the low and narrow coast-ranges, of which the southernmost in southern California unites with the Sierra Nevada.

Turning now to the flora of North America north of the tropic, we find that the distribution of its plants is in remarkable conformity with its geographical and climatal features, being in meridional belts from the Arctic Ocean to the Gulf of Mexico; the botanical components of these belts differing more and more in advancing south, till in the principal parallel that we have traced, the diversity between the eastern and western belts is greater than between any two similarly situated regions on the globe.

Polar Area.—Commencing in the Polar area, the Arctic American flora, though on the whole a uniform one, is distinctly divisible into three; the first extends from Behring's Straits to the mouth of the McKenzie River, and is marked by the presence of certain Asiatic genera and species that advance no farther eastward; the second extends thence onwards to Baffin's Bay, and presents various American genera and species not found either eastward or westward of it; and the third is that of Greenland, which is almost exclusively European, and presents several anomalies, which I shall hereafter discuss. Besides this eastern and western distribution of the Arctic flora, it streams southward along the three meridional mountain chains of the continent.

British North-American Flora.—South of the Arctic flora is that of the British possessions, that is of temperate America north of the 47th parallel; it consists of a mixture of North European, North Asiatic, and American genera, in very different proportions, disposed in five meridional belts. 1, to the eastward, the Canadian forest region; 2, the woodless region, a continuation of the prairie region farther south; 3, the Rocky Mountain region, where Mexican genera appear; 4, a dry region, a continuation of the Desert or Sink regions to the south of it; and 5, the Pacific region, which assimilates very closely in its vegetation to that of Kamtschatka.

United States Flora.—It is on entering the United States that the flora of temperate North America attains its great development of genera and species in all the meridians, and that the boundaries of the meridional belts of vegetation are most strictly defined.

I. *The Great Eastern Forest region,* extending over half the continent, and consisting of mixed deciduous and evergreen trees, reaches from the Atlantic to beyond the Mississippi, dwindling away as it ascends the western feeders of that river on the prairie. It is noteworthy for the number of kinds especially of deciduous trees and shrubs that are to be found in it, even on a very limited area. Of this I shall select two examples from my Journal. One was a patch of native forest, a few miles from St. Louis, on the Missouri, where in little more than half an hour, and less than a mile's walk, I saw forty kinds of timber trees,* including eleven of oak, two of maple, two of elm, three of ash, two of walnut, six of hickory, three of willow, and one each of plane, lime, hornbeam, hop-hornbeam, laurus, diospyros, poplar, birch, mulberry, and horse-chestnut; together with about half that number of shrubs.

The other example was afforded me by Goat Island, which divides the great cataract of Niagara, and covers less ground than Kew Gardens. Here the vegetation was more boreal and less varied than in Missouri; but with Dr. Gray's aid I counted thirty kinds of trees, of which three were oaks and three poplars, together with nearly twenty different shrubs.

I know of no temperate region of the globe in which any approach to this aggregation of different trees and shrubs could be seen in such limited areas, and perhaps no tropical one could afford a parallel.

No less remarkable is the composition of the flora of the Eastern States. Professor Gray has shown that most of its genera are common to Europe and Asia, but very many are all but confined to North-eastern Asia and Western America. This generic identity, however, gives but a faint idea of the close relationship between the East American and East Asiatic, especially the Japanese, floras, for there is further specific identity in about two hundred and thirty cases, and very close representation in upwards of three hundred and fifty; and what is most curious is, that there are not a few very singular genera, of which only two species are known, one in East Asia, the other in East America; and in some of these instances the Asiatic species is a widespread plant in East Asia, whilst the American is an extremely scarce and local plant in its country, which and other considerations render it conceivable that the Asiatic element in East America is a dying-out one.

Leaving out of consideration the purely American genera of this flora, there remain the genera common to Europe, Asia, and America; the genera confined to America and Asia; and the genera confined to America and Europe. I shall give an illustration of the proportions in which these occur by a reference to the principal trees and large shrubs only, their names being familiar to you, though the smaller shrubs and herbs afford infinitely more numerous and striking examples; thus, of those common to the three northern continents, I find in America thirty-eight genera with about one hundred and fifty species, these include maples, ashes, hollies, elms, planes, oaks, chestnuts, nut, hornbeam, birches, alders, willows, beech, poplars, &c. Of those confined to America and East Asia I find in America thirty-three genera and fifty-five species, including magnolias, tulip tree, negundo, wistaria, Virginia creeper, gleditschia, hydrangea, liquidamber, nyssa, tecoma, catalpa, diospyros, sassafras, benzoin, mulberry, walnut, and others, which, not being European, are unfamiliar to you. Lastly, of those confined to Europe and America I find only one genus, namely the hop-hornbeam, of which there is but a single representative in each country.

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* For the indication and names of them I am indebted to Dr. Engelmann of St. Louis, who took me to the forest.

Here then is conclusive evidence of the close botanical relationship of North-eastern Asia and Eastern North America; a relationship of which there is but little evidence in the vegetation of the Prairies and Rocky Mountains, and still less, perhaps, in the regions farther west.

II. The *Prairie region* succeeds, a grassy land with many peculiar herbaceous American genera, including Mexican types, of which last the most conspicuous are a yucca and cacti, which latter increase in number as the Rocky Mountains are approached, where they form a noticeable feature in the landscape.

In the parks and lower valleys of the Rocky Mountains deciduous trees are few and scattered, and the forest is an open one of conifers, amongst which a pine, allied to the Mexican nut-pines, *P. edulis*, first appears. Higher on the mountains the coniferous forests are dense, and almost the only deciduous tree is an aspen, which forms impenetrable brakes on the slopes and in the gullies. Above the forest region are the sub-alpine and alpine regions, presenting a mixture of European, Asiatic, and American types.

III. Descending to the Sink region the cacti and yucca almost disappear, though they increase to a maximum farther south in this meridian. Deciduous trees are very few, and confined to the gullies of the mountains, and Mexican genera increase in numbers. The hoary sage-bush (Artemisia) covers immense tracts of dry soil, and saline plants occupy the more humid districts.

Another nut pine of Mexican affinity (*P. monophylla*) traverses the centre of this region in a narrow meridional strip, and the proportion of endenic plants, herbaceous especially, is very large.

IV. The *Sierra Nevada* is clothed with the most gigantic coniferous forest to be found on the globe, amongst which a very few species of duciduous trees are scattered; but none of these are identical with trees of the Eastern forests, though several are representative of them. New Mexican genera occur at all elevations from the crest of the range to its base, and thence extend across the Californian valley and the Coast-ranges to the Pacific, mixed with northern West American genera and species.

In this slight outline of the botanical features of temperate and Arctic North America, I have alluded to three as most noteworthy, namely, the vegetation of Greenland, the Asiatic character of the vegetation of the eastern half of the continent, and the more southern and even Mexican character of the vegetation of the western half. How are these features to be accounted for?

It so happened that Dr. Gray, Professor of Botany in Harvard College (Boston), and I were contemporaneously, but without concert, engaged in botanical investigations, which have resulted in explanations of the two first features. He was at work on the flora of Japan,[*] I on that of the Polar Zone,[†] and we were both bringing to bear upon our subjects considerations regarding the variation of species which Mr. Darwin[‡] almost simultaneously laid before the public, and which, I need not say, powerfully directed our studies.

I shall take the vegetation of Greenland first, as being first in order, though second in date of appearance and least in importance. Its chief peculiarities are, 1, that its plants are almost all of them Scandinavian (that is, North-west European), hardly any of the peculiar plants of the American Arctic sea-coast and Polar islands crossing Baffin's Bay and Davis Straits; 2, that of its 300 flowering plants hardly any present even a variation from their Scandinavian prototypes; 3, that it is poorer in species than is any other division of the Arctic flora, and wants many Scandinavian plants that are found in most other Arctic countries; 4, that though Greenland extends 400 miles south of the Arctic circle, its extra-Arctic continuation adds only about 100 species to the flora, and these all cross the Arctic circle in other longitudes; 5, some Greenland species are confined to it and to the mountains of the Atlantic side of America, being found nowhere else in Arctic or sub-Arctic America.

My explanation of these anomalies was, that at a period previous to the Glacial, a flora common to Scandinavia and Greenland was spread over the whole American Polar area, and that, on the accession of the cold of that period, this flora was driven southwards, and was affected differently in different longitudes. In Greenland many species were exterminated, being as it were driven into the sea at the southern extremity of the peninsula, where only the hardiest survived. On the return of warmth the Greenland survivors migrated northward, peopling the peninsula with the hardiest of the species of its former flora, unmixed with American species; and unchanged in aspect, from never having been brought into competition with those of any other flora. On the other hand, the same Scandinavian plants when driven south on the plains of the continent, multiplied there in individuals, and being brought into competition with American species descending from the continental mountains on to the plains, assumed varietal forms. On the return of warmth, therefore, many Scandinavian species that had been exterminated in Greenland, would, having survived on the continent, travel northwards on it, some unchanged, others under varietal forms, accompanied with American species that had descended from the mountains during the cooling of the continent. Lastly, as some of the Scandinavian species were no doubt local, and confined to near the meridian of Greenland, it is not surprising to find that a few such should survive only in Greenland and on the eastern alps of North America.

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[*] "Observations upon the Relations of the Japanese Flora to that of North America, and of other parts of the North Temperate Zone." 'Memoirs of the American Academy of Sciences,' vol. vi. p. 377. Read December 14, 1858, and January. 11, 1859.

[†] "Outlines of the Distribution of Arctic Plants." Read before the Linnean Society of London, June 21, 1860. 'Trans. Linn. Soc.' xxiii. p. 257.

[‡] "On the Tendency of Species to form Varieties," by C. Darwin, Esq., F.R.S., and Alf. Wallace, Esq. Read July 1, 1858. 'Journal of the Proceedings of the Linnean Society of London,' vol. iii. (Zoology), p. 45.

Thus only could I satisfactorily account for the almost complete identity of the Greenland flora with the Scandinavian after such changed conditions of climate; for the paucity of its species; for the absence in it of varieties; for the rarity in it of peculiarly American species; for the few species which extra-Arctic Greenland adds to its Arctic flora; and for certain of its plants being limited in range to Greenland and the eastern American alps.

The relationship between the flora of North-east Asia and Eastern North America has been fully explained by Dr. Asa Gray in an essay on the Flora of Japan, which is the first entirely satisfactory contribution of its kind to the science of Botanical Geography known to me.

After a detailed comparison of the botany of Japan and North America, and proving their affinity, Professor Gray refers to the fact that many of the existing genera and even species of both floras coexisted in the high latitudes of America during Miocene times, as shown by Heer, and other palæontologists; during which period he further assumes that the three northern continents were conjoined, or so contiguous as to allow of a commingling of their floras.

The Glacial period followed, carrying an Arctic climate south to the latitude of the Ohio, but so gradually, that these plants were not exterminated, but wholly or in part driven southward, followed in the rear by the Arctic vegetation. As the temperature rose with the retreating ice, this flora returned northward, leaving the Arctic and sub-Arctic plants on the mountains of both East and West America.

He next shows that the retreat northward was to a somewhat higher latitude than the same plants now attain; and this he accounts for by a reference to the Fluvial epoch of Dana,* when the region of the Great Lakes was submerged 500 feet below their present level. This diminished area and lowered elevation of the land, by inducing a milder climate than now obtains in the Lake region, favoured the extension of the flora to a higher latitude than it now attains, and hence effected a second commingling of American and Asiatic plants. Lastly, Dana's Terrace epoch supervened, when the previously depressed northern region was again raised, cooling the climate, finally dissociating the Asiatic and American floras, and giving to the Arctic and sub-Arctic plants of the continent their present limits.

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* Whilst these pages were still in the press, Prof. Gray has informed me that he now lays little stress on the conditions supposed to be due to the Glacial and Fluvial epochs; and that he is rather disposed to consider the separation of the northern floras by the Glacial epoch to have been final.

It remains now to account for the great rarity of East Asiatic types in America west of the Prairies, and the presence in those meridians of Mexican and still more southern ones. Hitherto there have been no other attempts at a solution of this problem than such unsupported speculations as that the western half of the continent, though so much the loftier, was submerged during the southern migration of the northern Miocene plants; or that the climate of the West was unsuited to the habits of these, which appears to me to be at variance with the fact that when imported into it they thrive luxuriantly.

The explanation which I have to offer will be best understood by a reference to our section (p. 571), which shows the western half of the continent to be enormously elevated as compared with the eastern, and to have been singularly adapted for the retention of vast bodies of ice for long after the Glacial period. We find there a valley (the desert region), upwards of 400 miles broad, and upwards of 4000 feet elevation, with many ranges of over 8000 feet in it, bounded by broad and lofty mountains, together occupying at least two-thirds of the breadth of the western half of the continent. We further know that these mountains were clothed with ice during the Glacial epoch, and that the valley was then occupied by a vast lake; for on the uppermost of the many shelves which the retiring waters of this lake cut on the flanks of the Rocky Mountains and Sierra Nevada, the skull of the musk-ox, the most Arctic of land quadrupeds, has been found.

It is obvious that this whole western region must have retained its glacial mantle for an incalculable period after Eastern America had been sufficiently warmed to admit of the northward return of the plants that had been driven southward in it; and that this glaciated condition must have effectually barred a similar return of the same plants in those western meridians; these must have perished in short on reaching Southern California. Long ages after, when the western ice disappeared, and the climate of the valleys warmed, the Mexican and more southern plants would, as a matter of course, take possession of the unoccupied soil, and advance northward till they encountered the boreal vegetation of North-western America, with which they now commingle.

I have said that the extinction of East Asiatic types in Western America was not total; a few escapes are found in the valleys of the Rocky Mountains and Sierra Nevada,* and also along the coast of the Pacific, the warming influence of which favoured their preservation during their northern migration.

Two instances of these escapes are of such interest that I shall in concluding this lecture bring them under your notice; they are those giants of the vegetable kingdom, the Sequoias, the Redwood (*S. sempervirens*), and the "Big-tree," or "Wellingtonia (*S. gigantea*).

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* And also on the highlands of Central Mexico, where some Asiatic types remain which have not migrated farther north or south in America. Such are the eminently Asiatic genera Bocconia, Meliosma, Photinia, Cotoneaster, Deutzia, and Abelia.

The fossil remains of these trees, or species most closely allied to them, are found in Miocene beds in high latitudes all round the globe; in Vancouver's Island, Sitka, on the Arctic American seacoast, in Greenland, Spitzbergen, and in Arctic Asia, &c. The genus, therefore, which first appeared in the Cretaceous times, was undoubtedly a member of that mixed Americano-Asiatic flora that was driven southward during the Glacial period. The genus is now confined to western North America, and to the two above-named species, but is represented in Eastern America by the very closely allied genus *Taxodium*, and in Eastern Asia by *Glyptostrobus*.

The distribution of the two Sequoias is most instructive. The redwood forms a dense narrow forest tract for about 500 miles, skirting the ocean, along whose warmer shore it crept northward after the Glacial epoch. It rivals in height its sister of the Sierra, and attains an enormous girth and age, though I can find no account of any attempt having been made to estimate its age.

The *S. gigantea* or "Big-tree" (the *Wellingtonia* of British gardens), again, is a plant of a cooler climate; and hence, having survived the glacial cold, was enabled to establish itself in the Sierra Nevada, under certain very restricted conditions. It extends at intervals along the western slope of the Sierra to a little north and south of the parallels of 36° and 38° N., that is for nearly 200 miles in a north-west and south-east direction, at elevations of 5000 to 8000 feet above the sea. Towards the north the trees occur as very small, isolated, remote groves of a few hundreds each, most of them old and interspersed amongst gigantic pines, spruces, and firs, which appear as if encroaching upon them; such are the groves visited by tourists (Calaveras, Mariposa, &c.). To the south, on the contrary, the Big-trees form a colossal forest, forty miles long and three to ten broad, whose continuity is broken only by the deep sheer-walled cañons that intersect the mountains; here they displace all other trees, and are described as rearing to the sky their massive crowns; whilst seen from a distance the forest presents the appearance of green waves of vegetation, gracefully following the complicated topography of the ridges and river basins which it clothes.

But by far the most remarkable fact hitherto reported regarding the disposition of the groves is, that they occupy only those spots in the Sierra which were first laid bare when its icy mantle became broken up into isolated glaciers. Thus, commencing at the north, the gap of 40 miles between the Calaveras and Tuolome groves was occupied by the great glacier of the Tuolome and Stanislaus rivers; that between the Merced and Mariposa groves by the glacier of the Merced River, which sculptured the famous Yosemite Valley; and so on, each successive group of trees occupying a lofty spur between the sites of ancient glaciers, and the greatest continuous extension of the forest (of 40 miles) occurring exactly where, owing to the topographical peculiarities of the region, the ground was most perfectly protected from great fields of ice.

Mr. Muir, a very intelligent and accurate observer, who has studied the groves throughout their length and breadth most diligently,[*] and to whom I am indebted for the above and much other information regarding the southern forest of Big-trees, considers that these have never since the Glacial epoch been more widely distributed or in greater vigour than now, and doubts, indeed, if the forests have reached their prime, founding his opinion on the high state of health of the mass of the trees, the multitude of seedlings and saplings in the southern groves, and the absence of any trace of trees having existed outside the present limits of the groves (as of dead trees, stumps, or the great holes left by fallen trees).

So little that is trustworthy has hitherto been published regarding the age, size, and durability of the Big-tree trunks when fallen, that I shall offer you some accurate data which I obtained on these points chiefly from Mr. Muir. A tree felled in 1875 had no appearance of age; it was 69 feet in girth inside the bark, and the number of annual rings counted by three persons varied between 2125 and 2139. Another was 107 feet in girth inside the bark at 4 feet from the ground; its wood was very compact, and showed, throughout a considerable portion of the trunk, 30 annual rings to the inch. This, if the rings were of uniform diameter in the rest of the trunk, would give the incredible age of 6400 years; but as the interior rings of such trees are much broader than the outer, half that number to the inch is a more conceivable estimate, which would give an age of 3500 years. The only other instance of careful counting of rings which I can find, is of that of the felled tree in the Calaveras grove, which measured 70 feet girth inside the bark, at 6 feet above the ground, and which at 40 feet above the ground had 1255 rings. In this case the rings next the bark were 33 to the inch, a number which at 5 feet inward had diminished one-half. The result of many measurements, chiefly by Professor Whitney,[†] gives as the average height of full-grown trees, 275 feet, and a maximum a little over 320, and a girth outside the bark, at 6 feet above the ground of 70, with a maximum of 120; whilst the maximum age possibly attained may be 4000 years, though this is very improbable.

The duration of the dead wood in the forest is very great. I rarely observed signs of rot in the fallen trees I examined, whilst in similar forests in Northern California I saw gigantic trunks of silver firs forming mounds of rotten débris without an atom of sound wood, and this in two years after their fall, as I was assured. I had no data for ascertaining the length of time during which any of the prostrate

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[*] "On the Post-glacial History of Sequoia Gigantea," by John Muir, of San Francisco, Cal. (Proceedings of the Amer. Assoc. for the Advancement of Science, Buffalo meeting, Aug., 1876).

[†] Very careful measurements of the trees in the Calaveras and Mariposa groves are given by Professor Whitney (State Geologist) in the 'Yosemite Guide Book,' published under the authority of the Geological Survey of California (1874).

Sequoia trunks which I saw may have lain on the ground, but Mr. Muir has supplied me with a very crucial case. It is that of a prostrate trunk with no signs of decay in any part of it, which had been burnt in two by a forest fire, and in the trench between the severed portions of which a silver fir grew. This fir was felled, and had 380 annual rings; therefore, to estimate the time during which the Sequoia trunk had lain uninjured, we must add to the 380 years, first the time it lay before the forest fire burnt it in two, and then the unknown interval between that time and the arrival of the silver fir seed.

The milleniums during which these Sequoia trees must have remained *in statu quo*, proving the long duration of existing conditions of climate, are but as minutes compared with the time occupied by the migration of this very species, or its ancestors, north and south in the continent of America. Whatever might otherwise be the extent of the Sequoia's travels, they are now at an end. Man has pronounced the sentence, "Thus far shalt thou go, and no farther!" The doom of these noble groves is sealed. No less than five saw-mills have recently been established in the most luxuriant of them, and one of these mills alone cut in 1875 two million feet of Big-tree lumber; and a company has lately been formed to cut another grove. In the operations of the California woodcutters the waste is prodigious. The young, manageable trees are first felled; after which the forest is fired to clear the ground and get others out, and thus the saplings are destroyed. More destructive still are the operations of the sheep-farmers, who fire the herbage to improve the grazing, and whose flocks of tens of thousands of sheep devour every green thing, and more effectually than the locust. The devastation of the Californian forest is proceeding at a rate which is utterly incredible, except to an eye-witness. It is true that a few of the most insignificant groves of the Big-trees at the northern extreme of its range are protected by the State Legislature, and that a law has been enacted forbidding the felling of trees over 15 feet in diameter; but there is no law to prevent the cutting or burning of the saplings, on which the perpetuation of the grove depends, or to prevent the burning of the old trees, which, if they do escape the fire, will succumb to the drought which the sweeping away of the environing forest will occasion.

During the last quarter of a century the Anglo-Saxon has been ruthlessly carrying fire and the saw into the forests of California, destroying what he could not use, and sparing neither young nor old, and before a century is out the two Sequoias may be known only as herbarium specimens and garden ornaments; indeed, with regard to the Big-tree, the noblest of the noble coniferous race, the present generation, which has actually witnessed its discovery, may live to say of it, that "The place which knew it shall know it no more."

[J. D. H.]

ANNUAL MEETING,

Wednesday, May 1, 1878.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1877, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. During the last twenty-five years the number of Members paying annually (five guineas) has increased from 344 to 544. The Real and Funded Property now amounts to above 84,500*l.*, entirely derived from the Contributions and Donations of the Members.

Forty-one new Members paid their Admission Fees in 1877.

Sixty-two Lectures and Nineteen Friday Evening Discourses were delivered in 1877.

The Books and Pamphlets presented in 1877 amounted to about 190 volumes, making, with those purchased by the Managers, a total of 371 volumes added to the Library in the year, exclusive of periodicals.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, D.C.L. LL.D. the Lord Privy Seal.

Treasurer—George Busk, Esq. F.R.C.S. F.R.S.

Secretary—William Spottiswoode, Esq. M.A. LL.D. Treas. R.S. Corresponding Member of Academy of Sciences, Paris.

Managers.

George Berkley, C.E.
William Bowman, Esq. F.R.S.
Thomas Boycott, M.D. F.L.S.
Warren De La Rue, Esq. M.A. D.C.L. F R.S.
Sir T. Frederick Elliot, K.C.M.G.
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The Hon. Sir Wm. R. Grove, M.A. D.C.L. F.R.S.
Thomas Hyde Hills, Esq.
The Lord Lindsay, M.P. Pres. R.A.S.
Sir W. Frederick Pollock, Bart. M.A.
John Rae, M.D. LL.D.
Chas. Wm. Siemens, Esq. D.C.L. F.R.S.
Benjamin Leigh Smith, Esq.
James Spedding, Esq.

Visitors.

John Frederic Bateman, Esq. F.R.S. F.G.S.
Charles Brooke, Esq. M.A. F.R.S.
William Henry Domville, Esq.
Sir Philip de Malpas Grey Egerton, Bt. M.P. F.R.S.
Alexander John Ellis, Esq. F.R.S. F.S.A.
Edward Enfield, Esq.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
William Watkiss Lloyd, Esq.
Vernon Lushington, Esq. Q.C.
Henry Pollock, Esq.
Lachlan Mackintosh Rate, Esq.
The Rev. Arthur Rigg, M.A.
Basil Woodd Smith, Esq. F.R.A.S.
George Andrew Spottiswoode, Esq.
The Lord Vernon.

WEEKLY EVENING MEETING,

Friday, May 3, 1878.

PROFESSOR E. FRANKLAND, D.C.L. F.R.S. &c. *M.R.I.* in the Chair.

W. SPOTTISWOODE, Esq. LL.D. Tr.R.S. Sec.R.I.

A Nocturne in Black and Yellow.

IT is well known that the coloured bands and rings shown by white light, when polarised and transmitted through crystals, fade, and cease to be visible when the retardation of the rays, due to the thickness of crystal traversed, is large. The feebleness of tint and confusion of definition arises from the overlapping of figures of different colours. But when monochromatic light is used no mixture of colour can take place, and the bands and rings remain perfectly defined, even when the thickness of the crystal is considerable. The more complicated figures produced by two plates of crystal are still more liable to obliteration; and the use of monochromatic light is in this case even more important for maintaining the integrity of the phenomena.

One essential requisite for bringing out the figures in question is purity of colour in the light employed. On this account the ordinary method of absorption by coloured media fails; and it is only by the use of a monochromatic source of light that a satisfactory result can be obtained. For eye observations, a spirit lamp, sometimes with the addition of a little salt, suffices; but the illumination from this source is insufficient for projection on a screen. My first attempt at supplying this requisite was made by replacing the lime in an oxy-hydrogen lamp by a cylinder of carbon through which a hole was bored and filled with chloride of sodium. This answered very well as long as there was enough sodium in the presence of the jet; but it was found difficult to maintain a constant supply at the particular point required. The carbon and sodium were next replaced by a kind of glass formed by melting borax, and to this small pieces of hard German glass were occasionally added. A bead of this substance was placed in a small platinum cup, so fixed that the jet of mixed gases could play upon it at a distance of about three-sixteenths of an inch. This arrangement serves perfectly for laboratory and experimental work; but for projection on a large scale a still more powerful source of light is required. For a burner adapted to lecture-purposes I am indebted to a suggestion of Professor Dewar. The burner consists of an oxy-hydrogen jet, with the addition to the hydrogen tube of a chamber containing metallic sodium. The metal is volatilized by a Bunsen's burner placed below it; so that the hydrogen emerges charged with sodium vapour. The result is a bright monochromatic light.

Let us begin our experiments with the simple case of a Babinet's compensator traversed by a beam of parallel rays. This instrument consists of two quartz wedges, having the axis of the crystal contained in one of the plane faces of each; but the axis in one wedge is parallel, in the other perpendicular to the refracting edge. The wedges are usually made with a refracting angle too small to show dispersion, but of such a thickness that the bands of colour due to their wedge form, shown by each singly, are pale. When the wedges are placed with their edges turned in opposite directions they form a plate substantially of uniform thickness; but owing to the fact that the axes are perpendicular to one another, the ray which has traversed one wedge as the ordinary or swifter, will traverse the other as the extraordinary or slower ray. Hence the total retardation of the rays will be that due to the difference of thickness of the wedges at each point; and therefore the compound plate will be optically of thickness zero along a central line parallel to the edges, and of increasing thickness towards each side.

If one of the wedges be turned through a right angle (say, to its second position), so that their axes are coincident, the two will form a wedge whose thickness, greatest at one angle, diminishes diagonally, at a rate double of that of each wedge singly.

If the same wedge be turned through a second right angle (say, to its third position), the refracting edges will be coincident, and the axes at right angles. The whole will then form substantially a wedge with a refracting angle double of that of each. But since the rays will at every point have traversed an equal thickness of each crystal, the one as an ordinary and the other as an extraordinary ray, the retardation will be everywhere neutralized.

In the first position, with white light, there will be a central band, dark or bright according as the polariser and analyser are crossed or coincident; and on each side first a white, or dark, band, and then coloured bands, whose tints are fainter in proportion to their distance from the centre. With monochromatic light, the bands are alternately dark and bright, and all equally well defined. In the second position, the bands are diagonal, and are perfectly distinct with monochromatic light, even when, as usual, the thickness of the wedges is too great to show them with white light. In the third position the field is uniformly dark or bright, whatever be the nature of light used.

If convergent or divergent light is used, uniaxal crystals (with which alone we are here concerned), when cut perpendicularly to their axes, show the well-known systems of isochromatic rings and dark brushes. When cut at other angles, they show portions of the same systems. But when two such plates are used in combination, theory indicates the presence of some secondary phenomena, which it is our business now to investigate. Of these only very small portions are visible with white light; but with monochromatic light the configuration may be traced throughout the entire field of view.

The effect of the two plates of crystal in producing secondary effects due to the crossing of two sets of isochromatic curves may be well illustrated by Tisley's harmonograph. In this instrument the figures due to vibrations having two rectangular components of (generally) different periods, are approximately produced. This is effected by a compound pendulum: but with the details of this ingenious piece of mechanism we are not here concerned. The curves due to various intervals between the components are well known; but the instrument does not accurately trace out circles or ellipses, or any other re-entering curves; for, owing to the friction, which gradually diminishes the amplitude of vibration, its traces are transcendental curves, or spirals. But on the other hand the friction, and the consequent deviation of the curves from their normal form, is so small that the various turns of the spirals may be taken as representing a series of rings with a sufficient degree of accuracy for our present purpose. Two features of the harmonograph curves should be noticed. First, as actually drawn, they are not geometrical lines, but bands of finite breadth; and on that account are the better suited to represent the interference rings of crystals. Secondly, the distance between the several convolutions of the harmonograph curves is greatest near the outer part of the figure and less towards the centre, while in the crystal rings the reverse is the case; and with reference to any secondary figure due to the crossing of the curves or rings, this difference must be borne in mind.

The curves here used as representing the rings of uniaxal crystals are those produced when the two vibrations of the pendulum are in unison, viz. they are as nearly circular as may be, but it is difficult to avoid a slightly elliptic form. A plate, originally drawn by the instrument, has been photographed twice; and the two facsimiles are now together projected on the screen. The secondary figures in question are then seen in the portion of the field comprised between the centres. I have selected three such pairs and fixed them with their centres at suitable distances; one of them shows ellipses; another parallel straight lines; the third hyperbolas, as secondary figures. If one plate be made to slide over the other, the following effects are usually observed. When the centres are near together, the crossing of the curves gives rise to secondary hyperbolas; as the centres recede, the hyperbolas, at first rectangular, become oblique; they then collapse into straight lines parallel to the plane passing through the axes; and finally they are converted into ellipses approximating more and more to circles as the centres recede still farther from one another. I have, however, found a pair of plates in which the order of the figures is reversed, and which consequently represent the phenomena as they actually occur with crystals.

Similar secondary figures are produced by two plates of crystal used together, as will presently be seen. In order to examine their mode of formation, it will be simplest to begin with some thick plates and moderately convergent light, by which means the details may be brought out on a sufficiently large scale. For this purpose I have three pairs of plates, cut respectively at 67° 30′, 45°, 22° 30′ to the axis. These, and more particularly the second, are used for producing the well-known Savart's bands. If convergent light be made to traverse any of these plates, some portion of the rings are produced. The proportion of the ring system contained in the field of view depends upon the convergence of the rays; and the distance of the centre of the rings from that of the field upon the inclination to the axis at which the plate is cut. In the specimens here used the rings, for reasons stated in connection with Babinet's compensator, are invisible with white, but visible with monochromatic light.

If a pair of these plates be used, and the principal plane of one, originally coincident with that of the other, is made to turn gradually through 180°, then with white light a series of coloured bands is produced. The distance between the bands increases, from a minimum when they are first visible, indefinitely as the angle of turning approaches 180°; while the brilliancy of their tints and accuracy of definition attains its maximum at 90°. With monochromatic light, when the angle between the principal planes of the plates is 0°, the vibrations will traverse the second plate in the same manner as they traversed the first; and the retardation between the ordinary and the extraordinary rays will be double of that due to each plate singly. The two plates will thus act as a plate of double thickness, and the number of bands (portions of rings) visible with one plate, will be doubled. When one plate is turned as before in front of the other, and the angle between their principal planes is gradually increased, the rings due to one plate cross those due to the other; and the intensity of illumination at the overlapping parts will depend upon the angle at which the rings cross one another. Bearing in mind the fact that at every point of the field the polarisation of one ray is parallel, and that of the other perpendicular to the direction of the ring, it is seen that when the rings cross at 90°, the ray which traversed the first plate as an ordinary, will traverse the second as an extraordinary ray; and consequently the retardation due to the two plates together will be the difference of that due to each alone. The result will therefore be similar to that produced by the two wedges when their refracting edges are at right angles; and the field will be crossed by diagonal straight lines.

For all other angles of crossing, including the case particularized above, the following will take place. Each ray which enters the first plate will emerge as two rays polarised, the one radially, the other tangentially with respect to the rings, and with a certain retardation. On entering the second plate, the vibrations of each of these rays will be again resolved radially and tangentially with respect to the rings due to the second plate. Each of the new components, whether radial or tangential, will therefore consist of two parts, generally of different intensities, one of them having been retarded behind the other in their passage through the first plate. The two parts of each tangentially vibrating ray will partially interfere; and so likewise will the two parts of each radially vibrating ray. In consequence of this interference, the two rays emerging from the second plate will in general be of different intensities, and one of them will be retarded behind the other in their passage through that plate. Finally all the vibrations will again be resolved into one plane by the analyser; and when so resolved they will in general partially interfere. This partial interference will cause the dark rings due to the first plate to be broken with patches of partial brightness, and the bright rings with patches of partial darkness, where the rings of one system cross those of the other. The general effect is in many cases that of a diaper-pattern over the field of view.

The distribution of these interruptions and the nature of the secondary figures which they form, will depend upon the curvature and angle of crossing, of the rings at each point of the field. The mathematical formulæ, which give the details of the illumination, present no difficulty beyond tediousness of calculation. And it will be sufficient here to say that, when carried only to a first approximation (with respect to the angle of incidence of the rays) they indicate, for the secondary figures, a series of straight lines alternately dark and bright, known as "Savart's bands." When carried to a second approximation the formulæ indicate that in the neighbourhood of the ring-centres, the secondary figures will be conic sections. When the principal planes of the crystals (planes containing the axis and the normal to the plate) are at 180° to one another, the conic sections are central. In that case, the expression for the square of one of the principal axes of the curve is a cubic in the line of the angle at which the crystal has been cut. This expression when equated to zero must, by the theory of equations, have one real root; in other words, it will vanish for one particular value of the angle, and be negative for greater values and positive for less values of the angle. If, therefore, the crystals be cut at an angle to the axis smaller than the angle given by the cubic equation, they will, when placed with the axes inclined to opposite sides of the field, show hyperbolas for the secondary figures; when cut at a certain angle (about 59° 50′ in the case of quartz), the figures will be straight lines parallel to the line joining the ring-centres; and when cut at a greater angle, the figures will be ellipses, approximating to circles as the angle of section approaches to 90°.

But leaving aside the mathematical aspect of the question, the principal interest of the method of monochromatic light consists in the simplicity of the results, and in the opportunity which it affords of examining in detail all the effects due to two plates of crystal. It enables us in fact to follow the peculiarities of the secondary figures throughout the entire field of view, and to trace by a continuous process the modifications which these figures undergo when the relative positions of the crystals are changed.

Many of these effects are best seen with the optical arrangement usually employed for showing the crystal rings. I have here four pairs of quartz plates cut respectively at 45°, 59° 50′, 67° 30′, 90°, to the axis; the plates of each pair having their principal planes at 180° to one another. The first of these shows, in the region about the line joining the centres of the ring systems, a series of ellipses for the secondary figures; the second shows straight lines; the third oblique hyperbolas; the fourth rectangular hyperbolas. With a view, however, of exhibiting not only these the more important, but also all intermediate phases of the phenomena, I have prepared two curved sections of quartz, each forming nearly a quadrant, and cut at one end perpendicular, at the other nearly parallel to the axis. By placing these end to end, with the axes at opposite ends, and sliding one over the other, all the phases of the secondary figures can be shown in succession. And if the point midway between the axes be kept in the centre of the field of view, the figures will be symmetrical; otherwise unsymmetrical. Beginning with the ends, which are perpendicular to the axis, together in the field of view, and causing the quadrants to slide at the same rate in opposite directions, the secondary circles will be seen elongating into ellipses, the ellipses stretching out until they pass into parallel straight lines; and lastly, these lines contracting towards the centre of the field and diverging towards the sides, until they form hyperbolas, the obliquity of which gradually diminishes as we approach that part of the section which is parallel to the axis.

The case of two quartz plates cut at an angle of 67° 30′ to the axis, whereof one is free to revolve in front of the other, gives rise to some interesting transformations of the secondary figures. When the principal planes of the crystals are coincident, the field generally shows rings double in number to those due to one plate. But towards the side away from which the axes are directed the rays are more nearly parallel, while towards the opposite side the rays are more nearly perpendicular to the axis. Hence, towards the first side, the rings will approximate more nearly to circles, and towards the other they will show indications of becoming branches of hyperbolas. As the principal planes are turned round in opposite directions from their initial position, the secondary figures begin to appear. At 45° discontinuous bands with hyperbolic curvature towards the part of the field most distant from the ring centres are seen. At 90° these bands become continuous and sharply defined, while towards the ring-centres a portion of the ellipses may be observed entering the field. Beyond 90° the hyperbolic branches leave the field, and the rectilinear part of the bands is replaced by the diaper-pattern described in a former part of this lecture; while the ellipses are gradually elongated to parallel straight lines, and then are converted into hyperbolas. At 180° the hyperbolas occupy the centre of the field.

Many similar experiments may be made with biaxal crystals; but it would exceed our present limits to describe them here. I will, therefore, confine myself to a repetition with monochromatic light of the well-known experiment of showing the passage of mica from its proper biaxal form to an apparently uniaxal form, by crossing a number of films of the crystal.

It has perhaps been abundantly shown on more than one occasion in this theatre, by the spectra of polarised light, that colour is in fact a shadow; and that the varied tints, produced by crystals in light under this condition, are due to selective shadows thrown as it were over the various components of a colourless beam. And the present method of monochromatic light affords a striking illustration of the fact that suppression of light is a factor of all chromatic effects. But beside this, the method affords an opportunity of tracing, by a continuous process, the transformations of the results due to a continuous change of crystalline circumstance. The experiments supply a fresh instance of the fact that nature does nothing *per saltum;* and they may perhaps be regarded as adding one more link, however insignificant, to that great chain of continuity which is gradually binding more closely together the diversified phenomena of the material universe.

Lastly, they remind us that the most beautiful, the most delicate, the most instructive features of nature are dependent, not so much on abundance of material, on profusion of ornamentation, or variety of display, as upon simplicity of character, on fidelity to truth, and on strict but willing obedience to law.

[W. S.]

GENERAL MONTHLY MEETING,

Monday, May 6, 1878.

Sir W. **Frederick Pollock**, Bart. M.A. Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:

The Lord Lindsay,
Sir W. Frederick Pollock, Bart.
C. William Siemens, Esq. D.C.L. F.R.S.
George Busk, Esq. F.R.S. Treasurer.
William Spottiswoode, Esq. LL.D. Tr.R.S. Secretary.

Philip Boyd, Esq.
Frank Crisp, Esq. LL.B. B.A. F.L.S. Hon. Sec. R. Mic. Soc.
James McClelland, Esq. J.P. F.S.S.
Colin Mackenzie, Esq.
Emile R. Merton, Esq.

were *elected* Members of the Royal Institution.

John Tyndall, Esq. D.C.L. LL.D. F.R.S.
was re-elected Professor of Natural Philosophy.

The Secretary announced that the Managers had granted the use of the Lecture-Theatre to the **Sanitary Institute of Great Britain** for their Anniversary Meeting on July 3 at 3 o'clock, when an Address would be given by Mr. **Frank Buckland**, M.A. on "The Pollution of Rivers, and its Effects upon the Fisheries and the Supply of Water to Towns and Villages."

The Special Thanks of the Members were returned to **Charles Hawksley**, Esq. for a donation of Five Guineas for the Promotion of Scientific Researches.

The Chairman announced that the Fullerian Professorship of Physiology became vacant on the 5th of April last; and that the Managers would proceed to the election of a Professor on the 4th of November next.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, *viz.*:—

FROM

Academia dei Lincei, Rome—Atti, Serie III. Transunti, Vol. II. Fasc. 1, 3, 4. 4to. 1878.

Actuaries, Institute of—Journal, No. 110. 8vo. 1878.
Antiquaries, Society of—Proceedings, Second Series, Vol. VII. No. 3. 8vo. 1878.
Astronomical Society, Royal—Monthly Notices, Vol. XXXVIII. Nos. 5, 6. 8vo. 1878.
Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1877, Heft 3. 8vo. 1877.
British Architects, Royal Institute of—Sessional Papers, 1878. Nos. 10, 11, 12. 4to.
Chemical Society—Journal, 1877: Supplementary No. and for April, 1878. 8vo.
Chilian Minister, The—Nitrate and Guano Deposits in the Desert of Atacama. 8vo. 1878.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LI. 8vo. 1878.
Cornwall Polytechnic Society, Royal—Forty-fifth Annual Report. 8vo. 1877.
Coutts, John, Esq. (the Author)—The Philosophy of Man and Creation (the Microcosm and the Macrocosm) as manifested by Revelation and Science. 16to. 1878.
Dawson, Dr. J. W. F.R.S. (the Author)—Notes on some Scottish Devonian Plants. (K 102) 8vo. 1877.
Editors—American Journal of Science for April, 1878. 8vo.
Analyst for April, 1878. 8vo.
Athenæum for April, 1878. 4to.
Chemical News for April, 1878. 4to.
Engineer for April, 1878. fol.
Horological Journal for April, 1878. 8vo.
Iron for April, 1878. 4to.
Journal for Applied Science for April, 1878. fol.
Nature for April, 1878. 4to.
Nautical Magazine for April, 1878. 8vo.
Telegraphic Journal for April, 1878. 8vo.
Franklin Institute—Journal, No. 628. 8vo. 1877.
Geographical Society, Royal—Proceedings, Vol. XXII. No. 2. 8vo. 1878.
Geological Survey of India—Records, Vol. XI. Part 1. 8vo. 1878.
Iron and Steel Institute—Journal, 1877, No. 2. 8vo. 1878.
Mechanical Engineers' Institution—Proceedings, Jan. 1878. 8vo.
Medical and Chirurgical Society, Royal—Proceedings, Part 46. 8vo. 1878.
Meteorological Society—Quarterly Journal, No. 25. 8vo. 1878
Pharmaceutical Society of Great Britain—Calendar for 1878. 8vo.
Journal for April, 1878. 8vo.
Photographic Society—Journal, New Series, Vol. II. No. 6. 8vo. 1877.
Preussische Akademie der Wissenschaften—Monatsberichte: Jan. 1878. 8vo.
Rossetti, Sig. F. (the Author)—Relazione su Esperienze Telefoniche. 8vo. 1878.
Sulla Temperature del Sole. 4to. 1878.
Royal Society of London—Proceedings, No. 186. 8vo. 1878.
Russian Physical Central Observatory—Annalen: Jahrgang, 1876. 4to. 1877.
St. Petersburg, Académie des Sciences—Mémoires. 7e Série. Tome XXIV. Nos. 4-11; Tome XXV. Nos. 1-4. 4to. 1877.
Tuson, Professor R. V. (the Editor)—Cooley's Cyclopædia of Practical Receipts. Sixth edition, Part 2. 8vo. 1878.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, 1878. Hefte 2, 3. 4to.

WEEKLY EVENING MEETING,

Friday, May 10, 1878.

C. William Siemens, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Sir William Thomson, LL.D. F.R.S.

The Effects of Stress on the Magnetization of Iron, Cobalt, and Nickel.

Though, as discovered by Faraday, every substance has a susceptibility for inductive magnetization, the three metals, iron, nickel, cobalt, stand out so prominently from among the other known chemical elements, that they only are commonly regarded as *the* magnetic metals, and the magnetism of all other substances is so feeble as to be comparatively almost imperceptible.

The magnetization of each of the three magnetic metals is greatly affected by mechanical stress. From the beginnings of magnetic science it must have been known that the magnetism of iron and steel is disturbed, sometimes lost or much diminished, by blows, striking the metal with a hammer, or letting it fall on hard ground. Gilbert, nearly three hundred years ago, showed that bars of soft iron held in the direction of the dipping needle and struck violently by a hammer acquire much more magnetism, and again more reverse magnetism when inverted in that line, than when placed in those positions gently without shock of any kind. An ordinary fireside poker shows these effects splendidly, with no other apparatus than a little pocket-compass or a sewing needle, magnetized and slung horizontally, hanging by a fine silk thread. If habitually the poker rests upright the upper end will be found a true north pole, the lower a true south when first tested by the needle. Holding the poker with exceeding gentleness, invert it :—The end that was down, though now up, is still a true south pole, and repels the north end (or true south pole) of the movable suspended needle. A gentle tap with the hand on the poker now produces a surprising result. Instantly it yields to the terrestrial influence, and its upper end, becoming a true north pole, attracts the northern end of the suspended needle. Even more surprising is the slightness of the agitation which suffices to shake the retained magnetism of a former position out of a soft iron wire, and let it take the magnetization due to the position in which it is held. A superstitious person would say ;—that is animal magnetism !—when he sees an iron wire becoming a notably effective magnet when held vertically and rubbed gently from end to end between finger and thumb.

Changes of magnetism produced by mechanical agitation are shown to a much greater degree in thin bars than in thick ones; and when the diameter exceeds a quarter of the length they are hardly sensible. Hence when the "Flinders bar" is applied to compensate the error produced in a ship's compass by change of magnetic latitude, its length ought not to be more than six or seven times its diameter; and for the same reason long iron rods or stanchions in the neighbourhood of a compass are very detrimental to its trustworthiness. Half a hundredweight of iron in the shape of rails or awning stanchions, too often to be found very near a compass, are more dangerous than tons or hundreds of tons in the shape of heavy steam steering gear, or of armoured turrets in an ironclad.

A piece of iron left in the Royal Institution by Faraday, with a label in his own handwriting to the effect that it had been between three hundred and four hundred years fixed in a vertical position in the stonework of the Oxford Cathedral, having been given to him by Dr. Buckland in May, 1835, was exhibited and tested. It was found to have its upper end a true north pole. It was inverted before the audience, and instantly that end became a true south pole and the other a true north pole. Thus nearly four hundred years in one position had done nothing to *fix* the magnetism. In its inverted position it was hammered violently on each end by a wooden mallet: this increased the magnetism somewhat, but did not *fix* it. The bar was inverted again, and then, in its first position, its original upper end, now up again, became again a true north pole.

The stoutness of the bar (that is to say, the greatness of the proportions of its breadth and thickness to its length) were such, that if of modern iron, it probably would not have behaved as it did; but probably also it may have been superior in "softness" to the ordinary run of modern bar iron.

Bars of nickel and cobalt, unique and splendid specimens, for which the speaker was indebted to the celebrated metallurgical chemist, Mr. Wharton, of Philadelphia, were exhibited, and found to show effects of concussion quite as do bars of iron of different qualities.

An altogether new effect of stress was discovered about ten years ago by Villari, according to which longitudinal pull augments the temporary induced magnetism of soft iron bars or wires when the magnetizing force is less than a certain critical value; and diminishes it when the magnetizing force exceeds that value; and augments the residual magnetism when the magnetizing force, whether it has been great or small, has been removed.

The speaker had measured approximately the Villari critical value, and found it to be about twenty-four times the vertical component of the terrestrial magnetic force (or about 10 C.G.S. units). The maximum effect in the way of augmentation by pull he had found with about six times the Glasgow vertical force. He had found for bars of nickel and cobalt opposite effects to those of Villari for soft iron, and had found a maximum value, with a certain degree of magnetizing force, and evidence making it probable that a critical magnetizing force would be found for each of these metals also, such that the magnetization would be *increased* by pull when the magnetizing force exceeds it.

The speaker had found corresponding effects of *transverse pull* in soft iron, and had found them to be correspondingly opposite to those discovered by Villari for longitudinal pull. The transverse pull was produced by water pressure in the interior of a gun-barrel applied by a piston and lever at one end. Thus a pressure of about 1000 lbs. per square inch, applied and removed at pleasure, gave effects on the magnetism induced in the vertical gun-barrel by the vertical component of the terrestrial magnetic force, and, again, by an electric current through a coil of insulated copper wire round the gun-barrel, which were witnessed by the audience. When the force magnetizing the gun-barrel was anything less than about sixty times the Glasgow value of the vertical component of the terrestrial force, the magnetization was found to be less with the pressure on than off. When the magnetizing force exceeded that critical value, the magnetization was *greater* with the pressure on than off. The residual (retained) magnetism was always less with the pressure on than off (after ten or a dozen "*ons*" and "offs" of the pressure to shake out as much of the magnetization as was so loosely held as to be shaken out by this agitation).

It is remarkable that the critical amount of the magnetizing force in respect to effect of transverse pull is more than double that of the Villari effect of longitudinal pull. Thus for intermediate amounts of force (say forces between 10 and 25 C.G.S. units), both longitudinal pull and transverse pull diminish the induced magnetization. Hence it is to be inferred that equal pull in all directions would diminish, and equal positive pressure in all directions would increase, the magnetization under the influence of force between these critical values, and through some range above and below them; and not improbably for all amounts, however large or small, of the magnetizing force; but further experiment is necessary to answer this question.

A most interesting further inquiry in connection with this subject is to find if æolotropic stress (pressure unequal in different directions), beyond the limits of elasticity, leaves in iron, nickel, or cobalt a permanent æolotropic difference of magnetic susceptibility in different directions analogous to that discovered thirty years ago by Tyndall in the diamagnetic quality of soft, imperfectly elastic material, such as fresh bread. Special difficulties prevented the speaker from obtaining any results thirty years ago, when he tried to discover corresponding effects in iron; but the investigation is not hopeless, and he intends to resume it.

[W. T.]

WEEKLY EVENING MEETING,

Friday, May 17, 1878.

Sir W. Frederick Pollock, Bart. M.A. Vice-President,
in the Chair.

A. Graham Bell, Esq.

Speech.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, May 24, 1878.

Sir W. Frederick Pollock, Bart. M.A. Vice-President,
in the Chair.

A. C. Ramsay, LL.D. F.R.S.

DIRECTOR-GENERAL OF THE GEOLOGICAL SURVEY OF THE UNITED KINGDOM.

The Geology of Gibraltar and the Opposite Coast of Africa; and the History of the Mediterranean Sea.

The Rock of Gibraltar is about 2½ miles in length, by 1550 yards in breadth near the north front, and 550 yards at Europa Flats at the southern end of the fortress. The north side of the Rock forms a noble cliff, close to which is the Rock gun battery on a peak 1349 feet above the sea. Farther south, close to O'Hara's Tower, the highest part of the Rock attains an elevation of 1370 feet.

The strata which form the principal part of the Rock consist of compact grey limestone, which from the north front to O'Hara's Tower and the western edge of Windmill Hill Flats dip westerly at very high angles, and at Windmill Hill are absolutely vertical. No fossils have been found in these beds, except a few brachiopods of the genus Rhynchonella of an unnamed species, but which somewhat resembles *Rh. concinna* of the British Oolites, and therefore it has been commonly considered that the limestone of the Rock belongs to the Oolitic or Jurassic series of continental geologists. All along the western part of the peninsula, from Grand Casemate Square to the New Mole Parade and the Hospital Road, the strata consists of dark shales and thin bands of calcareous grits, which overlie the limestone and may be well seen standing on edge in the ditch outside the batteries between the Old Mole and Careening Bay. No fossils have been found in these strata; but their perfect conformity to the limestone beds indicates that they belong to the same geological series. Without going into details I may mention, that between the New Mole, the Devil's Bellows, and the neighbourhood of the Advance

Battery a fault crosses the Rock from north-west to south-east, and south of that line on the Windmill Hill Flats and Europa Flats the disturbance of the rocks has been so extreme that the strata have a *reverse dip* and the shales underlie the limestones at angles of from 40° to 50°.*

The neighbouring coasts of Spain seem to belong to the same series of strata as those which overlie the limestone, excepting a patch of late Tertiary beds (Crag?), which lie close to the coast at and near Campo. The rocks of the opposite coast of Africa between Ceuta and Cape Spartel resemble those of the Spanish coast, and the rugged and woody height of the opposite Pillar of Hercules, Gebel Abyla, is recognized as being formed of limestone of the same age as the Rock of Gibraltar.

I have said that the limestone of Gibraltar is overlaid by shales, which, however, are in places concealed by a remarkable brecciated conglomerate, or *agglomerate*, as it is styled by Mr. James Geikie in our joint memoir on the 'Geology of Gibraltar,' now in course of publication. It is well seen at Buena Vista and on the ground between the Mount and Rosia Bay, and consists of angular fragments of limestone of all sizes, up to three or four yards in diameter, agglutinated together into a compact mass, which very rarely shows any traces of stratification. Its origin seems to be as follows.

It is well known that during the Glacial epoch the range of the Sierra Nevada maintained an independent set of glaciers, and one on a very small scale even now remains in one of the higher and most shady recesses of the mountains. In Africa the range of the Atlas also had its glaciers on a large scale; but it is impossible to believe that a small isolated boss of rock like Gibraltar could have given birth to a glacier, even though the whole had been heaved much higher into the air than now.

It is clear, however, that when glaciers occupied the valleys of the Sierra Nevada and the range of the Atlas in North Africa, the climate of Spain must have been more severe than at present, and the land must have suffered from winter snows and frosts now almost unknown at Gibraltar. It is therefore surmised that, when the Rock with its innumerable joints was subject to heavy snowfalls and alternating frosts and thaws, angular stones, small and large, were split from and wedged out from the parent mass of limestone and lodged on the steep slopes of the hill. These, when thaws became powerful and rain also fell, being saturated with the half-melted slush of snow, would year by year be carried confusedly down the slopes even to the very foot of the Rock, beyond which in places they now pass out beneath the sea.

During this episode in the history of the Rock, there is no proof that it was inhabited by Mammalia.

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* The lecture was illustrated by a map and sections showing all these points and much of what follows.

After a time the climate ameliorated, probably during one of those inter-glacial episodes which have been proved by analyses of the ancient glaciers of the Alps and Britain. Simultaneously with this change of climate the Rock and adjoining parts of Spain were united to Africa by the gradual upheaval of the whole territory, probably of not less than 1000 feet above the present level. The soundings between Cape Plata in Spain, and Cape Spartel in Africa, show that this upheaval would be sufficient to connect the opposite continents. There is nothing extreme in this supposition, for similar oscillations of level at the same epoch of geological history can be proved in many other regions, as, for example, by sea-shell beds in North Wales and Lancashire from 1200 to 1300 feet above the sea, and by similar phenomena in Norway, Greenland, and the far-north islands of the Arctic Sea.

When the junction of the continents was complete, a migration of Mammalia took place into the area of which Gibraltar forms part. These, for the most part, were not forms such as inhabit the north of Europe, but African species, the remains of which are found in fissures and bone-caves in the limestone rock, and in similar cavities in the limestone breccia. Such cavities and fissures are found in almost all limestones, and were, and still are, being produced by the solvent action of carbonic acid in rain-water percolating through cracks and joints. A list of those found in the Genista Cave has been printed in the 'Journal of the Geological Society' (1865), by Mr. Busk and the late Dr. Falconer, who visited the rock for the purpose of investigating the phenomena. It is unnecessary to repeat the list here, but among other species there occurs *Rhinoceros etruscus* and *R. leptorhinus*, a horse, the common pig, and *Sus priscus*, oxen, deer, *Ibex œgocerös*, leopards, the African lynx, and *Hyœna crocuta*, now living in Africa.* In connection with this subject, it is important to mention that many years ago Mr. Smith of Jordanhill, found a tooth of *Elephas antiquus* in an old beach at the south end of Europa Flats. In like manner, on the coast of Africa, close to Tangier, while examining a sea-cliff, formed of coralline sands and gravels, with Mr. J. Geikie and some friends from Gibraltar, we found a molar tooth of the same species sticking in the jaw. It is important to notice that *E. antiquus* is so closely allied to the living African elephant that it is considered by some persons to have been its ancestor, just as the *E primigeneus*, or mammoth, is by others surmised to have been the ancestor of the living Indian elephant.

By and by there began a slow intermittent depression of the area, the result of which was the formation, by marine erosion, of the limestone platforms of Windmill Hill and Europa Flats, the first from 370 to 390 feet, and the latter from 90 to 120 feet above the present level of the sea. Both are undoubtedly true *plains of marine denudation*, and similar smaller terraces of erosion occur elsewhere in the limestone and the limestone breccia. This gradual sinking of the land went on till the Rock sank at least 700 feet; for on the more precipitous east side, sands with sea-shells occur high on the promontory, and also what appear to be numerous minute fragments of Echini. At this time Gibraltar was a little rocky islet, miles from any continuous shore; and it is obvious that, under such circumstances, it could have been inhabited by no large, and only, possibly, by a few very small mammalia.

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* For the latest details of the Mammalia found in the Genista Cave, see 'Transactions of the Zoological Society,' 1877, by George Busk, F.R.S. &c.

A re-elevation of the Rock and adjacent sea-bottom followed the temporary depression, and Gibraltar may have been re-united to Africa, in which case a second migration of Mammalia may then have taken place from that continent into Europe. A later formation of limestone breccia also took place; for, on a minor scale, it is found overlying the sands on the east side of the Rock.

The important question now arises, What effect had these various oscillations of the relative levels of the land on the history of the Mediterranean Sea, and what in earlier times was its origin?

Let anyone consider Asia and Europe as a whole, and he will find that the Black Sea, the Sea of Marmora, and the Mediterranean lie in a western continuation of the great area of inland drainage of Central Asia, which, from the confines of Europe on the west shore of the Caspian Sea, extends eastward into Asia for a distance of about 3000 miles. In this immense area, the Caspian, the Sea of Aral, Lake Balkasch, and all the minor lakes are salt, with the exception of those lakes from whence rivers flow into other lakes, in which case the lakes having outlets are fresh, and those into which they discharge their waters are salt, the latter having no outflowing rivers by which to discharge their surplus waters. The reason of this is obvious; for all rivers hold salts in solution, generally insensible to the taste, but appreciable to the analyses of the chemist; and, as lakes which do not discharge their water by rivers only get rid of it by means of solar evaporation, the result is, as in the well-known cases of the Dead Sea and the Salt Lake of Utah, that the salts by degrees get concentrated, sometimes almost to the point of complete saturation.

The surface of the Caspian Sea is 83·6 feet below the level of the Black Sea, and its extreme depth I do not know, except that it is more than 2000 and less than 5000 feet where deepest. The Black Sea, which flows through the Bosphorus into the Sea of Marmora, is at least 6000 feet deep where deepest, while the Sea of Marmora, which discharges its water into the Mediterranean through the Dardanelles, is 3360 feet in depth. Beyond this lies the Mediterranean, more than 1700 geographical miles in length, which when critically examined with relation to its soundings, resolves itself into three distinct basins of great depth. The first, between the coast of Syria and Sicily, is about 1040 geographical miles long, the whole being more than 5000 feet deep, while its profoundest depths range from 9600 and 10,980 feet between Africa and Asia Minor, and

13,020 feet half-way across the sea between the Gulf of Sidra and the mouth of the Adriatic. The next basin, lying south of Sicily, is about 170 geographical miles in length, the whole being over 2000 feet deep, while its greatest depth as given by Admiral Spratt between Malta and Pantellaria is 4200 feet.* The western basin where more than 5000 feet deep is about 830 miles in length. Where deepest near the African coast opposite the mouth of the river Kebir the depth is 9162 feet, between Marseilles and Minorca 9258, and between Majorca and Algiers 9342 feet.

Taking a sinuous line of soundings between Cape Plata in Spain and Cape Spartel in Africa as we open into the Atlantic, the average depth, as given in the Admiralty chart, is from 600 to 780 feet for at least three-quarters of the way, while the greatest depth is 996 feet.

If we consider the relation of these three basins and their soundings to the closing of the Mediterranean Sea by upheaval into land of the above-named opening, it becomes obvious that had the upheaval been sufficient also to uplift the comparatively shallow area between Sicily and Tunis, what is now one long inland sea must then have presented the spectacle of three salt lakes, comparable in their nature to the Caspian, the Black Sea, and the Sea of Marmora, the whole being then as now a mere western prolongation of the area of inland drainage of Central Asia.

The eastern lake between the coast of Syria and Sicily must have covered an area much more than twice as large as the Caspian Sea, and south of the present coast of Sicily lay the second basin, which, as shown by Admiral Spratt, both in form and size somewhat resembled the Sea of Marmora. Like that sea or salt lake, it also communicated with the more eastern lake by a narrow gorge 1740 feet deep where shallowest, and comparable to the Bosphorus, through which the water of the Black Sea flows as a rapid salt river into the Sea of Marmora, while at its north-west end, it communicated by a long, narrow valley or channel with the great western basin that lies between Italy and the Straits of Gibraltar. This channel, which is 1272 feet deep where shallowest, is aptly compared by Admiral Spratt to the salt stream of the Dardanelles, for they are much the same in length and breadth, and in each case the soundings rapidly deepen outside their ends.

It thus appears that at a certain period of its history, the Mediterranean area of depression was occupied by three great lakes which communicated with each other by narrow river-like channels, and it may be that, though temporarily closed, a similar gorge for a time connected the western lake with the Atlantic.

Under these circumstances it is hard to say whether the waters of the Mediterranean area were salter or fresher than they are now. That they were not fresh we may well believe; but if a river current ran from the Mediterranean into the Atlantic it may be that, like the

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* See Admiral Spratt "On the Maltese Bone-caves," 'Quarterly Journal of the Geological Society,' 1867.

Black Sea now, these inland Mediterranean lakes were gradually freshening, unless solar evaporation of these broad land-locked waters helped to keep them salt, or even to make them salter than the Atlantic Ocean, which indeed the Mediterranean water is now, in spite of the influx of such great rivers as the Don, the Dnieper, the Danube, the Nile, the Po, the Rhone, and the Ebro. The freshening effect of these is more than neutralized by the mighty river-like rush of Atlantic water which flows through the Straits of Gibraltar, and is due to the immense amount of evaporation that is constantly going on from the surface of the great continental sea. These things combined have tended to make the present Mediterranean somewhat more salt than the Atlantic, for the latter contains 2·673 of common salt by weight in 100 parts of water according to Bischoff, while according to William Ramsay the Mediterranean contains 2·946 grains, and the proportion of pure water in the Atlantic to all the substances in solution is greater by ·243 than in the Mediterranean.

Such is a brief account of the past history and of the present state of the Mediterranean area, but something remains to be said respecting the origin of the great basin in which its waters lie.

When we examine a geological map of Europe and the north coast of Africa, one important feature is, that the Mediterranean area on the north side of the sea consists to a great extent of Miocene and Pliocene strata, and the same is the case with the Sea of Marmora, the Black Sea, the Crimea, the Sea of Azof, and part of the west coast of the Caspian. Where Pliocene beds form the coasts they often merely conceal Miocene strata that underlie them. In like manner the Balearic Islands, Corsica and Sardinia, Gozo and Malta, contain Miocene strata, Malta and Gozo being entirely formed of these, as shown in the map by Admiral Spratt and Lord Ducie. Candia and Greece are partly formed of the same kind of rocks. In like manner the north of Africa at and near the sea between Tunis and Tangier largely consists of Miocene strata, forming part of the mountain district south of Oran and Algiers, and extending to the flanks of the farther Atlas.

Considering that on the mainland of the Mediterranean region the Miocene strata are fragmentary, and also that the same is the case in the islands, it is, perhaps, not too much to assume that a very great part of that area was once occupied by Miocene rocks, which, before the Mediterranean came into existence, formed a long and broad land, of which Malta and parts of other Mediterranean islands are fragments. By and by a gradual sinking of this vast area began, probably simultaneously with that of the subsidence of the Asiatic area of inland drainage (first described by Pallas), of which, as already stated, the Mediterranean area is a western prolongation.

It is now a widely accepted canon in geology, that when on a great scale one part of the surface of the earth is depressed, other portions often more or less parallel to the area of depression are upheaved, and these general effects being due to shrinkage of the earth's crust, mountain chains have been formed at various periods throughout all geological time. It is for this reason that the strata of all great mountain chains are contorted, the beds, once horizontal, being by lateral pressure forced into a smaller space than they originally occupied when flat. I must add that all mountain chains of which I have any knowledge, personal or acquired by reading, are the result of several such shrinkages and upheavals.

It is a fact well known to geologists, that after the close of the Eocene or Lower Tertiary epoch, the Pyrenees, and the Alps and Carpathian mountains underwent one of those last and greatest upheavals, which then raised them into mountain chains of the first European magnitude. At the same period the Apennines, and the mountains east of the Adriatic and Ionian seas were elevated, together with the Caucasus and other mountain tracts, needless to name in this abstract. Till this elevation took place, what we now call Europe had but little resemblance to our modern continent, and it was after the elevation of these mountain chains that the Miocene strata of Europe were deposited in seas and at intervals in lakes that washed the bases of the mountains.

The Miocene epoch of our European area was a period of repose, excepting the occurrence here and there of ordinary volcanic phenomena, accompanied by minor oscillations of the level of land in relation to the sea, as, for example, in Switzerland, where the Miocene strata consist of alternations of marine and lacustrine strata. But at the close of this epoch, or rather what brought it to an end in the area under review, a renewed upheaval took place of the Alps, the Pyrenees, the Caucasus, and other mountain ranges already named. So important was this event, that the thick flat-lying consolidated Miocene rocks that flanked the bases of the older Alps, were with these mountains heaved thousands of feet above their former level. Most Swiss tourists know the minor mountains on the north side of the Alps, one of these being the Righi, the summit of which is 5919 feet high; and in older times these hills must have been much higher, considering the great denudations they have suffered during long geological ages.

The same kind of minor Miocene hills adhere to the Pyrenees and the Caucasus, and also in other regions already named, including the Atlas south of the Mediterranean; and indeed they are equally applicable to the Himalayan range, which, after a prodigious upheaval in post-Eocene times, underwent, like the Alps, a post-Miocene elevation of great amount, as witnessed by the Sewalik hills on the southern flanks of the mountains.

The cause, or rather the complement, of these last important elevations seems to me to have been the gradual sinking of the great area of inland drainage, by which, according to Pallas, an old Asiatic Mediterranean was formed, the approximate limits of which from the Black and Caspian seas eastward, have in later days been insisted on by Sir Roderick Murchison, in his work on 'Russia and the Ural Mountains.' In like manner, in my opinion, the gradual sinking of the Mediterranean region by degrees produced a vast inland sea, united to part at least of that of Asia. Even the European and African part of that sea was for a time much more extensive than the present Mediterranean, as witnessed not only by the fossil mollusca of the Sahara, which are of Mediterranean species, but also by other marine Pliocene strata high above the level of the sea on the southern coasts of Europe. One important result of this great subsidence was to produce those deep basins, which subsequently passed through the various physical phases described in this lecture.

To sum up the subject:—

1. What is now the Mediterranean area, in old times consisted of a wide land surface chiefly formed of Miocene strata.

2. The Alps, the Pyrenees, the Atlas, and other mountain chains, underwent renewed upheaval, accompanied by contemporaneous depression of the Mediterranean area. This closed our Miocene epoch.

3. The greatest extent of the Mediterranean Sea was in the Pliocene epoch.

4. Partial elevation of the mouth of the Mediterranean, and probably of the ground between Sicily and Africa, permitted the migration of African mammals into Europe at a time or times when the inner parts of that sea formed three great and deep salt lakes, in what then was and still is a western continuation of the great Asiatic area of inland drainage.

In conclusion, let me remark, that in the study of physical geography it is by no means sufficient merely to indicate outside forms, such as the positions, heights, and other obvious characters of mountain ranges, the extent of table-lands and plains, and of lakes and rivers, great and small. The mere physiography of an area is not enough to know. It is not enough to say that things are thus and thus. Why are they so? is a question that must always recur to the philosophic mind. Geology lies at the base of almost all modern researches in Physical Geography, and by means of geological data we can alone struggle to explain the origin and nature of the complicated events that produced every feature, large and small, of every part of the outside world. Till this in the far future is accomplished, though we may have able sketches, the actual details of the physical geography of the world cannot be perfectly understood.

WEEKLY EVENING MEETING,

Friday, May 31, 1878.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

William Henry Flower, F.R.S.

HUNTERIAN PROFESSOR OF COMPARATIVE ANATOMY AND CONSERVATOR OF THE MUSEUM OF THE ROYAL COLLEGE OF SURGEONS OF ENGLAND.

The Native Races of the Pacific Ocean.

The region of the world treated of in the present lecture is that known to geographers as Oceania, or Oceanica, consisting of a vast number of islands of various sizes, from Australia downwards, scattered throughout the great ocean tract bounded east and west by the continents of America and Asia.

The inhabitants of these islands offer many advantages for the commencement of a study of physical anthropology, and for an exposition of the principles of the science. We shall find that they present great diversity; some of the widest contrasts to be met with in the human species are to be found among them. We shall also find in this area some of the lowest existing types of mankind, affording material for studying the most extreme deviation known from the highest race, as exemplified in the European. Lastly, the comparative isolation in which the greater number of these islands have remained for countless generations, lying hidden in their ocean solitudes, far away from the track of commerce and civilization, has caused their inhabitants to develop and retain distinctive characteristics more sharply defined than those of other regions of the world, where constant intercommunication has resulted in infinite and intricate blendings of primitive races, and but partial and imperfect evolutions of new ones.

The people of these islands will be treated of here mainly from an anatomical point of view, and in great measure from observations made upon such portions of their bodily frame (chiefly crania) as are preserved in the collection under my charge. But it must be observed *in limine*, that this collection, as with all others yet formed, large as it may appear to the uninitiated in the difficulties of craniology, is wholly insufficient for the purpose of constructing a classification of mankind, founded on physical structure. It can only afford certain indications, valuable as far as they go, from which a provisional or approximative system may be built up. Very many, indeed the majority, of the islands are totally unrepresented in it; others are illustrated by only one or two individuals. Far larger collections, and far more systematic and minute observations, than have yet been made, are required before the natural history of man in this region can be worked out in any detail. The results obtained at present are however sufficient to encourage us to persevere, and to vindicate the study of anatomical characters, especially those of the skull, as a basis for a natural classification, from the disrespect into which it has fallen, on account of the failure of tentative systems of craniology, founded on far too imperfect materials, and too imperfect use of those materials.

I will begin by speaking of the great continental island, as it may be called, of Australia, which when discovered by Europeans was inhabited throughout by a race, distinct in the totality of its characters from any known to exist in any other part of the world. It will be convenient to consider this race first, partly because the materials at my disposal for its investigation are more abundant and more complete than in the case of any other of the races of the Oceanic area, and partly because a comparison of its characters with those of the best known race (that to which we ourselves belong, and which is commonly taken as the standard in works on human anatomy) will afford a good idea both of the kind and the degree of variation to be met with between one of the lowest and one of the highest groups of mankind, and we shall at the same time be able to appreciate whether, and if so, to what extent, any approximation is made by the former towards any still lower types of animal organization.

Although the northern coast of Australia had previously been seen by Spanish and Portuguese navigators (as by Torres in 1606), the first rencontre of any European with the native inhabitants appears to have been that of Abel Tasman, the celebrated Dutch seaman, who in 1644 was sent out by Van Dieman, governor of the possessions of Holland in the East Indies, on his second voyage of discovery. The part which he visited (and to which he gave the name of New Holland) was the north-west coast, and he describes the natives as naked, black, and curly-haired.

The earliest description of the aborigines of Australia by any Englishman is that of Dampier, who in his first adventurous voyage round the world stayed on the north-west coast, not very far from the spot visited by Tasman, from January 4th to March 12th, 1688. He has left us a tolerably full account of the inhabitants, which, although presenting some discrepancies from those of more modern travellers, is valuable, and probably on the whole trustworthy. "The inhabitants of the country," he says,* "are the miserablest people in the world. The *Hodmadods* [Hottentots] of *Monomatapa*, though a nasty people, yet for wealth are gentlemen to these; who have no houses and skin garments, sheep, poultry, and fruits of the earth, ostrich eggs, &c., as the *Hodmadods* have. And setting aside their humane shape, they differ but little from brutes. They are tall, strait-bodied, and thin, with small, long limbs. They have great heads, round foreheads, and great brows. They have great bottle noses, pretty full lips, and wide mouths. The two fore-teeth of their upper-jaw are wanting in all of them, men and women, old and young; whether they draw them out I know not. Neither have they any beards. They are long-vizaged, and of a very unpleasing aspect, having no one graceful feature in their faces. Their hair is black, short, and curl'd, like that of the Negroes; and not long and lank, like the common *Indians*. The colour of their skins, both of their faces and the rest of their body, is black, like that of the Negroes of *Guinea*. They have no sort of cloaths, but a piece of the rind of a tree ty'd like a girdle about their waists, and a handful of long grass, or three or four small green boughs full of leaves, thrust under their girdle to cover their nakedness. They have no houses, but lie in the open air, without any covering; the earth being their bed, and the heavens their canopy. Their only food is a small sort of fish, which they get by making wares of stone across little coves, or branches of the sea. In other places they seek for cockles, mussels, periwincles," &c. He describes their weapons as wooden swords and lances. "The sword is a piece of wood shaped somewhat like a cutlass," probably a boomerang. It is satisfactory to note that the relations of Dampier and his companions with the natives, both on this and his second visit to Australia at Shark's Bay, eleven years later, were perfectly amicable, or at all events unattended by any serious disagreement or casualty on either side, a sudden and vigorous beating of the drum being sufficient to scare them away on one occasion when they had become troublesome and even threatening.

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* 'A New Voyage round the World,' by Captain William Dampier. Sixth edition, 1717, vol. i. p. 464.

The next visit of an Englishman to Australia was one which led to far more memorable consequences. It was that of Captain Cook, who, on his first voyage round the world, after sailing westward from New Zealand, reached the coast of "New South Wales," as he named it, near Cape Howe, on the 19th of April, 1770, and sailing northwards, explored the whole east coast to Cape York in Torres Straits. His first sight of the natives, on April 27th, is described at page 489, and his first landing, on the following day, at page 492 of the great navigator's deeply interesting narrative. Read by the light of subsequent events, the gallant though unsuccessful defence of their native land by two naked savages against a boat's crew of forty armed men must excite our sympathy. Certainly no more critical event has ever occurred in the history of any nation, nor combat ever fought attended with such momentous consequences, to one at least of the races engaged, as that which took place in Botany Bay on April 28th,

1770. On that day the fate of the Australian race, which had been for untold ages in undisturbed possession of their native soil, was sealed. Cook's discovery led to the settlement of the country by the English. The settlement of the country by the English means the inevitable annihilation of the aboriginal race.

Cook afterwards saw more of the natives of the northern part of Australia (now Queensland) during his enforced stay in "Endeavour Bay," and has left us a detailed account of their physical characters, condition, and customs,* which, as in the case of all other descriptions given by the illustrious navigator, subsequent observation has fully corroborated, and which will be incorporated with what I shall have to say on these subjects presently.

The whole of the habitable part of the great land tract, 2400 miles from east to west, and nearly 2000 from north to south, when first explored by Europeans was found to be occupied, though very sparsely, by people having a remarkable general similarity in physical characters, language, and customs, though whether they are all to be considered as belonging to one race, or whether, as some suppose, they result from the blending of two originally distinct races, it is not easy upon the present evidence to decide. The latter theory certainly has the merit of reconciling the discrepancies between the accounts of different observers. It will be reverted to in speaking of their physical characters.

The geographical position of the country has isolated them in a remarkable manner, probably for long ages, from all the rest of the world; except for a little infusion of Papuan and Malay influence on the north coast, all the civilization they possess is undoubtedly their own. This isolation must be taken into account, in considering their social condition, as an index of their real elevation in the scale of humanity; for as with individuals so with nations, those naturally of inferior endowments may, by the influence of educated and civilized neighbours, appear superior to others who have not had these advantages. Although, as will be shown hereafter, the anatomical structure of the Australian, as compared with that of the European, shows signs of degradation, yet, in some respects, he is not below, but, perhaps, rather above, some African negroes, who may greatly surpass him in knowledge of the arts, in social customs, and other conditions, by which relative superiority of race is usually tested. But then the Africans have been living from time immemorial in contact with more highly educated races.

However this may be, it is certainly true that there is nowhere existing at present any large group of human beings, the inhabitants in fact of a whole continent, so totally removed from what we call civilization, as were the Australians when first discovered; as the following short summary of their condition will show:—

Of clothing the majority had none, being like those described by

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* Op cit., p. 631, et seq.

Cook, absolutely naked, both sexes alike. Some, however, wore a girdle or band of bark, leaves, or skin, and others a short cloak, made of kangaroo skin, thrown over the shoulders. Yet they were not destitute of the idea of personal adornment, daubs of red or yellow ochre mixed with grease on the body, a stick or a bone stuck through the septum of the nose, and feathers in the hair were indispensable to the full dress at least of the men. They had no fixed dwelling-places, but moved about from spot to spot as inclination or necessity compelled, erecting temporary shelters of the most primitive and unstable character, of boughs of trees, or pieces of bark. Some (as those described by Dampier) appear to have made no habitations of any kind. Their bark or log canoes were of the roughest and most simple construction, though in this, as in some other respects, an improvement is observed near Cape York, doubtless under Papuan influence. They had no bows and arrows, their arms being spears, lances, and shields, and two remarkable and ingenious weapons peculiar, or nearly so, to themselves, and almost universally distributed throughout the country, the boomerang or the "wummera," or throwing stick. With the aid of the latter in propelling their lances, "at fifty yards," Captain Cook says, "they were more sure of their mark than we were with a single bullet." They had no metals and no kind of pottery for domestic use. The only vessels that they had for holding water were curved pieces of bark, or, in some districts, the skulls of their deceased relatives. They knew, however, how to manufacture knives of flint and shells, axes of stone, and cord and nets out of native grass. Their cookery was of the rudest kind, as they had no vessels in which they could boil water. Their food consisted of the flesh of kangaroos and other marsupial and rodent mammals, fish, molluscs, crabs, snakes, lizards, wild seeds, roots, and fruits. They made no sort of attempt at cultivation of the ground, and possessed no domestic animal, except the half wild dingo, or native dog. Cannibalism, though occasional, was not so universal a custom as with many other races higher in the social scale, as the New Zealanders and Fiji islanders.

They were divided into numerous small tribes, each composed of a varying number of individuals (from fifteen to three hundred or more), which were constantly at war with each other. They acknowledged no hereditary or formally elected chiefs, but had several curious and complicated social customs, of which those relating to the initiation into manhood, and others designed to prevent the intermarrying of near relations, are the best known. They possessed nothing resembling writing, but native drawings have been discovered, which (as in the case of many others of the least elevated of mankind) show some power of representing graphically the forms of men and animals. Though every tribe spoke its own dialect, all the languages of the continent are said to have possessed closely affined common characters.

It will be seen from this summary that the Australian of the present day is on an immeasurably lower level of civilization than the

Britons were, even as far back as the neolithic period, ages before the invasion of Cæsar, as there is fairly good evidence that the country "was then inhabited by a tolerably large population, divided into tribes, and living principally on their flocks and herds, acquainted with agriculture, and supplementing their food by hunting and fishing. They were acquainted with the arts of spinning and making pottery, and with mining, and exchanged their commodities by barter. They were possessed of boats, in which they could make voyages from France to Britain or from Britain to Ireland. They reverenced their dead by erecting tombs, and they worshipped the Great Unknown in those rude temples which astonish us on the lonely moor, or the swelling chalk down, or within reach of the sound of the waves on the sea-shore." *

I do not propose to enter into the question of the moral and intellectual character of these or of the other people of whom I shall have to speak, as there is no subject upon which it is so difficult to obtain satisfactory evidence or to draw just conclusions. It is hard enough to do so with people about whom we have ample means of judging, but to attempt it with savages, whose language is imperfectly understood, and whose ideas and notions are most difficult to appreciate, would lead me far beyond the scope of the subject I have undertaken; so I will pass at once to the physical characters of the race.

Although there are many general traits common to all Australians, yet, as indicated above, it is by no means certain that they are such a homogeneous people as has been often supposed. Topinard, who has made a careful summary of the descriptions of various travellers,† thinks that he can distinguish two races, which, either pure or mixed in various proportions, constitute the various tribes now, or recently, inhabiting the continent.

1. The finer race, taller and lighter coloured (chocolate or coppery), with straight or wavy hair, inhabiting the elevated plains of the whole of the interior, and reaching the coast at Queensland, and to the north. These are the people described by Cook at Endeavour Bay.

2. The lower race, negroid, black, and small, with woolly hair, and more prognathous. They are met with chiefly on the coast at various parts, as on the north-west (Dampier), King George's Sound, and the neighbourhood of Sydney. These, Topinard considers the primitive inhabitants of the land; they are now becoming extinct, by absorption into the other, the invading race, and by the encroachments of the latter and of European settlers. He thinks, moreover, that he can distinguish two types of Australian crania; but these have not been associated hitherto with the other characters, as unfortunately the larger number of osteological specimens in our Museums have

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* 'Edinburgh Review,' April 1878, No. 302, p. 448.

† 'Etude sur les Races Indigènes de l'Australie.' Paris, 1872.

no indication of the tribe, or of the external appearance of the individual, to which they belonged.

Under these circumstances, it is necessary at present to treat them all as belonging to one race, trusting to future and more careful observations to discriminate between the different branches into which it may have become divided, or perhaps the different roots from which it may have sprung.

With regard to the stature of the Australians, we have really no very precise information; travellers almost always trusting to somewhat vague impressions, instead of actual measurements. Stanbridge, however, gives the average of men of Victoria as 5 feet 5½ inches. As facts contributing to a knowledge of this subject, I may mention that the height of four adult male skeletons now in England are respectively, 1. (Middlesex Hospital) 5 feet; 2. (Cambridge University) 5 feet 4 inches; 3. (Cambridge University) 5 feet; 4. (Barnard Davis collection) 5 feet 1 inch; and four in the Blumenbach collection at Gottingen, are according to Dr. J. W. Spengel, respectively 5 feet 8 inches, 5 feet 5 inches, 5 feet 3 inches, and 5 feet 3 inches, giving an average of 5 feet 3 inches for the eight males. Three female skeletons, two in the museum of the Royal College of Surgeons, and one at Haslar Hospital, are respectively 5 feet 2 inches, 4 feet 11 inches, and 4 feet 11 inches. These numbers are of course quite insufficient to give the true average of the race, but I think that we may infer from them, that the general height is somewhat less than that of Englishmen, whose average, as ascertained by the very careful observations of Dr. Beddoe, is not very far from 5 feet 6½ inches.*

The colour of the skin presents various shades of darkness, never really black, but more usually of a dark brown or chocolate colour. The hair is always black, though often artificially discoloured by lime or ochre. It is greatly developed upon the scalp, face, breast, shoulders, and arms; the men being nearly always full-bearded. The hair on the head has neither the stiff, lank character of the Mongolian and American races, nor (unless in exceptional cases, as those described by Dampier, indicating mixture of other races) the frizzly or "woolly" character of the negro or Melanesian; but is fine, silky, and slightly curled or wavy. When allowed to grow long, it commonly hangs on the head in tangled, shaggy masses. As in general appearance, so in microscopic section, it is intermediate between the two extreme forms mentioned above, having neither the nearly cylindrical form of the lank-haired races, nor the flattening of the frizzly-haired groups of men. It is, in fact, very similar in size and form to that of many Europeans.

The figure of the Australian is variously described by different travellers; but the limbs, especially the legs, are generally said to be slender, and the head appears disproportionately large. In fact, as will be shown presently, though the cerebral cavity is small, the outline of the bony framework of the head is large and long, and the overhanging brows, profusion of shaggy hair, and luxuriant beard, heighten the effect of size.

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* "On the Stature and Bulk of Man in the British Isles." 'Mem. Anthrop. Soc. Lond.' vol. iii. 1870.

Judging from descriptions, and numerous photographs which I have seen, there is a type of countenance common to, and very distinctive of, all Australian natives. In the album of the Anthropological Institute, there are excellent photographs of upwards of fifty natives of both sexes and different ages, which present a remarkable family likeness. It is true that, though from various tribes, these are all from one district, near Melbourne; but others from New South Wales, and from North Australia, show the same common characters, and the general resemblance of the facial portion of the skulls indicates a general prevalence of similar and strongly marked features. These are as follows:—The head is narrow and long behind the ears. The eyes (which are said to be bright and sparkling) are sunk beneath very heavy and prominent brows. The nose is short, not prominent, but very wide, its width at the lower end equalling its height, and being about one-third of the whole width of the face. The upper part of the dorsum is deeply sunk under the projecting forehead, there being no prominence of the "bridge"; the dorsum seen in profile is straight or slightly rounded. The apex is thick and round, the nostrils dilated, their plane directed downwards, outwards, and forwards. The mouth is very wide; the lips thick and projecting, though by no means to the extent observed in most African negroes. The degree of prognathism varies, as will be seen when speaking of the cranium. The chin is usually small.

No part of the organization offers such definite characters for description, analysis, and comparison as the skeleton. The bones are nearly imperishable, readily preserved, and easily examined and measured. Of all parts of the skeleton the cranium is the most valuable, as it gives the means of estimating the volume and form of the brain; and the facial characters, by which the races of mankind are so strongly differentiated, have their outlines clearly indicated in its bony framework.

The Museum of the Royal College of Surgeons contains a fine series of crania of native Australians, fifty-four in number—a far larger series than is contained in any one collection elsewhere. Of these, twelve are from North Australia, Cape York, and Port Essington; one from Queensland; six from New South Wales; nineteen from South Australia; four from West Australia; and the remainder from unknown localities. As I have not been able to find any constant characters by which the skulls from different regions of the continent can be distinguished, I have taken them all together in the following summary of their characteristics, irrespective of locality. Five of the skulls have not arrived at full maturity, having the basal suture still open (being, therefore, below the age of twenty), and are consequently rejected in the average of measurements. Of the remaining forty-nine, twenty-six appear to belong to males and nineteen to females; the remaining four being doubtful. The sexual characters are generally very well marked. Of pathological deformities sufficiently marked to interfere with the normal characters of the cranium, or of variations of form caused by premature synostosis of the sutures, there are none; and not one of the series (or any other Australian which I have examined) shows any signs of having been artificially deformed during infancy. These skulls show that the practice of knocking out some of the front teeth on initiation into manhood is not so frequent as some writers on the customs of the Australians would lead us to believe. In only one case, both central upper incisors have been lost, and the right only in five cases, the left in one, though this latter may have been due to natural decay. Among fifteen Australian skulls in the Army Medical Museum, at Netley, three have lost the right central incisor, none the left.

In order to appreciate the distinctive characters of the Australian crania more fully, I have compared them with a corresponding number of Europeans, and have taken Italians as the only nation of which a sufficient number exist in the Museum to obtain a fair average of both sexes. This is owing to the College having a few years ago purchased the valuable collection of ancient and modern Italian and Greek crania, formed by Professor Nicolucci. From these I have selected forty male and twenty female crania from various parts of Italy, taken at hazard from the modern collection, regard only having been paid to their being adult and of no abnormal form. These were probably all from people of the least cultivated classes, and whose average height would not differ greatly from that of the Australian.

In general size, as estimated by the principal external diameters of the cranium, there is a wonderful similarity between the two races. The average horizontal circumference * of the Australians of both sexes is exactly 19·7 inches, while that of the Italians is 19·8 inches.† Though the average length, height, and breadth of each differ individually, these three mean dimensions added together come to exactly the same in both races, as the following table shows:—

	Australian.	Italian.
Average length	7·2	6·9
„ breadth	5·1	5·5
„ height	5·1	5·0
	17·4 inches	17·4 inches.

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* This is measured by a tape passed round the skull, just above the glabella, and over the most prominent part of the occiput—the line Op O in Fig. 1.

† The average for both sexes, where the number of skulls of each has not been equal, is obtained by adding together the average procured for each sex separately, and dividing the result by two; otherwise a preponderating number either of males or females in the series would have a disturbing effect upon the general average.

Yet the capacity of the interior of the cranial cavity is very different, the average of the Australians of both sexes being 74·7 cubic inches, or 1224 cubic centimetres, and that of the Italians 83·4 cubic inches, or 1367 cubic centimetres, giving an advantage of nearly 9 inches to the latter. This difference is accounted for partly by the greater thickness of the Australian cranium, but chiefly by its angularity, the upper lateral parietes being flattened and sloping from the median line above like the roof of a house, instead of having the round, dome-like form of the European cranium; being, in fact, as Professor Cleland expresses it, "ill-filled."

The average of the twenty-six Australian male skulls is 78·4 inches, or 1285 cubic centimetres; that of nineteen females, 69·7 inches or 1142 cubic centimetres. The highest male is 88·5 inches (1450 cubic centimetres), or less than the average male Italian (89·1 inches = 1460 cubic centimetres). The smallest of the Australian series (a female) is 62·9 inches, or 1030 cubic centimetres. The greatest care was used to ensure accuracy in the measurements, which were taken by the method to which, after many thousand trials, I have now given the preference—i. e. filling the skull with mustard seed and estimating the quantity by means of Busk's choremometer,* certain precautions being adopted which it would take too long to describe here. It is perfectly clear, then, that in cranial capacity, which is the most accurate way of estimating volume of brain, the Australian savage is very inferior to the Italian peasant.

The general form of the cranium seen from above (the *norma verticalis* of Blumenbach), is ordinarily estimated by stating the proportion which its greatest breadth in the parietal region (Fig. 3, P P) bears to its extreme length from before backwards (O Op), the latter being reckoned as 100. This gives the index of breadth, or latitudinal index, or, as it is often called, the cranial index.† According to the most convenient system of nomenclature, when the index is below 75 the skull is called *dolichocephalic*, or long-headed; when between 75 and 80, *mesocephalic;* when 80 or above, *brachycephalic*, or short-headed. The average length of the male Australian crania of this collection is 7·31 inches, the length of the female being 7·00. This diameter is taken from the *ophryon* of Broca (Fig. 1, Op) to the most distant part of the occiput (O), and does not include the projection of the glabella (G). The average breadth of the male is 5·18, of the female 5·06. The indices are more interesting than the absolute diameters, as they give an idea of the form of the skull. The average latitudinal index of the twenty-six male skulls is 71·2, of the nineteen females, 72·3; so that as a race, the Australians are strongly dolichocephalic. On analyzing the indices of the forty-nine skulls separately, I find that forty-five of them range between 68 and 74, which may thus be called the normal limits of variation. One is exceptionally low, viz. 67, making forty-six out of the forty-nine truly dolichocephalic. Three come into the category of mesocephaly, one having an index of 75, one of 76, and one the altogether exceptional index of 78. Of the genuineness of this last, I have, however, some doubts, as it presents some other aberrant characters. Excluding this, not one approaches the borders of brachycephaly.

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* G. Busk, "Note on a Ready Method of Measuring the Cubic Capacity of Skulls," 'Journ. Anthrop. Institute,' vol. iii. p. 200.

† Obtained thus: $\frac{\text{breadth} \times 100}{\text{length}} = \text{index}$.

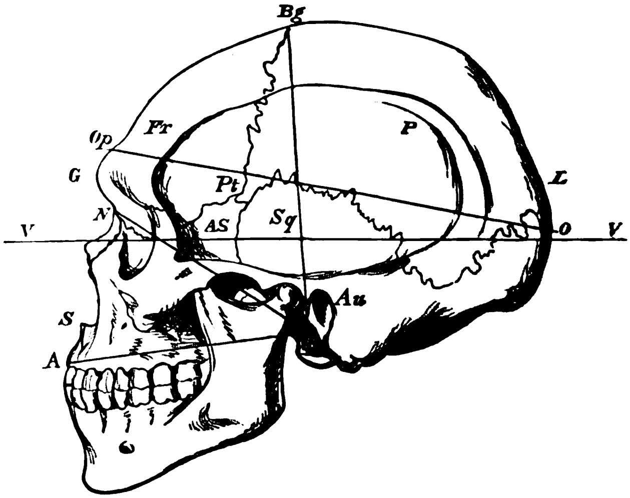

Fig. 1.—Side view of skull of male Australian.* V V. Horizontal line, corresponding with visual axis. A. Alveolar point. S. Spinal point, or base of nasal spine. N. Nasion, or centre of fronto-nasal suture. G. Glabella. Op. Ophryon, or centre of supra-orbital line. Bg. Bregma, or union of coronal and sagittal sutures. L. Lambda, or union of lambdoid and sagittal sutures. O. Occipital point. Au. Auricular point, or centre of external auditory meatus. B. Basion, or centre of anterior margin of foramen magnum. Pt. Ptereon, or point where the frontal (Fr), parietal (P), squamosal (Sq), and ali-sphenoid (A S) bones meet.

It is interesting to find that other collections of Australian crania give closely similar results. Thus I found the average latitudinal index of ten male Australian skulls in the Army Medical Museum at Netley, to be 72. Broca gives 71·93 as the average of seventeen of both sexes at Paris, and Dr. Barnard Davis 72 as the average of twenty-three in his collection. From all these various data, there can be no doubt that it is a perfectly well-established fact, that the average cranial index of the skull of the Australians is 72, or slightly less, and that they are, therefore, to be placed among the most dolichocephalic of races.

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* The figures are all from specimens in the Museum of the Royal College of Surgeons of England. They are drawn geometrically by means of Broca's stereograph, and reduced one-third.

The Italians show in this respect far greater variation than the Australians, the extremes ranging between 71 and 91, the average of both sexes being as high as 80, or just within the compass of brachycephaly.

The height of cranium is estimated in various ways by different anthropologists, but the most convenient is the distance from the basion or anterior margin of foramen magnum (B, Fig. 1), to the bregma or junction of the coronal and sagittal sutures (Bg, Fig. 1). This dimension in the Australian skulls of both sexes in the College Museum is practically equal to the breadth. In the males it rather exceeds the latter, but in the females falls short of it. The average altitudinal index (ratio of height to length, the latter being 100) in the male is 72·0, in the female 71·1. The range of variation is greater than that of the breadth, being from 63 to 80. The average altitudinal index of ten skulls of males at Netley is 74, or rather higher than the College series—a circumstance probably due to the latter containing a number of skulls belonging to a peculiar tribe from the neighbourhood of Adelaide, of exceptionally depressed form, of which there are no representatives in the Netley collection.

Every Australian cranium yet examined, of either sex, is what Busk calls *phænozygous*—that is to say, in the *norma verticalis*, when held at arm's length and looked at with one eye, both zygomatic arches are seen at the same time. Of the Italian skulls, out of twenty females, only one is phænozygous, and that very slightly; out of forty males, eighteen present this condition. This depends upon the comparative development of the cranial parietes (Fig. 2, P P) and the zygomatic arch (Z Z), or cerebral *versus* muscular development. In the Australian crania it rather indicates narrowness of brain cavity, than any great size of the zygomata, for as a general rule the various ridges and processes for the attachment of muscles are not very strongly marked. The mastoid processes are not large, and the inion and occipital curved lines are moderate, the former in no case exceeding No. 3 of Broca's scale,* usually but No. 1 or 2 in the males.

On the other hand, every skull, without exception, male or female, has a prominent glabella. In the males it equals No. 3 or No. 4 of Broca's scale, rarely as low as 2; in the females 2 or 3. Even in the children's skulls this character begins to show itself.

The sutures of the cranium are generally less complex than in European skulls, and Wormian bones in the lambdoid suture are less frequent and more simple in character. Metopism,† or persistence of the frontal suture into adult age (see Fig. 4), does not occur in a single instance; whereas out of the sixty Italian skulls, as many as ten are metopic, which nearly agrees with the statement of Broca, that in European skulls this feature occurs in one out of every seven. The condition of the sutures at the region of the skull, called the "ptereon" by Broca (the anterior part of the temporal fossa, near the great wing of the sphenoid bone, Fig. 1, P), is of some interest as a race-character. These conditions may be classified thus:—1. The squamosal actually coming in contact with the frontal (*Ptéreon retourné*, Broca). 2. The squamosal coming near the frontal (less than half a centimetre) "stenocrotaphitic" crania. 3. An "epipteric" bone, or small separate ossification developed at the upper end of the great wing of the sphenoid. 4. Neither of the above conditions present, but a simple spheno-parietal suture of more than half a centimetre in length, as in the skulls usually considered normal among Europeans (*ptéreon en H.* Broca). Comparing both sides of the whole number of Australians and of Italians examined, the relative frequency of the different conditions in the two races, reduced to terms of 100, is as follows:—

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* "Instructions Craniologiques et Craniométriques," 'Mem. de la Soc. d'Anthrop. de Paris.' T. ii. 2nd ser. 1875.

† One of the many convenient terms introduced by Broca into craniology See "Notions complémentaires sur l'Osteologie du Crane," 'Bull. de la Société d'Anthrop. de Paris,' 20 Mai, 1875; and "Instructions Craniologiques et Craniométriques," 'Mem. de la Soc. d'Anthrop.' Tom. ii. 2nd ser. 1875.

	No. 1.	No. 2.	No. 3.	No. 4.	Total.
Australians	9·1	37·3	14·5	39·1	100
Italians	2·5	14·2	10·8	72·5	100

The form of the skeleton of the face is, as has been mentioned before, extremely characteristic of race, though it has not usually received as much attention as it merits. The facial angle, as it is called, or angle formed by the profile of the face with a horizontal line, has certainly been very much talked about since the time when Camper first drew attention to its interest as a distinguishing character of higher or lower races. Many modifications of Camper's angle have been proposed, both as to the horizontal and the vertical line, and many methods of measurement have been adopted, none, however, so commodious as Broca's "median goniometer." Measured by this instrument, the angle having its apex at the "alveolar point" (anterior and inferior point of premaxillæ in the median line, Fig. 1, A) and one limb passing through the centre of the external auditory meatus (Au), and the other through the "ophryon," or centre of forehead, immediately above the glabella (Op), which may be called the ophryo-alveolo-auricular angle, averages in 42 adult Australian crania of both sexes, 64·5°, or 63·9° for the males and 64·8° for the females; in 60 Italians the average of both sexes is 68·0°, or 67·9° for the males and 68·2° for the females. The size of this angle, it will be observed, depends upon several distinct conditions of the skull, which are not directly related to each other; the chief of which are—(1) the prominence of the forehead, (2) the projection forwards of the upper jaw, and (3) the length of the face from above downwards. The difference of the angle in the two races is chiefly due to the second, for the Australian forehead, though considerably narrower than the European, is very nearly, if not quite, as prominent; the average distance between the basion and centre of the frontal bone (the frontal radius) being almost exactly the same in both races. This is not surprising considering, as mentioned before, the actual height of the skull at the upper part of the frontal bone (bregma) measured from the basion is greater in the Australian than the Italian.

The prognathism or projection forward of the jaws is most readily estimated by comparing the distance from the basion to the nasion (naso-frontal suture), or *the basi-nasal length* (Fig. 1, B N), with that from the basion to the alveolar point, the *basi-alveolar length* (B A), both most easily measured with the sliding callipers. When the latter dimension considerably exceeds the former, the face is said to be *prognathous;* when the reverse is the case, it is *orthognathous;* when the two dimensions are equal or thereabouts, it is *mesognathous.*

The exact degree of gnathic projection is expressed by an index formed by the relation of the basi-alveolar (B A) to the basi-nasal length (B N), the latter, as the more fixed, being taken as 100. If the index is between 98 and 102, the face may be considered mesognathous; if below 98, orthognathous; if above 102, prognathous.

The Australians taken altogether come into the prognathous category, the general average of the indices of the adult skulls of both sexes, in which the face is preserved, being 104; that of the females alone being nearly 105, and that of the males 103. There is considerable individual variation. In seven cases out of the forty-two, B A is equal to B N, and in five it is actually less. Among the sixty Italian crania measured for comparison, there is also much individual difference, some few being prognathous, and six having the two dimensions equal; but taking the general average, orthognathism prevails, the mean index being 97. In a very well-formed English skull, the gnathic index is as low as 92.

The height of the face of the Australians is less than in the Italians; the length from the nasion to the alveolar point (N A) averages in the males of the former, 67 millimetres, in the males of the latter, 70 millimetres.

The nasal bones of the Australians are extremely characteristic, being remarkable among all races for their shortness, and for the depression of their upper part, which is sunk beneath the overhanging glabella. The importance of the general form of the nasal aperture as a race-character, was pointed out by the late Dr. Williamson, in his catalogue of the crania in the Army Medical Museum; but the readiest mode in which this may be estimated and expressed is one of Broca's numerous and valuable contributions to craniology.* His "nasal index," or ratio between the greatest width of the nasal aperture (Fig. 2, *n n*) and its height—including the nasal bones, and measured therefore from the nasion or centre of the fronto-nasal suture (N) to the lower border of the aperture or base of the nasal spine (S)—is one of the most useful of all the cranial indices in distinguishing races, and is of great assistance in forming an idea of the characteristic physiognomy of the individuals composing them. The most usual height of the nasal aperture is about 100 millimetres, the width about 50 millimetres; the index, consequently, 50. This may be taken as a general mean of all races, and thus individuals or races in which the index varies only slightly on each side of this figure (between 48 and 52) are called by Broca, *mesorhine*. Those in which the index is lower than 48, are *leptorhine*, or narrow-nosed; those in which the index is 53 or higher, are *platyrhine*, or broad-nosed. The Australians come decidedly under the latter category, the general average index of 41 crania of both sexes being 56·5.* Out of the whole number, 34 are platyrhine (more than half have an index between 54 and 58, the highest index being 69), 7 are mesorhine, and these nearly all on the platyrhine side, in one only does the index fall below 50, and not one in leptorhine. Indeed, a leptorhine Australian cranium would be as great a phenomenon as a brachycephalic one, and would require strong proof of its authenticity.† The females, on the whole, are rather more broadnosed than the males, their average being 57·6, that of the males being 55·8. The average nasal index of ten male Australian crania in the Army Medical Museum at Netley, is 54·8.

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* "Recherches sur l'Indice Nasal," 'Revue d'Anthropologie.' Tome i. 1872.

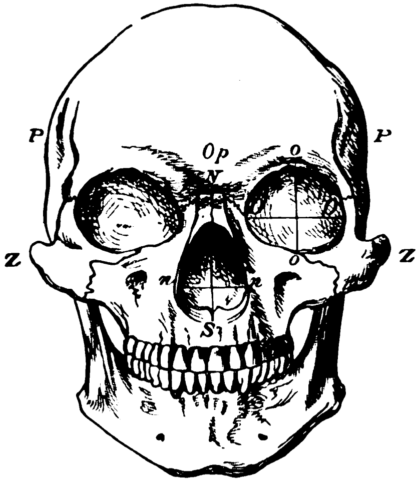

Fig. 2.—Front view of skull of Australian. P P. Parietal eminences. Z Z. Zygomatic arches. Op. Ophryon. N. Nasion. S. Spinal point. *n n.* Width of nasal aperture. O O. Width of orbit. *o o.* Height of orbit.

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* This, as with the other general averages, is not the mean of the individual indices, but what is more accurate, the index of the means of the dimensions i. e. $\frac{\text{mean width} \times 100}{\text{mean height}}$.

† There is, however, one in the collection of the Anthropological Institute, which appears to be genuine. The index is only 46.

In the sixty Italian skulls, the average nasal index is 47, there being little difference between the sexes; they are, therefore, as a race, leptorhine, though approaching the confines of the intermediate category. There is, however, very considerable variation among them, as many as 8 being platyrhine, 18 mesorhine, and 34 leptorhine; the highest being 58·3, the lowest 39·6.

The general character of the face depends much on the form of the orbit. In the Australians this is elongated and rectangular rather than round, and with the upper and lower border nearly parallel. The orbital index of Broca, or relation of the height (Fig. 2, *o o*) to the width (O O) of the anterior margin, measured according to the method for which I must refer to his memoir,[*] gives a good idea of its general shape. A low orbital index shows a wide and depressed orbit, generally overshadowed by a heavy superciliary ridge; a high index shows a round, open orbit. There is much individual variation in this character, as in all others derived from the cranium, in every race; but the averages often give useful differentiating characters. As with the other indices, it is convenient to group them into three—the high (*megaseme*), intermediate (*mesoseme*), and low (*microseme*), the limits of which are set by Broca at 89 and 83 respectively; 86 being taken as the general average of all races. The orbital index of the average male Australian of this collection is 81·8, of the female 82·9, or of both sexes together 82·3; so that taken all together, they are microseme. With regard to the variations, 15 out of the 42 are mesoseme, and the highest index among the males is 88·1, so that not one of this sex is megaseme. Two females, however, enter this category, having respectively indices of 89·7 and 92·1. This quite accords with the fact pointed out by Broca, that as a general rule the orbital index of the female is greater than that of the male; indeed, these Australians are in this respect exceptionally equal. The mean orbital index of the ten male Australians at Netley accords remarkably with that of those in the College of Surgeons' collection, being exactly 82·0. The Italians have a higher orbital index, and therefore rounder orbits than the Australians, the mean index being 86·0 for the males and 90·9 for the females.

The malar bones are remarkably small and weak in the Australians, the lower border especially is very little developed. They also slope away from the median line of the face, and the outer margin of the orbit, as best seen in the profile view of the face, is placed considerably behind the inner margin, offering the greatest contrast in this respect to the Mongolian type, which reaches its greatest development in the Eskimo. The malar bones of the European are deeper and stronger than the Australian, though they also slope backwards from the middle line.

The nasal spine is never large, usually No. 2 of Broca's scale, or often No. 1, and in two cases it is obsolete, as in the apes. The lower margin of the nasal opening is not sharply defined, as in the European; but the floor of the narial chamber passes gradually into the anterior or external surface of the alveolar process of the maxilla.

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[*] 'Recherches sur l'Indice Orbitaire.' Paris, 1876. Also 'Instructions C niologiques et Craniométriques.'

The palate, though varying in different individuals, is often of a form very rarely seen among Europeans, i. e. long and narrow, with lateral margins nearly parallel, and the anterior margin straight (hypsiloid). It has very seldom the even semicircular form (parabolic) seen in many other races.

Though the mandible or lower jaw varies in form in different individuals, when a considerable series is examined and compared with a corresponding series of Europeans, it will be seen that in the majority of the Australians the symphysis is shorter from above downwards, the mentum or chin more retreating, the horizontal ramus longer and lower, with its upper and lower margins more nearly parallel, the ascending ramus not so high, and broader from before backwards, and the coronoid process less developed. In all these characters, as in many of those of the cranium mentioned above, especially the relative smallness of the cranial cavity, the smallness of the nasal bones, the form of the lower margin of the nasal aperture, and the prognathism, the Australian presents some approximation towards the anthropoid ape.

The teeth of the Australian differ considerably, as has often been pointed out, from those of the European, and indeed from most other races, in their superior size, and in the more complete development of the cusps of the molars. In order to estimate with precision the difference in size, I have obtained the following average measurements from examples of both races expressed in millimetres; but as the teeth are lost in many of the skulls in collections, the numbers examined in both cases are not quite so abundant as might be wished:—

	Male European.	Male Australian.	Female Australian.
Width of upper canine	7·50	8·54	8·33
Length of three upper molars ..	41·53	46·67	46·00
Length of three lower molars ..	45·85	51·43	49·67
Breadth of second upper molar	11·05	12·67	12·21

The third molars, or wisdom teeth, are more constant, earlier in appearance, and better developed, both as to crown and root, than in the European. There are very few instances in which these teeth are very small and single-rooted among the Australians, and fewer still in which they are absent.

The teeth generally, as with all savages, are remarkably free from decay, though as life advances they wear down from the attrition occasioned by gritty particles in their food. But it is very rare to find skulls, even the oldest, in which any considerable number of teeth have been lost during life.

For an examination of the characters of the remainder of the skeleton, the materials at my disposal are, unfortunately, very insufficient. The attention of collectors has hitherto been concentrated too much on the skulls, and the preservation of complete skeletons, certainly a matter of greater difficulty, has been much neglected.

The bone, or group of bones, which next to the cranium is most likely to afford good differential characters for races, is the pelvis. The very striking difference between this part in all the apes and in man, would readily lead to the supposition that some difference might be found in it between the higher and lower races of the latter, and it is therefore natural that attention should be directed to the subject.

The most marked difference between the pelvis of man and that of the apes is expressed numerically in the "pelvic index," or relation between the antero-posterior to the transverse diameter of the brim, the latter being taken as 100. In various anthropoids this index ranges between 122 (orang) and 160 (chimpanzee) in the males, and somewhat less in the females. In the European males the average in sixty-three measured by Verneau,* was 80, which nearly corresponds with an average of eleven measured by myself, viz. 81. I have been able to measure eight male Australian pelves, and find the average index is as high as 99·5, the numbers in the different individuals being respectively 108, 105, 102, 100, 98, 98, 95, and 90; the various Europeans ranging between 96 and 71. The pelvis of the negro has been shown by Vrolik and others to possess the same peculiarity of form. There are other characters of the pelvis, and also of the form and relative proportions of the bones of the limbs by which the Australian appears to differ from the average European; but I will pass them by for the present, as the number of individuals examined is really not sufficient to draw general conclusions from with safety, merely indicating that as far as they go, they appear to show that the Australian resembles the negro and differs from the European in the relative superior length of the second compared with the proximal segment of both limbs, or in other words, the radius and tibia, as compared with the humerus and femur, are relatively longer in the black races.

I must now bring to a conclusion this brief summary of the physical characters of the Australian aboriginal people, for, beyond an imperfect knowledge of their osteology, we have at present no information as to their anatomical structure. The past history of this race, absolutely unknown from documents or traditions of any historic value, is a most interesting subject for speculation. Whether they have, as some suppose, fallen from a higher state of civilization and structure, and have, by whatever cause, degenerated into their present condition, or whether they represent a phase in the history of mankind generally, both social and physical, once universal, now confined to the Australian continent, and thus offer a parallel to so many of the phenomena connected with the fauna and flora of that remarkable land—a land of living fossils, as it may be called—is at present a question which fails to be answered for want of sufficient data. It may be stated, as a simple matter of fact, and with only such weight allowed to it as to other negative evidence of the kind, that hitherto no remains of any race, presenting the characters of the Australian savage, or indicating so great a departure from the normal modern European standard, have been discovered in any European land. Even for a parallel condition of culture, we must go back to very early prehistoric times.

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* 'Le Bassin dans les Sexes et dans les Races.' Paris, 1875.

Whatever the past history of the race may have been, its future is no matter of speculation. On April 28, 1770, the day when Cook first landed on the Australian shore, its fate was determined, and that fate, whether for good or for evil, in the great and complex succession of events which combine to make up the history of the world and shape the future destiny of mankind, is *extinction*. The causes and methods of this extinction will be best illustrated by the story of a kindred and neighbouring people, with whom the event is already an accomplished fact.

To the south of the southern extremity of Australia, and separated from it by an interval of about 150 miles, lies the large island now called Tasmania, having an area equal to about three-fourths that of Ireland. It was discovered in 1642 by Abel Jansen Tasman, on his first voyage of exploration in the South Seas, and named by him Van Dieman's Land, after the governor of the Dutch East Indian possessions. It was then, and indeed until 1798, when its insularity was proved by Bass and Flinders, thought to be part of the mainland of New Holland or Australia. Tasman anchored in the bay, which he named "Frederick Henry," and though he inferred from various signs that the land was inhabited, he did not see any of the natives during his short stay. In 1772, the French navigator Marion du Fresne arrived with two vessels at the same spot visited by Tasman, and there, on the 4th of March, the first meeting of the aborigines with Europeans took place. The former came with confidence down to the French boats, bringing their wives and children with them, but in consequence of a misunderstanding a conflict took place, in which one of the natives was shot and the rest fled.

The first Englishman who approached the shores of Tasmania was Captain Furneaux, of the 'Resolution,' who in March, 1773, having been accidentally separated from the ship of his commander, Captain Cook, coasted along the south and east shores of the island, but bad weather preventing him from landing, he saw none of the people, though he says the country "appeared to be thickly inhabited, as there was a continual fire along shore as we sailed."

On the 26th of January, 1777, Captain Cook, on his third voyage, entered Adventure Bay, Bruni Island, and then the intercourse between the English and the Tasmanians, so fatal to the latter, commenced. Cook thus describes them: "They were quite naked, and wore no ornaments, unless we consider as such, and as a proof of their love of finery, some large punctures or ridges raised on different parts of their bodies, some in straight and others in curved lines. They were of the common stature, but rather slender. Their skin was black, and also their hair, which was as woolly as that of any native of Guinea; but they were not distinguished by remarkably thick lips or flat noses. On the contrary, their features were far from being disagreeable. They had pretty good eyes; and their teeth were tolerably even, but very dirty. Most of them had their hair and beards smeared with a red ointment; and some had their faces also painted with the same composition." [*]

The next European visitors to Tasmania were the French Admirals D'Entrecasteux and Baudin, in 1792 and 1802; the latter being accompanied, as naturalist, by Peron, who has given us a full description, accompanied by the first published drawings of the people. These, however, are little better than caricatures.

In 1803, Van Dieman's Land was taken possession of by the English, and colonized by expeditions from New South Wales at two points, Port Dalrymple on the north, and Restdown, afterwards Risdon, on the Derwent, near the future Hobart Town. The latter settlement was formed by a military party and convict labourers, and here took place, in May, 1804, the first serious conflict between the natives and European invaders. A party of several hundred blacks—men women, and children—engaged, as it subsequently appeared, in a kangaroo chase, were suddenly seen running down the side of a hill towards the infant colony. The alarmed settlers, thinking they were about to be attacked by a strong force, without any parley, fired volleys among the harmless and unhappy natives, killing, it is said, as many as fifty before the rest could make their escape. After this, of course it was long before amicable relations could be re-established. In fact the "Black War," thus begun, ended only with the departure of the last natives from the island in 1835.[†] The usual difficulties which attend the colonization of a country already inhabited by a different race from the new comers, were aggravated in the case of Tasmania by the fact that a considerable proportion of the latter consisted of convicts of the most hardened and degraded type, who, frequently escaping from the European settlements, took to a roving and lawless life in the forests as bushrangers, or on the islands in the straits as sealers. From these men, utterly selfish, brutal, and cruel, the natives in most cases first received those impressions of European civilization and character which all the endeavours of the more humane colonists and of the Government could never eradicate; and the injuries that were inflicted by them, led to reprisals upon the more peaceful and well-intentioned settlers, which rendered life, except in the immediate neighbourhood of the most settled districts, so insecure as sensibly to damage the prospects of the colony, and to cause an urgent cry for Government interference. It is stated by Mr. Calder[*] that "in the five years preceding the close of 1831, ninety-nine inquests were held on the bodies of white people killed by blacks, and of course there were many more who were not known; and in the same period sixty-nine Europeans were reported wounded in encounters with natives." Of the corresponding losses upon the other side it is not possible to form an estimate. An unsuccessful endeavour was made by the Government to divide the country between the two races by a line of demarcation, and a proclamation to that effect was issued on April 15th, 1828; but as there were no means of imparting a knowledge of its contents to those most concerned, who naturally imagined they had a right to wander at their free will through the land which was once their own, it led to no result. More severe measures were then tried, and on October 1st, 1830, martial law was proclaimed against the blacks throughout the island, and the famous operation of the "Line" commenced. The intention of this was to surround the whole of the native tribes by a military cordon, reaching across the island, and gradually to close upon them and finally drive them into Tasman's Peninsula, on the east side of the island, which has a narrow neck, scarcely a mile in width, which was afterwards to be guarded and fortified; and here they were to be kept, while the European population enjoyed their lands in peace. This great operation, which employed nearly the whole population, military and civil, for many months, and cost the colony, it is said, upwards of 30,000*l*., resulted in the capture of a single black. When the line closed on the neck of the peninsula it was found that all the rest, active, supple, and naked, acquainted with the passes and byways of their accustomed hunting-grounds, had eluded the vigilance of their would-be captors. The original number of the natives appears by this time to have been greatly diminished. Those that had become partially civilized, and had attached themselves as labourers and dependents upon the European farms and families, were dying out, as such people always do, under the influence of the altered mode of life, and the habits (especially spirit drinking) and diseases acquired by contact with whites; and those that retained their original wild condition, were hunted from place to place, and harassed by perpetual skirmishes, not only with the English, but with each other; for when one tribe found its land occupied by the English it was driven into the territory of another, a serious matter to people with no resources for food but the chase in a country not abundantly supplied with wild animals. Under these depressing circumstances not only was the mortality of the adults great, but that of the children was greater still; and so it came about, as was ascertained by the events now to be narrated, though it was not suspected by the English colonists, that by this time the entire native population of the island had been reduced to little more than 300.

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[*] 'Third Voyage,' vol. i. p. 96.

[†] I am indebted to the painfully interesting work, 'The Last of the Tasmanians,' by James Bonwick, for most of the facts mentioned in connection with this subject.

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[*] "Some account of the Wars of Extirpation and Habits of the Native Tribes of Tasmania," 'Journ. Anthrop. Inst.' vol. iii. 1872, p. 7.

After the failure of the "Line," other methods were tried to secure the natives, chiefly the offer of rewards for individual captures, but the desired end was finally achieved in a manner almost unexampled in the history of such transactions. There was at that time living in Hobart Town a man of the name of George Augustus Robinson, a bricklayer by trade, of strong religious feelings, and of great enthusiasm for the cause of the oppressed blacks. He had for some time entertained the idea of gradually reclaiming and civilizing them by methods of conciliation; and he was well fitted for this object, having a remarkable natural gift for acquiring influence over them and gaining their confidence and esteem. He gathered round him at a place at Bruni Island, allotted him by the Government, as many as he could induce to adopt settled habits, taught them the rudiments of European education, and learned what he could of their languages and ideas. Believing that the only remaining hope for the savage tribes was to bring them under similar influences, he undertook, notwithstanding their exasperated state, to go among them, with a few English and native companions (among whom were the two by whose busts, now in many of our Anthropological museums, the features of the Tasmanian will be chiefly known to posterity, Wouraddy, and his wife Truganina, afterwards celebrated as the last survivor of the race), without arms of any description, and to persuade them, by promises of protection and good treatment, voluntarily to surrender their freedom. In the course of three years he actually succeeded in accomplishing his end. The last party of eight, consisting of one man, four women, and three boys (one of the latter being afterwards known as William Lanney, the "last man"), were taken at Western Bluff, December 28, 1834, and brought into Hobart Town, January 22, 1835, amid great rejoicings from the colonists. As the successive parties were brought in by Robinson, they were shipped off, first to Swan Island, then to Gun Carriage Island, and finally established on Flinders Island in Bass Straits. The whole number settled here scarcely exceeded 200. They were fed, clothed, and educated (most of them learning to read and write) at Government expense; but the unfavourable climate, total change of mode of life, absence of all the interests and excitements of the chase or of war, and home-sickness, told rapidly on their health. They died one after the other, until, in October, 1847, being reduced to forty-four in all—twelve men, twenty-two women, and ten children—this remnant, thoroughly tamed and not likely to occasion any further alarm to the English colonists, were once more allowed to return to their native land. A reserve of 1000 acres was assigned to them at Oyster Cove, not far from Hobart Town, and here they were kept under superintendence. Their numbers, however, continued to decrease at the same rate as before, and they lived the degraded life common to half-reclaimed savages, without interests, occupations, or hope. In 1854, there were three men, eleven women, and two boys alive. On the 3rd of March, 1869, died the last male of the race, William Lanney, mentioned before. He had become a sailor, and had made several voyages in a whaling ship, but unfortunately had, like so many in his position, taken to intemperate habits. In June, 1876, died the last woman, Truganina, or Lalla Rookh, as she was afterwards called, the faithful companion of Robinson's conciliatory missions, and who had been, at least on one occasion, the means of saving his life.

I have given this brief outline of what may be called the political history of the Tasmanians, though perhaps departing in doing so from the general scope of the lecture, because of its completeness, and of the illustration it affords, in a concise form, of the almost inevitable results of the contact of two such absolutely different races as the English and the Tasmanian. The details of the history are saddening and painful in the extreme, and yet it would be difficult to say what the world has lost by the extinction of the Tasmanian aborigines.

It appears tolerably certain, from what has been mentioned before, that the Tasmanians were not a numerous race, the various estimates of the whole population of the island, at the time of its settlement by the English, ranging between 4000 and 7000; so they must have been very thinly scattered, and many large districts must have been quite uninhabited. Their isolation from all the rest of the world was more absolute even than that of the Australians, and they were consequently inferior even to them in all the arts of civilization. They possessed no boats by which the straits between Tasmania and the Australian land could be crossed, and they show no indications of ever having been visited by, or receiving any extraneous culture from, natives of any of the Pacific Islands.

Like the Australians, they were divided into numerous small tribes, each speaking a different dialect, as many as nine having been recognized. They had no fixed habitations, wore no clothes of any kind, did not cultivate the ground, or keep domestic animals, had no pottery, and no bows and arrows. They were inferior to the Australians in not knowing either the boomerang or the throwing stick, in having no shields, no dogs, and apparently not knowing how to procure fire as occasion needed, as they always carried with them burning torches of vegetable fibre, which it was the especial duty of the women to tend and keep alive.

It is difficult, indeed, to imagine human beings living in a lower social condition than that of the aboriginal Tasmanians, and yet the partial education which some of the race underwent before their final extinction, showed that they possessed capacities, intelligence, and moral qualities, by no means inferior to those of many other of the uncivilized races of the world.

As might be supposed, the Tasmanians, having lived in all probability for a great length of time on a restricted portion of the earth's surface, under similar external conditions, and without any intermixture from any alien race, have developed, or at all events perpetuated, a very great sameness of physical characters; and have come to possess a peculiar structural type, by which, taken in its entirety, they can be distinguished from all other people.

It is greatly to be regretted that so little evidence of this has been preserved. Four complete skeletons * and less than thirty skulls, of both sexes and various ages, in this country, are all that we have by which to estimate their stature, proportions, and conformation generally. Their external appearance we judge of by descriptions, some portraits more or less indifferent in execution, some valuable photographs (though on too small a scale) of the latest survivors, and two excellent busts, before mentioned, of a man and a woman modelled by Mr. Murray, of Hobart Town. Of their remaining anatomical structure, nothing will ever be known; in fact we must now, when speaking of them zoologically, treat them as we do fossil animals, and rely chiefly on their bones for distinguishing characters; and the habit of burning their dead, which prevailed as long as they remained in their natural condition, renders these far scarcer than could be wished. It is greatly to be hoped, however, that the present occupiers of their land, who have profited so largely by their extinction, will spare no pains to search for, and secure to science, all that still remains of the race, which they or their predecessors have been the means of destroying.

The height of the Tasmanians is stated to have been somewhat below that of the Australians, but they were of rather stouter build, their bones being generally less slender. The average height of the three male skeletons in England is 5 feet 3½ inches, that of the female 4 feet 7½ inches. Of course these numbers are too small to place much value upon; but they do not differ greatly from Mr. G. A. Robinson's measurements of twenty-three Tasmanian men, whose height varied between 5 feet 1 inch and 5 feet 7½ inches, the average being 5 feet 3¾ inches, and of twenty-nine women, who were between 4 feet 3 inches and 5 feet 4½ inches, the average being 4 feet 11¼ inches. In colour, they were not unlike the darker complexioned Australians, but they completely differed from that race, or at least from the great majority of them, in the character of the hair, which was not straight, but crisp or frizzled. The women

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* These have all been obtained and sent to England by Mr. Morton Allport, of Hobart Town. Two are in the Museum of the Royal College of Surgeons, one in that of the Anthropological Institute, and one in the splendid private collection of Dr. Barnard Davis, at Shelton, in Staffordshire.

used to shave or burn it off close to the scalp, but the men allowed it to grow long, when it assumed the form of small, corkscrew ringlets, which they were in the habit of covering with grease and red ochre, giving it the appearance of a mat or mop of red strings hanging over the head and neck. Naturally its colour was brown of the darkest shade, or what is usually called black. They had a well-developed beard and whiskers, of the same fine curly or "frizzly" nature. In the general character of the features, they do not appear to have differed much from the Australians, having similar heavy brows, short, broad noses, and wide mouths.

The osteology of the Tasmanians has been described by Dr. Barnard Davis * and by Dr. Topinard,† from materials in the collections at Shelton and at Paris. The Museum of the College of Surgeons contains the largest series of skulls at present existing, but they have hitherto been but imperfectly and partially described. They are fifteen in number, of which three are young, and therefore not available for average measurements. Of the adult skulls, six appear to be those of men and six of women. The sexual characters are very well marked, the difference in size being particularly striking. There is no case of artificial or pathological deformation among them.

The crania have the general angular form, prominent median ridge above, and flattened upper parietal region noticed in the Australians, but their special character is a prominence of the parietal eminences, not found in any of the Australian crania, and developed to a greater or less degree in all, and giving a greater latitudinal index. This is even seen in the cranium of a young infant, the form of which is characteristically different from that of an Australian child of corresponding age. Most of the skulls of this series show the elevations and depressions of the surface pointed out by Topinard, as distinguishing the Tasmania crania in the Paris collection, for a description of which I must refer to the memoir cited above. Seen from behind, the skull appears pentagonal, though broader in proportion to its height than the Australian. The glabella is prominent, and overhangs the nasals in every case, even in the females, though to a less extent than in most Australians. The mastoids, inion, and other muscular ridges, are rarely much developed.

Having mentioned that there was no case of metopism or persistence of the frontal suture among the Australians, it is interesting to note that one of the Tasmanian skulls in the collection, that of an old woman, is metopic, and that the skeleton of an adult man in the Museum of the Anthropological Institute is in the same condition. With regard to the ptereon (and this is important in relation to the formation of this region in the Melanesians), in no case does the squamosal meet the frontal, though it comes very near it in fifteen

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* "On the Osteology and Peculiarities of the Tasmanians." Three Plates. 'Nat. Verhand. der Hollandsche Maatsch. der Wetenschappen,' 1874.

† "Étude sur les Tasmaniens." 'Mem. de la Soc. d'Anthrop.' t. iii. p. 307.

out of thirty-four cases, and in fourteen epipteric bones are developed. In only five out of the thirty-four is the spheno-parietal suture longer than half a centimetre.

The capacity of the Tasmanian cranium has been estimated, both by Barnard Davis and by Topinard, as somewhat greater than that of the Australian, and the measurements of this series lead to a similar conclusion. The average of the six male crania is 1309 cubic centimetres, or 79·9 cubic inches; that of the six females; 1135 cubic centimetres, or 69·3 cubic inches; the general average, 1222 cubic centimetres, or 74·6 inches. Of course these data are taken from too small a series to be regarded as more than approximations. They are somewhat lower than those given by both the authors just named, and though something may be due to difference of method of measurement, it is clearly not entirely so, as in other dimensions, in the estimation of which there is no difficulty, I find that my skulls are inferior to theirs. Thus the average horizontal circumference of the six males is 20·2 (the maximum being 20·4), while the skull at the Anthropological Institute is 21·3, the average of six in the Shelton collection 20·6, and the average of the six measured by Topinard is also 20·6.

As mentioned above, in consequence of the development of the parietal eminence, the latitudinal index is considerably greater than in the Australian. In my series it varies between 72 and 80, the average being 76·0. It is curious that this is exactly the same as the average of nine skulls at Paris measured by Topinard, but the fourteen in the Shelton collection give, according to Dr. Barnard Davis, a lower average, viz. between 73 and 74. The average altitudinal index is 72·8, or nearly the same as that of the Australians. Individually, in twelve out of fifteen crania is the height less than the breadth. The superior width of the skull lies wholly in the parietal region, the average frontal and zyomatic diameters not differing appreciably from those of the Australians.

Turning to the face, we find the principal features presenting a great resemblance to those of the Australians. The projection of the jaws, although in one instance it is so excessive as to disturb the average considerably (the index in this case being 113), is not generally so great as in the Australians—only four out of nine capable of being measured (a very insufficient number for obtaining an average of so variable a character) being within the limits of true prognathy, four being mesognathous, and one truly orthognathous (index 97), as defined above. The ophryo-alveolo-auricular facial angle gives an average of 67·8, or considerably higher than that of the Australian, almost as high, in fact, as the Italian. The face is short from above downwards, the measurement N A being less even than in the Australians (65 for the males, 58 for the females), and the malar bones are small, very shallow from above downwards, and retreating. The orbits are remarkably different in the two sexes; in the males they are low and elongated, with heavy overhanging supraciliary ridges, having an average index of 76·3, lower even than the male Australians; while in the females they are more rounded and open, giving an average index of 85·4, the general average for the race being 80·8, or decidedly microseme. Broca gives the general average of the orbital index of the Tasmanian skulls at Paris at very nearly the same figure, viz. 79·33, but he did not find so great a discrepancy between the sexes.

The form of the nasal bones and of the nasal aperture is not very different from that of the Australians, and, as with them, there is no example of a leptorhine nose among them. Two are mesorhine and the remaining ten platyrhine, the average index of the twelve being 56·8, which accords remarkably with Broca's average of 56·92, found in eight specimens at Paris. The general average of this important index is practically therefore the same as in the Australian.

In the large size, and strongly pronounced character of the teeth, the Tasmanians resemble the Australians; in one point, however, they seem to differ, not only from that but from all other kindred races; and this is one which, I believe, has not been previously recorded. It is the tardy development and irregular position of the posterior molars. These teeth are generally of large size, but there appears to be too little room for them in the jaw, so that only in two out of eleven adult skulls in which their condition can be observed, are all of them normally placed; in all the others, one or more of the wisdom teeth are either retained beneath the alveoli, or are in oblique or irregular positions. This is the more remarkable, as I have never observed a similar condition in any single authentic Australian skull, although occasionally, as before mentioned, small and imperfectly developed wisdom teeth may be found among them.

In the three proportions of the skeleton, which have been mentioned, in which the Australian differs from the European, viz. the greater antero-posterior diameter of the pelvis as compared with its width, the greater length of the tibia as compared with the femur, and especially the greater length of the forearm as compared with the humerus, the few Tasmanian skeletons which I have examined agree completely with the Australian type. The average of the pelvic indices of the three male Tasmanian skeletons in this country is 93; that of the one female, 79.

It will be seen by a reference to the map, that Tasmania, Australia, and the large, numerous, and closely placed islands, which lie between the latter and the Asiatic continent, divide the two great water tracts, called respectively the Indian and the Pacific Oceans. The Pacific Ocean proper is studded over with an enormous number of islands, all, with the exception of the New Zealand group, of moderate or small size, and to which the term Polynesia is collectively applied. Although this ocean had been traversed several times by the enterprising Spanish, Portuguese, and even English voyagers of the early part of the seventeenth century, and the route from the west coast of America to the Malay Archipelago was well known, the discovery of the greater number of the islands and the establishment of permanent relations with their inhabitants, was reserved for the numerous expeditions made at the latter part of the last century, which had for their special object the exploration of this then comparatively unknown region of the world. Among these the most memorable, both for the extent of new acquisitions to knowledge and for the importance of the results upon the world's history, were the three voyages of Captain Cook. At that time the observation was made, to quote the words of Forster, who accompanied Cook as naturalist in his second voyage, that there are "two great varieties of people in the South Seas—the one more fair, well-limbed, athletic, of fine size, of a kind, benevolent temper; the other, blacker, the hair just beginning to become woolly and crisp, the body more slender and low, and their temper, if possible, more brisk, but somewhat mistrustful. The first race inhabits Otaheite and the Society Isles, the Marquesas, the Friendly Isles, Easter Isle, and New Zealand; whilst the second peoples New Caledonia, Tanna, and the New Hebrides, especially Mallicollo." Subsequent observation has fully confirmed this division, and since the anatomical characters of the two races have been studied, it has been found that they show many strongly marked contrasts. This is seen especially when pure types of each have been examined, for, as might be expected, with races living in close proximity, often occupying the same small island, and prone to invade each other's territory, and to make extensive migrations by sea, a great mixture has taken place, not only along the boundaries of the respective regions inhabited by each, but even extending at certain points far into the interior. As it has become necessary to give distinctive names to these races, that of Polynesian, at first applied indiscriminately to the whole, is now usually restricted to the fairer race of Cook and Forster, while "Melanesian" has been invented for the darker race, although this word has frequently been used in a wider sense than I shall employ it here, to include Tasmanian, Papuan, and even Australian. "Papuan" is often also used as a race-designation for the people inhabiting the New Hebrides and adjacent islands; but the appropriateness of these terms will be better discussed after the facts at present known, connected with the structure and distribution of the races, have been stated.

The islands which at the present time are either wholly or mainly inhabited by the Melanesian race, are that group lying in the western part of the ocean, not far from the coast of Australia, and joining at their northern extremity to the great island of Papua or New Guinea, the principal being New Caledonia, the New Hebrides, the Solomon Islands, New Britain, and New Ireland, which form a nearly continuous semicircular chain embracing the north-east coast of Australia; and lying somewhat away from the rest, the Fiji group, the last but one of the annexations of the British empire.

The most southernmost and largest of these is New Caledonia, about 200 miles long and 30 broad. It was discovered by Cook in 1774 on his second voyage, and has recently been taken possession of by the French, and used as a penal settlement. To the north and east lies the long chain of the New Hebrides, with the Santa Cruz Islands still farther to the north. These were first discovered by the celebrated Spanish voyager Quiros in 1606, who considered them to be part of a southern continent to which he gave the name of "Tierra Austral del Espiritu Santo"; the latter part of this name being still retained for the largest island of the group, 70 miles long by 25 broad. They were visited in 1768 by the French Admiral, Bougainville, who, besides landing on the "Isle of Lepers," did no more than discover that the land was not a continent, but composed of numerous small islands, which he named the "Great Cyclades." Cook, in 1774, made a complete exploration of them, and thought himself justified therefore in changing the name to "New Hebrides," by which they are now generally known. He visited and stayed some time at Mallicollo and Tanna, but was repulsed by the natives at Erromango; he also partially surveyed several of the other islands, and gave the name of "Sandwich" to one, now more generally known by its native name of Vati.

The New Hebrides, and the nearly adjacent Santa Cruz Islands, have not yet been annexed by any European power, but they have for some years past been visited by the crews of European vessels, actuated by very different motives, and producing very different effects by their visits on the natives. These may be divided into four classes—(1) traders in sandalwood; * (2) collectors of labourers for the plantations of Queensland, Fiji, &c., often no better than kidnappers: to repress and repair the infamous deeds of these two classes, came (3) officers of the English war vessels which cruise in those seas, some of whom, as Captains Erskine and Goodenough, have given interesting accounts of the condition of the islands; and (4) missionaries of various denominations. It is very unfortunate for the reputation of the Melanesians, that the most striking historical events connecting them with our country have been the murders of three most excellent men, who were zealously labouring for their welfare, the Rev. J. Williams, at Erromango, in 1839; Bishop Patteson, at Nukupu, in 1871; and Commodore Goodenough, at Santa Cruz, in 1875—all due, in great probability, to the irritation and suspicion caused by the behaviour of previous visitors of different character and motives. Captain Goodenough himself wrote, "It is remarkable that just in proportion to the amount of people who have been taken away as labourers, so are the natives inclined to assault Europeans. Where white men are least known, the people are most friendly." †

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* One of these men, after he had taken in his cargo, was known to fire indiscriminately among the natives, in order to spoil the trade to those who should come to the island after him, and so keep up the price of the article.—Erskine's 'Journal of a Cruise among the Islands of the Western Pacific,' 1853, pp. 330 and 393.

† In historical justice, it should, however, be recollected, that Cook on his first visit to several of the islands of the Pacific was attacked by the natives, and only avoided bloodshed by abandoning the attempt to land.

The social condition of all the natives of these various islands when first discovered was greatly in advance of that of the Australians and Tasmanians, as the following extracts from Cook's account of his landing in New Caledonia, show:—"The ground near the village was finely cultivated, being laid out in sugar-canes, plantains, yams, and other roots; and watered by little rills, conducted by art from the main stream, whose source was in the hills. Here were some cocoanut trees, which did not seem burdened with fruit. We heard the crowing of cocks, but saw none. Some roots were baking on a fire in an earthen jar, which would have held six or eight gallons; nor did we doubt its being their own manufacture. * * * * The plantations were laid out with great judgment, and cultivated with much labour."

The condition of the other islands differed only in details. Their inhabitants possessed fixed habitations, thatched, and sometimes of more than one story, grouped together in villages: as just mentioned, they cultivated the ground, and they reared domestic animals, fowls, and in some cases pigs. They had large double canoes, 30 feet long, connected by a deck or platform, and with a lateen sail or sails, though of a more clumsy construction than those of the Friendly Islands, as the keen eye of the great navigator does not fail to notice. The men wore a girdle of bark or leaves, and the women a short petticoat. They had also earrings of tortoiseshell, necklaces or amulets and bracelets of shells and stones. They fought with bows and arrows, spears and darts. In many respects their moral character, as far as Cook was able to judge of it, contrasted favourably with that of the still more civilized and polished inhabitants of the Society and Friendly Islands, among whom he had just been sojourning. The New Caledonians were "not the least addicted to pilfering, which is more than can be said of any other nation of this sea." Some remarkable instances are related of the honesty of the people of Mallicollo, and the women were everywhere far more reserved and decorous in their behaviour than the true Polynesians.

Captain Goodenough estimated the population of the New Hebrides, Banks, and Santa Cruz Islands, in 1875, at about 70,000, but gives striking evidence of its rapid decrease since the establishment of regular intercourse with Europeans. Not a single island that he visited but presented the same appearance of diminishing population.

It is probable that the range of the Melanesian race was at one time more extensive than at present, and that they were settled in the islands where they at present dwell, and also in many others now occupied by Polynesians, before the arrival of the last-named race, who appear in many cases to have supplanted them. Even in the true Melanesian area, geographically speaking, there is a considerable infusion of Polynesian influence, apparent in the physical characters, customs, and language of the people. The inhabitants of some of the Loyalty Islands, near New Caledonia, for instance, are almost pure Polynesians, derived from an immigration which took place from Uea or Wallis Island in the beginning of the last century. Among the

New Hebridean Islands there are several Polynesian colonies of quite modern origin. In New Caledonia, according to Bourgarel,[*] the yellow race (Polynesians) number about one-fifth of the whole population, the black race (Melanesians) two-fifths, the remainder being formed by a mixture of the two. The chiefs, and what may be called the aristocracy of the island, mostly belong to the lighter race. On the other hand, traces of the former presence of a Melanesian population are found in some of the central and even eastern Polynesian Islands, as far, according to W. L. Ranken, as Niue (Savage Isle), Penrhyn Atoll, and Rarotonga,[†] and in all probability in New Zealand. The different proportions in which the two races are mixed is one of the circumstances which has given rise to the diversities observed in the appearance and character of the inhabitants of many of the islands.

As there is every reason to believe that the Melanesians have been established in the islands they now occupy for an immense length of time, and as they are not naturally given much to rove from place to place, like their lighter-coloured neighbours the Polynesians, strongly marked, special characters have been developed in the inhabitants of the different islands; and it is probable that if sufficient materials were collected, we might be able to distinguish even by the skull alone the particular island from which it was derived. Those who doubt the value of the cranium as a race-character may be surprised at this assertion; but the very few and imperfect observations already made lead me to think that it is probably true. The fact that a skull brought from a particular island without any history, beyond its having been found there, presents characters unlike those generally associated with the inhabitants of that island, proves nothing, as it may have belonged to an individual of another race, who had found his way there by some accidental circumstance. The frequent occurrence of such a case, should convince collectors of the necessity of obtaining larger series from each locality than we are now at present contented with. The larger the series, the more chance is there of obtaining average characters of the predominating race, and of eliminating the influence of individual variations and accidental mixtures.

Unfortunately our knowledge of the distinctive characters of the people of the various islands of the New Hebrides and Santa Cruz Archipelagos is still most imperfect, and that of the Salomon Islands and New Ireland and New Britain even more so. It will be better, therefore, for the present purpose to group them all together, and attempt to describe the characters of what may be considered as the average or generalized Melanesian type.

In stature these people present considerable variation. Some, as those of Mallicollo, are decidedly undersized; they are described by Cook as "a rather diminutive race," and by Goodenough as "a small, poor, weedy people." Cook says, "the people of Tanna are of middle size, rather slender than otherwise; many are little, but few tall and stout." Erskine says, "the people of Vati are of larger stature than the Tannese." Goodenough tells us, "the natives of Espiritu Santo are fine-looking men compared to those of Mallicollo, and reminded me of Fijians;" but we have no accurate measurements of any sufficient number, and there are no skeletons in any of the English museums.

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[*] Bourgarel, "Des Races de l'Océanie Française," 'Mém. de la Soc. d'Anthropologie de Paris,' vol. i. 1860.

[†] "The South Sea Islands," 'Journ. Anthrop. Inst.' 1877.

Their head is narrow, the forehead especially, and often retreating. The brow is not so prominent as the Australian. The nose is narrow at the root, but broad below, with wide nostrils; its root is not so depressed as the Australian, and its dorsum is often prominent and arched. In many cases, especially among the northern islands, it assumes what is commonly described as a "Jewish form," arched, and with the tip prolonged downwards. So common does this form of nose appear, that it may almost be considered characteristic of the race. The whole face is rather "hatchet shaped," the sides sloping away from the middle line. The jaws are prognathous and the lips thick. The complexion, though often called "black," in common parlance, is really a dusky brown or chocolate colour. By the character of the hair they are distinctly separated from the Australians, the Malays, and Polynesians, and allied to the Tasmanians and the Negritos, or black people of the Malay Archipelago. On the head it is rather coarse, elliptical in section, and more or less closely curled or frizzled. When allowed to grow long, sometimes it hangs down in close spiral ringlets, as with the Tasmanians, but it more often forms a large fuzzy mop, standing out to a considerable distance all round the head, which remarkable *coiffure* frequently occupies a considerable amount of time and attention on the part of the owner to keep in order. Though the hair is always naturally black, or nearly so, its colour is often artificially modified by the application of caustic lime, made from burnt coral, and by various colouring agents—a practice common among the inhabitants of the Pacific, and which has given rise to reports of fair, brown, and red hair among them; whereas black, or the dark shade of brown commonly so-called, is the universal colour of all the races spoken of in this lecture, as of the great majority of the people of the world. The beard is generally well developed, as is the hair upon the limbs and chest.

My remarks upon the osteological characters of the Melanesians must, from the paucity of materials, be limited to the skulls, and chiefly to those in the collection under my charge. These are twenty-one in number, six from the Isle of Pines, a small island near the southern extremity of New Caledonia; three probably from Vati or Sandwich Island; eight from Mallicollo, collected by the late Commodore Goodenough, and by Dr. A. Corrie, Assistant Surgeon to the 'Pearl'; two from Vanikoro, and two from the Salomons. Crania from New

Caledonia have been described by Bourgarel, and from the New Hebrides by Dr. Barnard Davis and Mr. Busk.

The skulls from Mallicollo all present a remarkable flatness of the frontal region, strongly suggestive of artificial pressure in infancy, such as is, or was formerly, practised by many of the Western Americans. It is not, however, the flattening produced by squeezing between two boards, as with the inhabitants of British Columbia, as there is no sign of counter pressure on the occiput, and no lateral bulging of the cranium. The forehead is simply depressed, the remainder of the skull retaining its normal form. This peculiar conformation of the head attracted the attention of Cook and Forster in the living people. The latter says: "In Mallicollo we observed that the greater part of the skulls of the inhabitants had a very singular conformation; for the forehead, from the beginning of the nose, together with the rest of the head, was much depressed, and inclining backwards, which causes an appearance in the looks and countenances of the natives similar to those of monkeys." *

No evidence has, however, yet been obtained of the existence of such a practice among the inhabitants, and no crania from any of the other islands yet examined present any sign of it. If it should prove to be a natural conformation, it will be one without parallel in any known race; if the result of custom, it will be very singular, as being peculiar to one out of hundreds of islands of the oceanic area.†

The average capacity of the eighteen Melanesian skulls in the collection which can be measured is 1320 cubic centimetres, or 80·5 cubic inches. This includes some females; but as there is some difficulty in distinguishing the sexes in several cases, I have taken them all together. It will be observed that this is higher than the average Australian *male* by about 2 cubic inches; showing, if so small a number of specimens can be relied upon, that the Melanesian, like his nearer relation the Tasmanian, is better endowed in this respect than his Australian neighbour.

The most striking general character of the cranial part of all these skulls is their great length and narrowness (see Fig. 3), the sides being remarkably flat, especially in the posterior parietal region. In this conformation they differ totally from the Tasmanians. The average latitudinal index among twenty skulls is 70·4, lower than the Australian, and in fact than any other known race; ‡ and among them are some of the longest and narrowest normal skulls known, one having an index as low as 62. The highest of the series is 75. The relative proportion of height compared to breadth in skulls of the New Cale-

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* J. R. Forster, 'Observations made during a Voyage Round the World,' 1778, p. 267.

† Flattening of the occiput is not uncommon among the South Sea Islanders, as among many other races; but it is probably undesigned, and arises from the practice of keeping the infant lying on its back upon a hard board or pillow.

‡ It is singular that the Eskimo, though so widely different in many other characters, approaches nearest to the Melanesian in the lowness of the latitudinal cranial index.

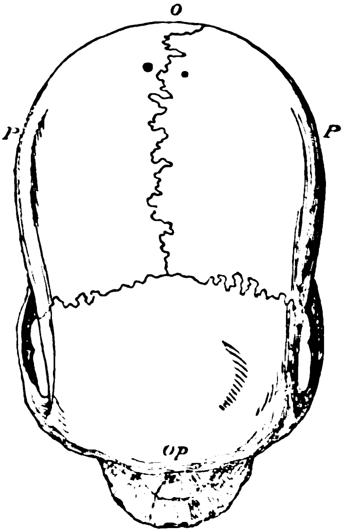

Fig. 3.—The upper surface of the skull of a Melanesian, from the island of Vanikoro, as an example of a dolichocephalic cranium, the relation of the greatest breadth (P P) to the length (Op to O) being as 70 to 100.

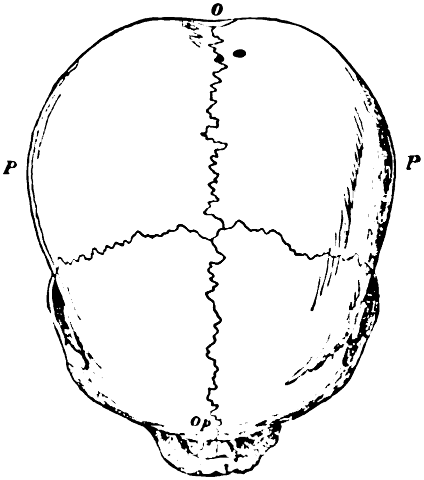

Fig. 4.—The upper surface of the skull of a Polynesian, from the island of Lifuka (Tongan group), a brachycephalic cranium, the relation of the greatest breadth (P P) to the length (Op O) being as 84 to 100. It also shows metopism, or persistence of the frontal suture.

donians has been already commented upon by Dr. Barnard Davis, who has applied the term "hypsi-stenocephalic" to them.* It is remarkable that in this series (none of which are from New Caledonia itself, though from neighbouring islands) the height of every skull, measured from the basion to the bregma exceeds the greatest parietal diameter, and the average altitudinal index is 74·6, or considerably greater than in either the Australian or Tasmanian. In six skulls of New Caledonians, apparently of the pure Melanesian race, in the Army Medical Museum at Netley, the average latitudinal index is 70·1, and the altitudinal index 73·9. These results nearly accord with those obtained from the larger collection at Paris, the respective indices of which, according to Broca, are 71·8 and 73·7, but these probably include some specimens of the mixed race.

In no race known does the condition of the ptereon differ so greatly from the average of Europeans as the Melanesian. This is more especially seen in the Mallicollo skulls, where it is the exception for the squamosal not to join the frontal bone, as it does so in ten cases out of sixteen cases, and sometimes very largely. Among these eight skulls there are also two cases of metopism.

The face is generally short, shorter even than that of the Australian, but it has much the same general characters, as narrowness of the frontal region, and weak and retreating malars. The prognathism is almost always marked, the indices of the different skulls in which the bones of the face are sufficiently perfect to allow of measurement never falling below 100, and rising as high as 111 in two and 115 in one case, the average being 105·4. The nasal bones and the nasal aperture are short and broad, but the former have not the extreme reduction and flattening characteristic of the Australian. The index is invariably above 50; the lowest being 51·1 and the whole series averaging 54·9, or rather below that of the Australian.

In the form of the orbit the Mallicollo skulls differ considerably from the others, the upper margin being elevated, drawn back as it were by the depression of the forehead, so as to give an almost circular shape to the opening, as in the case of the deformed skulls of the ancient Peruvians, an argument in favour of the artificial origin of this conformation. The average of the orbital indices in the skulls from this island rises as high as 90, whereas in those from the other islands it is not higher than 81, much the same as the Australian. The prominence of the supraorbital ridges and glabella is occasionally strongly pronounced, but is not universal as in the Australians.

The lower jaw, in some cases, presents most of the marks of inferiority of character which have been pointed out when speaking of the Australians. The teeth have unfortunately been lost in the greater number of the crania of this series, so that I am not able to give average measurements of any value; but they do not appear generally to have been so large and well developed as in the Australians. In one skull from the Isle of Pines, the third molars are misplaced, as was found to be so frequently the case with the Tasmanians.

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* 'On the Peculiar Crania of the Inhabitants of certain Groups of Islands in the Western Pacific,' 1866. Natuurk. Verhand. van de Hollandsche, Maatschappij der Wetenschappen te Haarlem, xxiv. Deel.

People having very much the same physical characters as the Melanesians inhabit the islands of the Louisiade Archipelago, those of Torres Straits, and a very considerable part of New Guinea, and even some of the islands farther west, as Aru, Timor, Gilolo, &c. The exploration of New Guinea in an ethnological sense is only now commencing, and promises a most interesting future. The greater part of the island is certainly inhabited by a dark-skinned race, with crisp or frizzled hair; indeed the name by which they are frequently known, "*Papuans*," is said to allude in the Malay language to the latter peculiarity. It is, however, very doubtful whether they all possess the uniform characters of the genuine Melanesian. In a collection of skulls lately presented to the Museum of the College of Surgeons from the east end of the island, by Dr. P. Comrie, late of H.M. ship 'Basilisk,' while some present the characteristic form of that race, others are short and round, and have facial characters indicating either Polynesian, Malay, or Negrito mixture. The same appears to be the case in other parts of the island.

The Museum of the College contains seven skulls of adult males from islands in Torres Straits, chiefly Erroob or Darnley Island, collected by Mr. Jukes during Captain Blackwood's Survey in 1842–46, and by Mr. Huxley in Captain Owen Stanley's Survey in 1847–50. The average cranial capacity of these skulls is 1340 cubic centimetres, or 82 cubic inches; the average latitudinal index, 72·6; the average altitudinal index, 75·6; gnathic index, 105; orbital index, 84·2; and nasal index, 52·8; so that except that the last measurement is somewhat lower (but it is only taken from five skulls, the other two having the face damaged) and the breadth rather greater, there is little to distinguish them from the New Hebrideans. In culture these people are quite on a par with the last, and contrast strongly with the Australians with whom they come in contact at Cape York.

The inhabitants of the Admiralty Islands to the north of New Guinea, on which no European landed until it was visited by the 'Challenger' in March, 1875, appear to belong to the same race, according to an interesting description of them by Mr. H. N. Moseley,* but their cranial characters have not yet been described.

Black-skinned people, with close curly hair, have long been known to exist still farther to the west of the Malay Archipelago, in the Philippines, the Andamans, and even the interior of the Malay Peninsula. As long as very little was known of their anatomical conformation, they were confounded with the Melanesians, and the name "Negrito," first applied to them by the Spaniards in Luzon, has often been used for all the people having these two characters, dark complexions and frizzly or woolly hair, throughout the Indian Archipelago and Pacific Ocean, even as far as Tasmania. More complete information has, however, shown that the blacks of the western and northern Malay region differ in many characters from the Melanesians, and I shall follow Quatrefages in restricting the term "Negrito" to them.

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* "On the Inhabitants of the Admiralty Islands," 'Journal of the Anthropological Institute,' May, 1877.

They are found at present in the most unmixed condition in the Andamans, a chain of long, narrow islands, in the Bay of Bengal, about 20 miles in breadth and 140 in length, and divided by several narrow channels. These islands were surveyed in 1789 by Lieut. Blair, of the East India Company's Service, and a penal settlement founded upon them, but this was abandoned a few years afterwards; and the islands were not visited by Europeans again until in 1857, after the Indian Mutiny, a commission, consisting of Drs. Mouat and Playfair and Lieut. Heathcote, was sent to examine them, and in consequence of their report, Port Blair was established as a convict settlement for our Indian possessions.

The islands were inhabited by a peculiar race of people, who must have lived in them for a great length of time, with very little, if any, admixture from other races, and have consequently acquired strongly marked and very uniform characteristics. Their warlike disposition, notwithstanding their diminutive size, and their implacable hostility to strangers who were led by accident or design to their shores, have been the chief causes of their isolation. The earliest accounts that were published of their condition led to the belief that their moral and social organization was upon the lowest scale, and that they were among the most degraded of mankind; but the larger experience which has been acquired since the establishment of the settlement, the elaborate memoir of Quatrefages,* containing references to all that had previously been written, the more recent information furnished on personal observation by Day,† Dobson,‡ and others, and the very detailed account of the customs and arts of the Andamanese lately transmitted to the Anthropological Institute, with a large collection of their weapons and manufactures, by Mr. Man, has caused a considerable modification of this idea.

The numerous photographs which have been taken and sent to this country give a very good idea of their external physical characters, and a close examination of them shows that the resemblance to African negroes, which appears to strike everyone who sees them for the first time, is rather superficial, and depending much upon colour and the character of the hair, though, by the way, this last is seldom seen, as both men and women have an almost universal custom of keeping the head closely shaved. When the hair does grow, however, it is found to be as finely frizzled as that of the most "woolly-headed" African. A specimen sent home by Mr. Man, for which I am indebted to General Lane Fox, is smaller in transverse section and more flattened than that of any Melanesian or Papuan which I have examined, and very nearly as much so as the hair of the Bushmen of South Africa. Whether the men are full-bearded I cannot say, as the face is always as carefully cleared of hairy appendages as the scalp. The head is very short and round, the forehead flat and tolerably full; the space between the eyes wide; the nose small, straight, and not very broad. In photographs of the full face of Australians, Melanesians, and African negroes, the width between the outer margins of the alæ nasi is usually fully one-third of the whole breadth of the face, sometimes more, rarely less, corresponding to the platyrhine character of the nasal aperture. In the Andamanese it is scarcely more than one-fourth, as in the mesorhine races. The jaws are not particularly prominent, nor the lips developed and everted to anything like the extent of the African negro, scarcely more, in fact, than in most Malays. The chin appears rounded and well formed.

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* "Étude sur les Mincopies," 'Revue d'Anthropologie,' tome i. p. 37, 1872.

† 'Proc. Asiat. Soc. Bengal,' June, 1870, p. 153.

‡ 'Journal of the Anthropological Institute,' vol. iv. p. 457.

These characters are taken, as I have said, only from the inspection of photographs, but they are strongly confirmed by examination of the actual crania. The number of specimens available till the last few years were very few, but now is rapidly increasing, and I hope in a short time, through the kind assistance of Dr. J. Dougall, Surgeon-Major, H.M. Madras Army, Senior Medical Officer at Port Blair, to have materials enough to draw up a complete account of their osteology. At present I will only indicate some of the more important points regarding the cranium, founded on the examination of nine specimens, two belonging to the Middlesex Hospital Museum,* one in the Army Medical Museum at Netley, and six in the Museum of the College of Surgeons. Of these, five are males and four are females. As the numbers are so nearly equal, and the differences between them are not great, they may be taken together in the following average measurements. In accordance with the diminutive size of the race (for the men are said to average somewhat under 5 feet, and the women less), the crania are of very small general size, the average circumference being 471 centimetres, or 18·5 inches, and the average capacity 1184 cubic centimetres = 72 cubic inches, the highest being 1280 = 78, the lowest 1100 = 67. The antero-posterior diameter averages 164 centimetres, or 6·5 inches. The breadth of the skull in the parietal region considerably exceeds the Tasmanian, giving a latitudinal index which varies in the nine skulls under consideration between 77 (which is quite exceptional and perhaps somewhat malformed) and 85, the average being 81·7. This is corroborated by the measurements of four skulls of Mincopies (as the Andamanese are sometimes called) in the collection of Dr. Barnard Davis, the average of the indices of which is 81; so we have here a truly brachycephalic race. Exactly contrary to what obtains among the Melanesians, the height is in every case less than the breadth, the average altitudinal index being 77·7. This relation is more due to the breadth being excessive than to the skull being low in proportion to length, as it will be seen that the last-named index is higher than that of any other of the races we have hitherto considered. The general contour of the cranium is more rounded and "well filled" than in any of those races. The forehead is flat, the glabella very little developed, and with no marked depression beneath. The nasal bones are straight and tolerably well formed, the aperture of moderate width, the nasal index varying between 47 and 53, the average being 50·6, so that they are distinctly mesorhine. The orbits of the males and females appear to differ nearly as much as in the Tasmanians, the index of the former being 85·6, of the latter 90·7. Some few of the crania show a considerable amount of alveolar prognathism, but generally much less than in any of the other black races. The average is only 100·3, so they may be considered as a race to be mesognathous. The whole profile of the face, from the middle of the forehead to the alveolar margin, is remarkably straight.

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* These were described by Mr. Busk, in 'Trans. Ethnol. Soc.' June, 1865.

People of small stature, with dark skins, round heads, and curly hair, apparently allied to the Andamanese, have been found in either a pure or mixed state in the interior of the Malay Peninsula (where they are called Semangs), in several of the Philippine Islands (called Aetas), in Formosa, and even as far north as the Japanese island of Kiou-siou. Their range appears formerly to have been more extensive, as they are supposed to have contributed something to the very mixed population of the mainland of Southern India, and their influence may be traced without much doubt into New Guinea, and even Borneo. All these indications appear to show that the present scattered and isolated Negrito populations, chiefly inhabiting comparatively inaccessible regions in the forest-clad mountain ranges in the interior of the islands, are the remnants of a race which once spread over a wide area of south-eastern Asia, but have been gradually dispossessed of their territory by the encroachments of other races, especially the Malays, of whom I must speak next. The whole of the evidence which has at present been collected on this subject will be found in the great work 'Crania Ethnica' of Quatrefages and Hamy, now in the course of publication.

The Malays at present occupy the southern half of the Malay Peninsula, and almost the whole of the Archipelago which is not still in possession of the darker, frizzled-haired people already spoken of. Though a totally distinct race, and when pure presenting most opposite physical characteristics to the latter, a great mixture has occurred at many points where they inhabit common ground, and it is often difficult to determine which element prevails most strongly in some of the islands near the junction of the territory mainly inhabited by each. I should also mention that there is some evidence of the existence of a third race, in the island of Gilolo and elsewhere, which appears to possess the characters of neither Malay, Negrito, nor Melanesian, or such as would probably be derived from a blending of either; but very little is at present known about them.*

Mr. A. R. Wallace, whose great opportunities of studying the appearance and character of the Malay race are so well known, has given the following graphic description of them:—†

"The true Malay race, as distinguished from others who have merely a Malay element in their language, present a considerable uniformity of physical and mental characteristics, while there are very great differences of civilization and of language. They consist of four great and a few minor semi-civilized tribes, and a number of others who may be termed savages."

1. Malays proper, inhabiting the Malay Peninsula, and almost all the coast regions of Borneo and Sumatra.

2. The Javanese, Java, part of Sumatra, Madura, Bali, and Lombock.

3. The Bugis, Celebes.

4. The Tagalas, the Philippine Islands.

The savage Malays are the Dyaks of Borneo, the Battaks and other wild tribes of Sumatra, the Jakuns of the Malay Peninsula, &c.

"The colour of all these varied tribes is a light reddish-brown, with more or less of an olive tinge, not varying in any important degree over an extent of country as large as all Southern Europe. The hair is equally constant, being invariably black and straight, and of a rather coarse texture, so that any light tint, or any wave or curl in it, is an almost certain proof of the admixture of some foreign blood. The face is nearly destitute of beard, and the breast and limbs are free from hair. The stature is tolerably equal, and is always considerably below that of the average European; the body is robust, the breast well developed, the feet small, thick and short, the hands small and rather delicate. The face is a little broad, and inclined to be flat; the forehead is rather rounded, the brows low, the eyes black, and very slightly oblique; the nose is rather small, not prominent, but straight and well shaped, the apex a little rounded, the nostrils broad and slightly exposed; the cheek bones are rather prominent; the mouth large, the lips broad and well cut, but not protruding; the chin round and well formed.

"In this description there seems little to object to on the score of beauty, and yet, on the whole, the Malays are certainly not handsome. In youth, however, they are often very good-looking, and many of the boys and girls, up to twelve or fifteen years of age, are very pleasing, and some have countenances which are, in their way, almost perfect. I am inclined to think they lose much of their good looks by bad habits and irregular living. At a very early age they chew betel and tobacco almost incessantly; they suffer much want and exposure in their fishing and other excursions; their lives are often passed in alternate starvation and feasting, idleness and excessive labour—and this naturally produces premature old age and harshness of features.

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* See Hamy, "Les Alfourous de Gilolo," 'Bulletin de la Société de Géographie,' Mai, 1877.

† 'The Malay Archipelago,' vol. ii. p. 270.

"The Malayan race, as a whole, undoubtedly very closely resembles the East Asian population from Siam to Mandchouria. I was much struck with this, when in the island of Bali I saw Chinese traders who had adopted the costume of that country, and who could then hardly be distinguished from Malays; and, on the other hand, I have seen natives of Java, who, as far as physiognomy was concerned, would pass very well for Chinese.

"It appears, therefore, that whether we consider their physical conformation, their moral characteristics, or their intellectual capacities, the Malay and Papuan [i. e. Melanesian] races offer remarkable differences and striking contrasts. The Malay is of short stature, brown-skinned, straight-haired, beardless, and smooth-bodied. The Papuan is taller, is black-skinned, frizzly-haired, bearded, and hairy-bodied. The former is broad-faced, has a small nose, and flat eyebrows; the latter is long-faced, has a large and prominent nose and projecting eyebrows. The Malay is bashful, cold, undemonstrative, and quiet; the Papuan is bold, impetuous, excitable, and noisy. The former is grave, and seldom laughs; the latter is joyous, and laughter-loving—the one conceals his emotions, the other displays them."

There is certainly no very great uniformity in the characters of the skulls in our collections which are said to belong to Malays. But at present craniology is labouring under a great disadvantage, owing to paucity of materials and want of accuracy in the indications as to the precise origin of the specimens with which we have to work, and hence is open to the criticisms which Wallace and others have bestowed upon it, as in the work just quoted. If we group in one category a varied series of skulls from the Malay Islands, which may be composed in greater or less proportion of true Malays, of Negritos, of Melanesians, of Chinese, Spanish, Dutch, and even English, we shall have much difficulty in assigning to them any common distinctive characters. Endeavouring as much as possible to avoid this source of confusion, I have selected seven male skulls, which appear to me to be characteristic of the Malay race in its purest form, and from them have taken the following averages. In general conformation they present as great a contrast to the Papuan or Melanesian type, as has been noted in the external and mental qualities of the respective races. On the other hand, their resemblance to the Negrito skulls is very singular: except in size, many of the Malay and the Andamanese skulls are wonderfully alike.[*] In capacity these skulls average 1424 cubic centimetres, or 86·9 cubic inches, or considerably higher than those of any of the races hitherto spoken of, and nearly equal to that of the Italians. They are all more or less short and round, the average latitudinal index being 81·4, so that they are equally brachycephalic with the Andamanese. The height is in every case less than the width, the average index being 76·4. The sutures are generally very complex; the ptereon in no case shows a union of the squamosal and frontal bones so common in the Melanesian; the forehead is flat and smooth, without projection of the glabella or supraciliary ridges; the nasal bones are flat and straight, the aperture mesorhine, the average index being 50·5; the orbits are fairly round, the average index being 90 (megaseme); the malar bones are large and prominent, the lower part especially projecting forward, quite differently from that of the Australian and Melanesian; the outer margin of the orbit is placed on a much more forward level than in those races, causing the whole face to be broader and flatter. This is a character which allies the Malays with the Mongolian people of the continent of Asia, in whom it is very strongly pronounced. The face is rarely prognathous, usually the reverse, the average index in these examples being 98·2; so that, taken together, they come into the mesognathous category. The palate is short and round, and the teeth small. Of the remainder of the skeleton I have nothing of any value to add from my own observation.

The last, and perhaps in some respects the most interesting and important, of the races of which I shall have to speak this evening is the one which has been called Malayo-Polynesian, Brown Polynesian, Mahori, and simply Polynesian. It is the one which, with, as before indicated, a certain proportion of admixture of the Melanesian race, and with considerable local variations, forms the native population of all the remaining islands scattered over the vast area of the Pacific Ocean. These islands, roughly speaking, form a triangle, with the Sandwich or Hawaiian group, Easter Island, and New Zealand at the three corners, at a distance of 5000 miles apart. Notwithstanding the apparent isolation of many of these, mere little specks as it were in an illimitable expanse of ocean, the greater number of them were when first discovered by Europeans in the last century inhabited; and what is more remarkable, inhabited by people having a great similarity in appearance, in social customs, and in language, so much so that no competent observer who has studied them closely appears to doubt that they must have had a common origin, and that whatever diversities they may now present must be due to local conditions or admixture with other races. The carefully preserved traditions of the people all tend to the same conclusion. The researches of Hale, attached to Commodore Wilkes' exploring expedition, followed out most ably by Quatrefages,* have even led to the construction of a map, on which are indicated the routes by which the different islands have been peopled, and the dates at which the various emigrations took place have been approximately settled from computations based upon the genealogies of the reigning chiefs. Although much of this is of course mere conjecture, it has only been arrived at after a very full and careful collection of traditions, language, customs, and physical characters of the inhabitants of a large number of the islands, and it forms a valuable basis for future researches into the subject. According to the view of Hale, adopted and modified by Quatrefages, the Polynesians came originally from the Malay Archipelago. The island of Borou is fixed upon by the last-named anthropologist for their last point of departure, though their earlier home may have been somewhere in the mainland of Asia, where probably the kindred race from which the modern Malays are derived was also developed. They proceeded eastward, passing to the north of New Guinea and the Salomon Islands, which they probably found already inhabited by the black population, and on which they could not effect a landing, and settled in the Samoan and Tongan Islands, where their descendants still exhibit the purest type of the race. These islands, and especially Savaii, or Havaii, as it is called in all other Polynesian dialects except the Samoan which alone pronounces the sibilant, became centres, as the population increased, for emigration, which, as these people, like the Malays, are able navigators, was readily accomplished. This accounts for the name Hawaii recurring as the native designation of the Sandwich Islands, and for the general tradition in New Zealand and elsewhere of the ancestors of the present inhabitants having come from an island of that name.† For the facts which have been collected bearing upon the details of these migrations, I must refer to the works just mentioned.

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[*] Wallace has already remarked that the Negritos "in most important chara ters differ more from the Papuan than they do from the Malay." Op. cit. v p. 278.

The social condition of the Polynesians when first discovered was somewhat in advance of that of the Melanesians. They lived in villages composed of large, airy, thatched houses, cultivated bread-fruit, cocoa-nuts, plantains, yams, shaddocks, sweet potatoes, and sugar-cane. As domestic animals, they kept pigs, fowls, and dogs. They mostly wore some kind of clothing, generally made of a kind of cloth manufactured from the inner bark of a tree. The practice of very elaborate and artistic tattooing of the skin prevailed extensively. They had no metals, and generally no pottery, but used stone axes, shells, gourds, &c., for domestic purposes. Their weapons were bows, spears, and clubs. Their large and finely built canoes excited the admiration of so good a judge as Cook, and their skill in handling them gained for one group the title of "Navigator Islands" from the French Admiral Bougainville. They were governed by chiefs, and had an hereditary aristocracy, who preserved their pedigrees with great care, and they possessed many complex social customs, among which that of the "taboo" was one of the most influential in its effects on their daily life.

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* 'Les Polynésiens et leur Migrations,' Paris, 1866.

† In Cook's account of his first visit to New Zealand (in 1770) he says:—"Having now given the best account in my power of the customs and opinions of the inhabitants of New Zealand, with their boats, nets, furniture, and dress, I shall only remark that the similitude between these particulars here and in the South Sea Islands [i. e. the Society Islands] is a very strong proof that the inhabitants have the same origin; and that the common ancestors of both were natives of the same country. They have both a tradition that their ancestors, at a very remote period of time, came from another country, and, according to the tradition of both, that the name of that country was *Heawije*; but the similitude of the language seems to put the matter out of doubt." 'Voyages by Hawkesworth,' vol. iii. p. 473, 1773. This, it must be observed, was written before the discovery of the North Pacific Hawaii (Cook's Owyhee), or the Samoan Savaii or Havaii.

The agreeable and courteous manners of these people, and their docility and generosity, always impress those who come in contact with them, although when excited, as in the not infrequent wars among themselves, they are capable of great cruelty, and human sacrifice and cannibalism prevailed extensively in many of the islands. Indolence, and a considerable laxity in several points of social morality, are also among their failings. As is well known, they have shown a great aptitude to adapt themselves to the external usages at least of European civilization, and the primitive, picturesque, if barbarous mode of life is everywhere rapidly giving way to an imitation of English or American institutions, dress, and customs. Hawaii, which exactly one hundred years ago came for the first time in contact with European influence, by its accidental discovery by Cook on his way to explore the north-west passage by Behring's Straits, has now, though still under native rule, its constitutional government by king, lords, and commons, its churches, its schools, its newspapers, and, as announced within the last few days, its railway.

With the disappearance of the ancient customs, strange to say, the people are themselves disappearing. All accounts tell us of a steady diminution of the population, not only in islands where, as in New Zealand, Europeans have invaded and practically annexed the land, and so curtailed the means of subsistence of the natives, but even in others where the original independence has been retained, the introduction of European institutions and customs, good and bad, and especially of diseases before unknown in those seas, appear to have a blighting effect upon the vitality of the islanders. We all remember how virulently destructive was measles, accidentally introduced among our new fellow-subjects in the Fiji Islands a very few years ago; and pulmonary consumption, apparently unknown in former times, now occasions an immense mortality. The cause of the rapid diminution of the Polynesian population is, however, too large and complex a question to be discussed here; the fact is, however, well attested. As an example, in the Hawaiian Islands, leaving out of the question the probably exaggerated estimate of Cook, since a regular census has been established, the population has diminished from 130,000 in 1832 to 60,000 at the present time.

Nearly all who have had personal opportunities of observation, agree that the inhabitants of the Samoan or Navigator Islands, and of the neighbouring Tonga or Friendly Islands of Cook, may be looked upon as the most typical representatives of the Polynesian race; and I shall therefore speak of their physical characters first. They certainly present the greatest contrast to the Melanesians, and perhaps the greatest resemblance to the Malays; indeed, the Rev. S. J. Whitmee, who lived many years in Samoa, and has studied the natives with great care, says, in an interesting article in the 'Contemporary Review' for February, 1873, that morally, intellectually, and physically, the description given by Wallace of the Malays (quoted above) applies exactly to the Samoans, with the only difference that the latter are a people of much larger stature, a circumstance which he attributes to their more abundant supply of food. Commodore Wilkes thus describes the Samoans:—"The average height of the men is 5 feet 10 inches, and some of the chiefs, whose limbs are well rounded, would be called fine-looking men in any part of the world. Their features are not in general prominent, but are well marked and distinct, and are all referable to a common type. The nose is short, and wide at the base; the mouth large, and well filled with large and white teeth, with full and well-turned lips; the eyes black, and often large and bright; the forehead narrow and high, and the cheek bones prominent. It was observed that some of them had the eyes turned up at the outer corner, like the Chinese. Of beard they have but little, but their hair is strong, straight, and very black. The general form of the skull is broad and short, and it is highest near the crown." The colour of the skin, as in other Polynesians, is yellowish or light brown; sometimes as light as that of Southern Europeans, but varying somewhat according to the habitual exposure to the atmosphere and sun and other causes.

The Museum of the College contains, unfortunately, only five crania of Central Polynesians on which to found a description of the race characters; but they agree so well in all their principal features, that I think it probable that they are fair specimens of the type. Of these three are Samoan, one Tongan, and one from St. Augustine's Isle, in the Ellice group. They are all adult males.

Their average capacity is 1420 cubic centimetres, or 86·7 cubic inches. They are all round skulls (see Fig. 4), the indices of breadth varying between 77 and 88, the average being 82·2. The height is either equal or less than the breadth in each case, though they may generally be described as high skulls, the average index being 77·8. In no case does the squamosal meet or even approach near the frontal, and none has epipteric bones. They are all phænozygous, but very slightly so. The forehead is flat, the glabella not greatly developed, the face long and straight; the nasal aperture narrow, the nasal index varying between 39·3 and 46·3, the average being 44·3; the orbits round and high, the average index being 92·8. The jaws in three cases are mesognathous with indices of 99, 99, and 100 respectively; but one is remarkably orthognathous, with an index of only 92; this may, however, be an exceptional case: the other cannot be measured. The malar bones are greatly developed, as in the Malay, presenting a marked contrast to those of the Australian. The size and forward position of these bones are among the features by which they can be most readily distinguished from European skulls. The lower margin of the orbit, and the long axis of the orbital aperture instead of being nearly horizontal, as in the Australian and Melanesian, are inclined downwards at the outer side. The palate is short and semicircular; the teeth not particularly large. It will be seen that in all their essential features these skulls resemble those of the Malays. They are, however, rather larger, and especially higher; the face is longer and somewhat less prognathous, and the nose is narrower.

When a typical Polynesian, as a Samoan cranium, and a typical Melanesian, as one from the New Hebrides, have once been compared and contrasted, they can be recognized at a glance; as they differ quite as much as does the external appearance of the people. Some skulls which were presented many years ago from the island of Lifu (Loyalty group) by Dr. George Bennett, are most characteristically Polynesian—the nasal index alone would separate them from the Melanesians, among which they would be arranged geographically; and this perfectly accords with what we know of the external characters and history of the people to whom they belong, the Loyalty Islands having been colonized, as already mentioned, some 150 years ago from Wallis Island, in Central Polynesia.

It is very interesting to observe the physical evidence of the gradual blending of the two different types in different proportions in regions where, on other grounds, they have been supposed to be intermixed. Thus among skulls brought from the Fiji and most of the neighbouring islands, some are purely Polynesian, some as purely Melanesian, others presenting a combination of characters. Professor Rolleston has been kind enough to allow me to examine a series of crania lately acquired by the University of Oxford from the Central Caroline Islands, inhabited by a population generally admitted to present the characters of a mixed race. These skulls exactly corroborate this view. All the principal cranial indices are intermediate between those proper to the Melanesian and those belonging to the Polynesian type.

The Maoris, or native population of New Zealand, if true Polynesians, as is usually supposed, have departed considerably from the Samoan type. They are darker in colour, have usually more curl in their hair, stronger beards, more prominent and aquiline noses, longer heads (the average cranial index of all that I have measured being 75), rather lower orbits (89), and slightly wider though still leptorhine noses (47). It is possible that this change of type may have taken place simply as the result of three or four centuries' isolation under different conditions, and is therefore something similar to that which appears to be in process among the English in North America; but it is very suggestive of an admixture of Melanesian blood, as every one of the points mentioned form an approximation more or less pronounced towards that race. Although it has been doubted by some authors, it is asserted by others that there are Maori traditions indicating the existence of an aboriginal population, though probably not a numerous one, upon the islands before they were invaded from Rarotonga in the beginning of the fifteenth century. If this were the case they were probably Melanesians, and their absorption into the ranks of the conquering race would cause the physical changes noted above.

However this may be, the present Maoris are a fine race, tall, muscular, and well built; brave, active, and intelligent; "in truth," as Sir David Wedderburn remarks, "as near an approach to the ideal of a 'noble savage' as has ever existed in modern times."* Notwithstanding this, and the great aptitude some of them have shown for adopting the habits of European civilized life, several being already members of the legislative assemblies in New Zealand, and one having at present a seat in the cabinet, their extinction under English influence appears to be coming on as certainly as that of their very inferior Tasmanian and Australian brethren. Their numbers in 1849 were estimated by Sir George Grey at 120,000, ascertained by census in 1858 at 56,000, and in 1874 at 45,470, all but 2000 being inhabitants of the North Island.

The Maoris themselves are guilty of having exterminated in a very ruthless and complete manner, even within the present century, a kindred race. The history of this transaction illustrates very well on a small scale one of the processes by which the ethnology of the Polynesian Islands, and indeed, we may say, of the whole world, has been gradually modified. In 1835 a party of New Zealand natives, finding themselves short of room in their own country, probably either directly or indirectly through the encroachments of European settlers, resolved to seek their fortunes elsewhere, and chartering an English brig, sailed for the Chatham Islands, which were then inhabited by a people called Morioris, a branch of the Polynesian stock, but who having long lived on a small and not very productive island, were inferior in physique and warlike acquirements to the Maoris. The invaders had, therefore, little difficulty in taking possession of the islands, and in a few years had destroyed and eaten most of the original inhabitants, and reduced the rest to slavery. At the present time, according to Mr. E. A. Welch, the islands are inhabited by as varied and motley an assemblage of people as can well be imagined:—Morioris, Maoris, Kanakas, Negroes, Chinese, Spaniards, Portuguese, Danes, Germans, English, Irish, Scotch, Welsh,

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* "Maoris and Kanakas," 'Fortnightly Review,' June, 1877.

Yankees, natives of South America, a Manilla native, a Laplander, a Russian Finn, a half-caste native of New Holland, &c.*

As far as we know, the Eastern Polynesians, the Hawaiians, Marquesans, and Tahitians, do not differ materially in their physical characters from the Samoans. The Marquesans were described by Cook as "without exception the finest race of people in this sea. For fine shape and regular features, they perhaps surpass all other natives." I wish that I could give some details of their cranial conformation from actual observation, which would corroborate or modify the ordinary view of their origin and affinities, but I have not hitherto had an opportunity of doing so. Dr. Barnard Davis has a magnificent series of 116 crania of Kanakas, or natives of the Hawaii or Sandwich Islands, and gives their latitudinal index in his valuable "Thesaurus" at 80, which nearly corresponds with that of the Central Polynesians. It will be interesting to see whether the facial characters also agree.

In bringing to a conclusion this very slight and superficial sketch of the anthropology of an immense region of the earth's surface, I may be expected to say something as to the meaning attached to the word "race," so frequently used. It is better to confess at once that it is extremely indefinite and arbitrary, than to attempt to give an accurate definition. To such groups as I have spoken of under this designation, some anthropologists would apply the term "species." Although this word has not now the definite signification that was formerly attached to it, yet having some experience of its customary use among zoologists, and looking from a purely zoological point of view at the distinguishing characters of these types, races, varieties, or whatever we like to call them, I certainly cannot apply the term species to them in the same sense in which it is ordinarily used in zoology; much less can I believe in the view of the separate and distinct origin of any of these races.

The attempt to form a precise and harmonious scheme of classification is here, as elsewhere, beset with insurmountable difficulties. The endless gradations of distinctive characters can only be most rudely expressed in our artificial systems. We may speak of branches and sub-branches, varieties and sub-varieties, races, species, &c.; but these all are attempts to express degrees of difference connected by endless intermediate conditions, and passing insensibly from one to the other. The first and lowest degree or indication of race characteristics in man, is seen in the inherited traits of various members of a family, or tribe; the next in the more strongly marked and more permanent characters seen in the inhabitants of some district, especially where distinction of language interposes a barrier to communication and intermarriage. Where there are few natural or artificial barriers to mutual and extended intercourse, the characteristics of the different families or tribes become blended, and an absence of uniformity and an irregularity or variety in characters is produced. On the other hand, isolated groups of people tend to uniformity of character in some special direction. Certain peculiarities become in the course of ages more and more pronounced, and the longer time that this process continues, the more permanent and indelible do such peculiarities become, and the more stable is the type produced; exactly as is well known to be the case with the different breeds of domestic animals. This applies not only to the bodily, but also to the intellectual and moral qualities. As the necessity for depending for very existence on the acuteness of the perceptive organs may, in some races, during the course of generations increase the powers of vision and smell, and modify the anatomical structure of the organs by which these senses act, so also may certain mental and moral characteristics in the course of time become stamped more or less firmly upon all members of the race.

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* E. A. Welch and Barnard Davis, "The Morioris or Native Race of the Chatham Islands," 'Journ. Anthrop. Soc.' Nov. 1869, p. 97.

The view of this great question, which appears to be most philosophical as well as most consistent with facts, is that which is intermediate to the two extremes held by certain anthropologists; viz. that of the inherent, radical, and impassable distinction between the different groups of man and that of the perfect equality and identity of all mankind. The theory that an Australian or a Polynesian merely requires to be educated and placed in the same circumstances with a European to be his equal in maintaining his position in those circumstances, or *vice versâ*, ignores the teachings of physiology. He can no more be expected to do so than the foal of a cart-horse, with any amount of training which may be bestowed upon him, can be expected to win the Derby. But just as the cart-horse and the thorough-bred have been developed from one original stock, and will unite and produce intermediate forms, and will without selective breeding revert to some common form, so it is with the races of man, however much this may be disputed by the extreme school of polygenists.

This being the general view of the position of these races, the divisions we make are necessarily arbitrary, and have no natural barrier between them, and no strict equivalency, and the nature of our classification will depend upon what characters we lay most stress upon as indications of affinity. We see already in the subjects of this course very striking characters, those of the hair and of the form of the skull, not correlated, as in the Andamanese and Melanesians. The various groups of men upon the world, whether originally all straight-haired or all frizzly-haired, must, it has been argued, have separated at some time into two primary divisions, the *Leiotrichi* and the *Ulotrichi* of Bory de St. Vincent, a view to which Professor Huxley is inclined to give much weight. This would be a simple starting point for our classification. But on examining other characters, some of which seem equally important, difficulties arise. First let us take the case of the Australians. Their general aspect, all their cranial and skeletal characters ally them so closely to the Melanesians, and

TABULAR OUTLINE OF THE PRINCIPAL PHYSICAL CHARACTERS OF THE RACES TREATED OF IN THIS LECTURE.

Race.	Stature.	Colour.	Hair.	Beard, &c.	CRANIUM.			FACIAL INDICES.		
					Capacity of ♂, in Cubic Inches.	Index of Breadth.	Index of Height.	Gnathic.	Orbital.	Nasal.
AUSTRALIAN	Medium	Blackish	Straight or Waved	Well developed	78	72 Dolicho-cephalic	72	103 Prognathous	♂ 82 ♀ 83 82 Microseme	57 Platyrhine
TASMANIAN	Medium	Blackish	Frizzly	Ditto	80	76 Mesoceph.	73	103 Prognathous	♂ 76 ♀ 85 81 Microseme	57 Platyrhine
MELANESIAN	Variable	Blackish	Frizzly	Ditto	80	71 Dolichoceph.	75	105 Prognathous	♂ 81 Microseme	55 Platyrhine
NEGRITO ..	Very Small	Black	Very Frizzly	?	74	82 Brachyceph.	78	100 Mesognathous	♂ 86 ♀ 91 88 Mesoseme	51 Mesorhine
MALAY ..	Small	Light Brown	Straight	Nearly absent	87	81 Brachyceph.	76	98 Mesognathous	♂ 90 Megaseme	50 Mesorhine
POLYNESIAN	Tall or Medium	Light Brown	Straight	Scanty	87	82 Brachyceph.	78	98 Mesognathous	♂ 93 Megaseme	45 Leptorhine
ITALIAN (added for comparison)	Medium	White	Straight or Waved	Well developed	89	80 Meso. or Brachyceph.	73	97 Orthognathous	♂ 86 ♀ 91 88 Mesoseme	47 Leptorhine

also to the African negroes, that it is extremely difficult to suppose that so many coincidences could have arisen in two stocks which had already diverged so far as to fix permanently the distinctive characteristics of the hair. Again, take the Negritos of the Indo-Malayan Archipelago. Here we have a woolly-haired people, with scarcely any of the osteological and perhaps cerebral characteristics of the other negroid races. The alternative supposition that woolly hair could have originated independently, upon different branches of straight-haired races, is also beset with difficulties. It is clear, however, that setting aside the doctrine of separate creation, one or other of these events must have taken place; but which is the more likely is impossible, in our present state of knowledge, to decide.

Very much still remains to be done with regard to the history of man in the part of the world we have been considering this evening, both in the confirmation or amendment of the truth of these general conclusions, and in the completion of the various details. And now is the time, if ever, when it must be done.

Many of these people have lived in their sea-girt homes, isolated from the rest of mankind, for ages untold, and with probably little or no change in their habits or physical characteristics. Among others, the movements, migrations, and interchange of ideas and customs, and progressive improvements, which have taken place have been of the most partial, slow, and gradual character. But within the lifetime of some still among us, a marvellous transformation has been wrought among them. It is scarcely a hundred years since the veil of darkness and mystery which enshrouded these regions was uplifted, and the very existence of most of the races of which I have been speaking was first revealed to the civilized world. It is only within the present century that the great movement has taken place, the rush of the Anglo-Saxon race into the islands of the Pacific, which is rapidly shaking to the foundations all the old ethnological landmarks, and, with accumulating speed, sweeping away, not only the characteristic customs, traditions, and languages of the people, but even the very people themselves. In another half century, the Australians, the Melanesians, the Maoris, and most of the Polynesians, will have followed the Tasmanians to the grave. We shall well merit the reproach of future generations if we neglect our present opportunities of gathering together every fragment of knowledge that can still be saved, of their languages, customs, social polity, manufactures, and arts. The preservation of tangible evidence of their physical structure is, if possible, still more important; and surely this may be expected of that nation, above all others, which by its commercial enterprise and wide-spread maritime dominion has done, and is doing, far more than any other in effecting this destructive revolution.

[W. H. F.]

GENERAL MONTHLY MEETING,

Monday, June 3, 1878.

WILLIAM BOWMAN, Esq. F.R.S. Manager, in the Chair.

The Lord Viscount Kilcoursie,
R. Faircloth, Esq. F.R.C.S.
J. Lawrence-Hamilton, M.D.
William Ford Stanley, Esq.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Rome—Atti, Serie III. Transunti, Vol. III. Fasc. 5. 4to. 1878.
Agricultural Society, Royal—Journal, Second Series, Vol. XIV. Part 1. 8vo. 1878.
American Academy of Natural Sciences, Philadelphia—Proceedings for 1877. 8vo. 1877.
Asiatic Society, Royal—Journal, New Series, Vol. X. No. 2. 8vo. 1878.
British Architects, Royal Institute of—Sessional Papers, 1878. No. 13. 4to.
Chemical Society—Journal for May, 1878. 8vo.
Editors—American Journal of Science for May, 1878. 8vo.
  Analyst for May, 1878. 8vo.
  Athenæum for May, 1878. 4to.
  Brain: a Journal of Neurology. No. 1. 8vo. 1878.
  Chemical News for May, 1878. 4to.
  Electrician for May 25, 1878. 4to.
  Engineer for May, 1878. fol.
  Horological Journal for May, 1878. 8vo.
  Iron for May, 1878. 4to.
  Journal for Applied Science for May, 1878. fol.
  Nature for May, 1878. 4to.
  Nautical Magazine for May, 1878. 8vo.
  Telegraphic Journal for May, 1878. 8vo.
Franklin Institute—Journal, No. 629. 8vo. 1878.
Geological Institute, Imperial, Vienna—Abhandlungen, Vol. VIII. Heft 2. 4to. 1877.
  Verhandlungen, 1877, Nos. 11-18. 8vo.
  Jahrbuch: 1877, Band XXVII. Nos. 3, 4. 8vo.
Geological Society—Quarterly Journal, No. 134. 8vo. 1878.
Linnean Society—Proceedings, No. 74. 8vo. 1878.
Manchester Geological Society—Transactions, Vol. XIV. Parts 18, 19. 8vo. 1878.
Montpellier, Académie des Sciences—Mémoires, Tome IX. Fasc. 1. 4to. 1877.

Pharmaceutical Society of Great Britain—Journal for May, 1878. 8vo.
Photographic Society—Journal, New Series, Vol. II. No. 7. 8vo. 1878.
Plateau, M. J. Hon. M.R.I. (the Author)—Bibliographie Analytique des Principaux Phénomènes de la Vision. Sections 2, 3. 4to. 1877.
Preussische Akademie der Wissenschaften—Monatsberichte: Feb. 1878. 8vo.
Royal Society of London—Philosophical Transactions for 1877, Part 2. 4to. 1878.
Royal Society of Tasmania—Papers and Proceedings for 1876. 8vo. 1877.
Statistical Society—Journal, Vol. XL. Part 1. 8vo. 1878.
St. Petersburg, Académie des Sciences—Bulletin, Tome XXIV. No. 4. 4to.
Trinity House Corporation—Fog Signals, Part 2. (P 12) fol. 1878.
Tuson, Professor R. (the Editor)—Cooley's Cyclopædia of Practical Receipts, Part 3. 8vo. 1878.
United Service Institution, Royal—Journal, No. 94. 8vo. 1878.
University of London—Calendar for 1878. 12mo. 1878.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1878. Hefte 4, 5. 4to.
Victoria Institute—Journal, No. 46. 8vo. 1878.
Vincent, Charles W. F.R.S.E. F.C.S. (the Editor)—Chemistry Applied to the Arts and Manufactures. Part VI. 8vo. 1878.

WEEKLY EVENING MEETING,

Friday, June 7, 1878.

William Bowman, Esq. F.R.S. in the Chair.

Walter Herries Pollock, Esq. M.A.

Romanticism.

The discourse began by the quotation of a portion of M. Reybaud's humorous description of the enthusiastic reception of Victor Hugo's 'Hernani' by his friends and the stormy opposition of his enemies, when first performed at the Théâtre Français on Feb. 25, 1830. The new romantic school had been gathering strength for some time, having at its head one whose genius fitted him to be a leader of men; while the opposite party, the classicists, were thoroughly disgusted with Hugo's 'Cromwell' and its preface, in which all their rules were violated. Now that 'Hernani' is accounted a classic drama, it is difficult to realize the storm that raged at its first appearance.

The term 'Romanticism' was at first applied to the disregard of the unities of time and place, in which Shakspeare so much differs from Sophocles and the other Greek dramatists; but about 1828 it was also applied to poetry and romances. Hugo asserted as a new discovery that the grotesque is a necessary element of modern poetry; but the mixture of the tragic and the comic, the terrible and the tender, is found also in the ancients, especially in Aristophanes.

In a satirical dialogue, quoted by Mr. Pollock, Alfred de Musset endeavours to prove that no accurate definition of romanticism can be obtained, except that it consists in the employment of a vast number of needless adjectives and nothing else. In fact, neither party could define what they were fighting for. The classicists went so far as to claim Byron as on their side; while their opponents might, had the struggle lasted long enough, have referred to the unbridled grotesque grandeur of Racine. Moreover, classicism abounds in the best plays of the modern romantic school. Reverting again to 'Hernani,' Mr. Pollock said that, in spite of its immense power and beauty, it is one of the most inartistic plays ever written by a great author; its plot consists of a constant reiteration of the same situation, during which the chief characters literally carry their lives in their hands, and offer them to each other as people in old comedies offer snuff; and he contrasted its method with that of the 'Phèdre' of Racine.

The origin of the Romantic school is traced to Madame de Stael; but the movement was mainly carried on by Chateaubriand, Casimir

Delavigne, and others, greatly influenced by the study of Shakspeare, Goethe, Scott, and similar writers.

The romanticists were very successful in demonstrating that it is quite possible to produce a great poem, play, picture, or opera without following any set of cut-and-dried rules; although at first they were led into extravagances often ludicrous.

De Mussett, defending the classic writers, asserts that their rules were really the result of long consideration of the best means of obtaining the highest results of art, and that young writers should return to the stately simplicity of the ancients; but, as Mr. Pollock showed, De Musset is not consistent with himself. His own most successful dramas are all cast in what may be termed the "middle style." He did not regard the unities as invincible; yet one of his finest pieces, 'Les Caprices de Marianne,' is so arranged that it may be played in accordance with them, and was so a few weeks ago. The time seems to have come for a new romantic school to arise.

Mr. Pollock then adverted to the predominance of the classic school in the seventeenth and eighteenth centuries in our own country, when Shahspeare was superseded by Dryden, Rowe, Otway, and Young, and to the reaction begun by Garrick and ably sustained by the Kembles and Kean. In reference to these Talma lamented that he never had a real part. He asked for Shakspeare, and they gave him Ducis! It was the desire to have at least some touch of nature that led him to play Marius bare-legged.

Taking Romanticism to mean something which varies a monotonous style, Mr. Pollock said that some infusion of it would be good for us now. We have in the art of painting a kind of romanticism; but, like that of the French, it consists, just as much as classicism does, in returning to models which were thought to be superseded long ago.

WEEKLY EVENING MEETING,

Friday, June 14, 1878.

GEORGE BUSK, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

PROFESSOR JAMES DEWAR, M.A. F.R.S.

The Liquefaction of Gases.

DALTON, twenty-two years before the discovery of the liquefaction of the gases, commences his essay 'On the Force of Steam or Vapour from Water and various other Liquids, both in a Vacuum and in Air,' with the following marvellous anticipation of subsequent research: "There can scarcely be a doubt entertained respecting the reducibility of all elastic fluids of whatever kind into liquids; and we ought not to despair of effecting it in low temperatures, and by strong pressure exerted upon the unmixed gases." * The same ideas are reiterated in Dalton's 'New System of Chemical Philosophy,' which appeared in 1808, yet no definite proof of the accuracy of his ideas regarding the relation of the gaseous and liquid states of matter were forthcoming until 1823.

The first information regarding the liquefaction of a gas is found in a letter sent by Faraday to Dr. Paris, the biographer of Sir Humphry Davy.

"DEAR SIR,

"The oil you noticed yesterday turns out to be liquid chlorine.

"Yours faithfully,

"MICHAEL FARADAY."

The letter is not dated, but we know from Dr. Paris that it must have been the 6th of March, 1823.

Faraday was then engaged in an investigation on the hydrate of chlorine. Davy suggested the examination of the action of heat in a closed vessel, as likely to yield interesting results, without stating, however, what he anticipated would occur. The letter addressed to Dr. Paris has reference to the result of this experiment. The solid hydrate of chlorine on being heated in a closed vessel, separated into water and a dense yellow oil, which Faraday proved to be pure liquid chlorine. Davy saw the importance of the discovery, and liquefied hydrochloric acid by the pressure of its own vapour, produced by generating it in a closed vessel.

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* 'Literary and Philosophical Society of Manchester,' vol. v. 1802.

Davy also succeeded in liquefying gases by a method which, at first view, appears very paradoxical—by the application of heat! The method consists in placing them in one leg of a bent sealed tube, confined by mercury, and applying heat to ether, or alcohol, or water, in the other end. In this manner, by the pressure of the vapour of ether, he liquefied prussic acid and sulphurous acid gas; which gases, on being reproduced, occasioned cold. There can be little doubt, he thinks, that these general facts of the condensation of the gases will have many practical applications. "They afford means of producing great diminutions of temperature, by the rapidity with which large quantities of liquids may be rendered aëriform; and as compression occasions similar effects to cold, in preventing the formation of elastic substances, there is great reason to believe that it may be successfully employed for the preservation of animal and vegetable substances for the purposes of food."

Faraday continued the experiments, and succeeded in liquefying many gases by the pressure caused by their continuous formation in a limited space, either through the ordinary reactions used for producing them, or by applying heat to some easily decomposed compound. By observing the volume of the liquid gas as compared with the same in the gaseous state the relative specific gravities could be determined, and by introducing small air manometers, the amount of pressure in atmospheres required to condense the various gases, and the influence of temperature on the pressure, could be observed. This series of experiments was completed in 1823.

Thilorier, in 1835, devised an apparatus to produce liquid carbonic acid on a large scale, and made the great discovery that the fluid ejected into air produced the solid in the form of snow. This substance while evaporating gave a temperature which he estimated about minus 100° C. The fluid gas was soluble in all proportions in ether, turpentine and bisulphide of carbon. It was four times more dilatable than air, and every additional degree of temperature added one atmosphere of pressure to the tension of the gas. The density of the vapour increased in a much greater proportion than the pressure, so that the law of Mariotte was no longer applicable to such a highly compressed condition of the gas.

Mitchell, in 1839, found the true temperature of the solid while evaporating in air was minus 78°, but mixed with ether and vaporized in vacuo it was reduced to minus 99°. With this powerful means of reducing temperature he solidified sulphurous acid.

In order to demonstrate the exceptional temperatures we have to deal with, a thermo-junction of iron-copper has the advantage over a fluid thermometer of having a very small mass and great sensibility, so that the length of a degree on our scale is easily observed. This junction placed in ice marks 0°. When placed in solid carbonic acid it maintains a fixed temperature of minus 80°, a point on the thermometer scale as far below the freezing point as the boiling point of spirits of wine is above it. If ether is added to the solid acid, a fluid which does not freeze, and which has the remarkable property of dissolving the carbonic acid, the same low temperature is kept up. The ether carbonic acid bath acts more rapidly, from the immersed body being wetted, and thus brought into close contact with the fluid. This thermometer is far more delicate than either an alcohol or bisulphide of carbon one, and yet it records a constant temperature, and why? The reason is, solid carbonic acid is actually boiling; this solution in ether is giving off continuously bubbles of gas like a boiling fluid. This is more readily seen if the snow is compressed into the form of ice by strong hydraulic pressure. This cylinder of clear carbonic acid ice weighs nearly half an ounce, and continues to give off gas when immersed in ether, as if it were a piece of marble dissolving in an acid. In this state it is one and a half times denser than water; and although the mass is at a temperature at least 20° below that of the Arctic regions, yet, strange to say, it is not covered with ice when dropped into a mass of water. It is in reality coated with a layer of gas constantly renewed, and thus the water never gets into actual contact. The solid is in a condition similar to that of the spheroidal state of liquids.

Another liquid gas that boils at a still lower temperature is nitrous oxide, or laughing gas. The temperature of boiling nitrous oxide freely exposed to the air is, as you observe, about 10° lower than that of carbonic acid. A temperature of minus 90° is thus the lowest we can reach through vaporization of a conveniently liquefiable gas, freely exposed to the atmosphere. As the boiling point, however, is dependent wholly upon the pressure of the gaseous atmosphere, if the pressure is removed from the surface of the liquid with a greater rapidity than it can accumulate through the generation of gas, much lower temperatures may be commanded. The lowest temperature recorded by Faraday was minus 110, and the experiment just made proves this is about the limit to be reached with his air-pump. The experiments of Cagniard de-la-Tour and Thilorier opened up new ideas and methods for further investigation, and Faraday attacked the subject for the second time in 1844. By the combined action of pressure, and the low temperature of the carbonic acid ether bath in vacuo, he now succeeded in liquefying all the gases with the exception of six, viz. oxygen, hydrogen, nitrogen, carbonic oxide, nitric oxide, and marsh gas, and obtained many of them in the form of solids. The most valuable and laborious part of the research was the determination of the tensions of the liquid gases. He was the first to show the great importance to be attached to these constants in defining the purity of gases. Thus he discovered that phosphuretted hydrogen, nitrous oxide, and olefiant gas, however carefully prepared, could by this method be proved to contain impurities. The experiments of Cagniard de-la-Tour induced him to conclude that no amount of pressure would liquefy the permanent gases unless the temperature was below some definite point of the thermometric scale. In fact he had a very clear idea of what is now called the critical point.

The following extracts from the investigation of 1844 are worthy of grave attention. They prove the wonderful accuracy of scientific prophecy and display the working of a mind, full of subtle powers of divination into nature's secrets:—

On the Liquefaction and Solidification of Bodies generally existing as Gases.[*]

"The experiments formerly made on the liquefaction of gases, and the results which from time to time have been added to this branch of knowledge, especially by M. Thilorier, have left a constant desire on my mind to renew the investigation. This, with considerations arising out of the apparent simplicity and unity of the molecular constitution of all bodies when in the gaseous or vaporous state, which may be expected, according to the indications given by the experiments of M. Cagniard de-la-Tour, to pass by some simple law into their liquid state, and also the hope of seeing nitrogen, oxygen, and hydrogen, either as liquid or solid bodies, and the latter probably as a metal, have lately induced me to make many experiments on the subject; and though my success has not been equal to my desire, still I hope some of the results obtained, and the means of obtaining them, may have an interest for the Royal Society; more especially as the applications of the latter may be carried much further than I as yet have had an opportunity of applying them.

"But as my hopes of any success beyond that heretofore obtained depended more upon depression of temperature than on the pressure which I could employ in these tubes, I endeavoured to obtain a still greater degree of cold. There are, in fact, some results producible by cold, which no pressure may be able to effect.

"Thus solidification has not as yet been conferred on the fluid by any degree of pressure.

"Again that beautiful condition which Cagniard de-la-Tour has made known, and which comes on with liquids at a certain heat, may have its point of temperature for some of the bodies to be experimented with, as oxygen, hydrogen, nitrogen, &c., below that belonging to the bath of carbonic acid and ether; and in that case, no pressure which any apparatus could bear would be able to bring them into the liquid or solid state.

"Thus though as yet I have not condensed oxygen, hydrogen, or nitrogen, the original objects of my pursuit, I have added six substances, usually gaseous, to the list of those that could previously be shown in the liquid state, and have reduced seven, including ammonia, nitrous oxide, and sulphuretted hydrogen, into the solid form. And though the numbers expressing tension of vapour cannot (because of the difficulties respecting the use of thermometers and the apparatus generally) be considered as exact, I am in hopes they will assist in developing some general law governing the vaporization of all bodies, and also in illustrating the physical state of gaseous bodies as they are presented to us under ordinary temperature and pressure.

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[*] 'Phil. Trans.,' 1845.

"Whether the same law may be expected to continue when the bodies approach near to the Cagniard de-la-Tour state is doubtful. That state comes on sooner in reference to the pressure required, according as the liquid is lighter and more expansible by heat and its vapour heavier; hence indeed the great reason for its facile assumption by ether.

"But though with ether, alcohol, and water, that substance which is most volatile takes up this state with the lowest pressure, it does not follow that it should always be so; and, in fact, we know that ether takes up this state at a pressure between thirty-seven and thirty-eight atmospheres, whereas muriatic acid, nitrous oxide, carbonic acid, and olefiant gas, which are far more volatile, sustain a higher pressure than this without assuming that peculiar state, and whilst their vapours and liquids are still considerably different from each other. Now whether the curve which expresses the elastic force of the vapour of a given fluid for increasing temperatures continues undisturbed after that fluid has passed the Cagniard de-la-Tour point or not is not known, and therefore it cannot well be anticipated whether the coming on of that state sooner or later with particular bodies, will influence them in relation to the more general law referred to above.

"The law already suggested gives great encouragement to the continuance of those efforts which are directed to the condensation of oxygen, hydrogen, and nitrogen, by the attainment and application of lower temperatures than those yet applied.

"If to reduce carbonic acid from the pressure of two atmospheres to that of one, we require to abstract only about half the number of degrees that is necessary to produce the same effect with sulphurous acid, it is to be expected that a far less abstraction will suffice to produce the same effect with nitrogen or hydrogen, so that further diminution of temperature and improved apparatus for pressure may very well be expected to give us these bodies in the liquid or solid state."

The classical researches of Regnault in 1847, on the compressibility of gases, proved that the permanent gases, with the exception of hydrogen, deviated from Mariotte's law in the same direction as those that were liquefiable, although to a much smaller amount. If Mariotte's law represents the perfect gaseous state, then hydrogen is a gas that shows greater resistance to alteration of volume than would result from this law. Natterer, in 1854, condensed hydrogen, oxygen, and nitrogen to a pressure of 2700 atmospheres without any change of state being observed. His experiments show that oxygen and nitrogen under great compression, behave like hydrogen, that is, become less and less compressible.

Andrews, in 1861, subjected the six gases that resisted the efforts of Faraday when cooled to the temperature of the carbonic acid ether bath to a pressure of at least 500 atmospheres without producing any change of state. During the course of this investigation Andrews observed that liquid carbonic acid raised to a temperature of 31° lost the sharp concave surface of demarcation between the liquid and gas and at last disappeared. The space was now occupied by a homogeneous fluid, which exhibited, when the pressure was suddenly diminished, or temperature slightly lowered, a peculiar appearance of moving or flickering striæ throughout its entire mass, owing to great alterations of density. At temperatures above 31° apparent liquefaction or separation into two distinct forms of matter could not be effected even when the pressure reached 400 atmospheres. This limiting temperature of the liquid state Andrews calls the "critical point." He has been engaged for the last twelve years in completing his researches on the gaseous state, which for accuracy and elegance can only be equalled by the work of Regnault.

Recent experiments resulting in the liquefaction of the permanent gases have been made simultaneously by M. L. Cailletet, of Paris, and M. R. Pictet, of Geneva. Each had large experimental resources and facility for conducting such investigations.

Both experimenters used the same ingenious method of reaching temperatures far below that of the carbonic acid bath in vacuo, by allowing the gases cooled to the above temperature and highly compressed, to expand suddenly. This expansion involves a rapid absorption of heat, and this is chiefly taken from the molecules of the gas forcing a portion to pass into the fluid or solid state.

Cailletet's apparatus is a modified form of that employed by Andrews in his great research on the continuity of the gaseous and liquid states of matter, and will be readily understood on seeing it in operation.*

Pictet's experiments were conducted on a manufacturing scale. A sulphurous acid ice machine cooled carbonic acid or nitrous oxide to a temperature of minus 65°, so that a pressure of from four to six atmospheres is all that is required to cause liquefaction. A system of compressing and exhausting pumps, worked at the rate of one hundred revolutions per minute by a steam engine, thus produce about 16 lbs. of liquid carbonic acid per hour. Vaporized in the very perfect vacua he can command, a temperature of minus 130° may be kept up for any length of time. The operations are so arranged in cycle that the same carbonic acid or nitrous oxide may be used again and again. [This photograph shows the general arrangements in the laboratory.] No pump is used by Pictet to compress the oxygen or hydrogen. The gases are produced by chemical reaction as in Faraday's tubes, only he replaces the glass by a strong tube of copper connected with a large iron bomb, in which by the application of heat the decomposition takes place. The narrow copper tube is cooled to minus 130° or 140° by the method explained, and here the liquefaction takes place. A screw stop-cock at the extremity of the tube allows the liquid gas to be ejected. The pressure of liquid oxygen at minus 138 is at least 273 atmospheres, and its density is a little less than that of water. Hydrogen in the liquid state at a temperature of minus 140° has a pressure of about 320 atmospheres and appears to solidify in the tube when the fluid jet is allowed to escape. The jet of liquid has a steel blue colour.

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* Handsomely presented to the Royal Institution, by Dr. Warren De La Rue, for the purpose of illustrating this lecture.

If oxygen, nitrogen, or air is compressed in Cailletet's apparatus to 250 atmospheres, something like three tons on the square inch, and the pressure suddenly released, there is an instantaneous cloud formed within the tube due to partial liquefaction.

This tube contains the first gas Cailletet succeeded in liquefying, the hydro-carbon called acetylene, which was discovered by an old Assistant of the Royal Institution, and is one of the most important bodies in the whole range of organic chemistry. After it is compressed to about 50 atmospheres a clear colourless fluid will result.

The work of Faraday has been completed; every gas may be forced to appear as a liquid.

[J. D.]

GENERAL MONTHLY MEETING,

Monday, July 1, 1878.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The following Alterations in the Bye-Laws of the Royal Institution, having been duly proposed at the previous Meeting, were passed unanimously:—

In Chapter VI. (*Of the Duties of the Committee of Managers.*)

In Art. 4, line 3, for "*one o'clock P.M.*" substitute "*four o'clock P.M.*"

In Chapter X. (*Of the General Meetings of the Members.*)

In Art. 4, line 5, for "*two o'clock P.M.*" substitute "*five o'clock P.M.*"

The Special Thanks of the Members were given to Warren De la Rue, Esq. D.C.L. F.R.S. for his Valuable Presents of a "High-Resistance Galvanometer" and "Cailletet's Apparatus for the Liquefaction of Gases."

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Indian Government—Memoirs of the Geological Survey: Palæontologia Indica, Ser. II. 3; Ser. X. 3; Ser. XI. 2. fol. 1877-8.

Accademia dei Lincei, Rome—Atti, Serie I. Anno VII. 4to. 1856-62; Serie III. Transunti, Vol. II. Fasc: 6. 4to. 1878.

Memorie: Scienze Fisiche, Matematiche e Naturali, Vol. I.; Scienze Morali, Storiche e Filologiche, Vol. I. 4to. 1877.

American Philosophical Society—Transactions: New Series, Vols. III.-XI. and Vol. XIV. Part 2. 4to. 1830-70. Proceedings, No. 100. 8vo. 1877.

Asiatic Society, Royal, Bombay Branch—Journal, No. 35. 8vo. 1878.

Astronomical Society, Royal—Monthly Notices, Vol. XXXVIII. No. 7. 8vo. 1878.

Author, The—Burlingtonia: Historical Notes about Burlington House and its Neighbourhood. (O 17) 16to. 1878.

Chemical Society—Journal for June, 1878. 8vo.

Editors—American Journal of Science for June, 1878. 8vo.

Analyst for June, 1878. 8vo.

Athenæum for June, 1878. 4to.

Chemical News for June, 1878. 4to.

Engineer for June, 1878. fol.

Horological Journal for June, 1878. 8vo.

Iron for June, 1878. 4to.

Journal for Applied Science for June, 1878. fol.

Nature for June, 1878. 4to.

Nautical Magazine for June, 1878. 8vo.

Telegraphic Journal for June, 1878. 8vo.

Erichsen, J. E. Esq. F.R.S. M.R.I. (the Author)—On Surgical Evidence in Courts of Law. (K 102) 8vo. 1878.

Franklin Institute—Journal, No. 630. 8vo. 1878.

Geographical Society, Royal—Journal, Vol. XLVII. 8vo. 1878.

King, Clarence, Esq. (United States Geologist)—United States Geological Exploration of the Fortieth Parallel, Vol. IV. 4to. 1877.

Linnean Society—Proceedings, Nos. 96, 97. 8vo. 1878.

Mann, Robert James, M.D. M.R.I.—J. L. Lagrange: Mécanique Analytique. Nouvelle Edition. 2 vols. 4to. Paris, 1811-15.

Mechanical Engineers, Institution of—Proceedings, April, 1878. 8vo.

Meteorological Society—Quarterly Journal, No. 26. 8vo. 1878.

Odling, Mrs. (the Author)—Memoir of the late Alfred Smee, F.R.S. by his Daughter. With a Selection from his Miscellaneous Writings. 8vo. 1878.

Pharmaceutical Society of Great Britain—Journal for June, 1878. 8vo.

Photographic Society—Journal, New Series, Vol. II. No. 8. 8vo. 1878.

Plateau, M. J. Hon. M.R.I. (the Author)—Bibliographie Analytique des Principaux Phénomènes de la Vision. Sections 4, 5, 6. 4to. 1877.

Preussische Akademie der Wissenschaften—Monatsberichte: Marz, April, 1878. 8vo.

Rayleigh, The Lord, M.A. F.R.S. M.R.I. (the Author)—The Theory of Sound, Vol. II. 8vo. 1878.

Royal Society of London—Proceedings, No. 187. 1878.

Tuson, Professor R. V. (the Editor)—Cooley's Cyclopædia of Practical Receipts, Part 4. 8vo. 1878.

United Service Institution, Royal—Journal, No. 95. 8vo. 1878.

Zoological Society of London—Transactions, Vol. X. Part 6. 4to. 1878.

Proceedings, 1878, Part 1. 8vo. 1878.

GENERAL MONTHLY MEETING,

Monday, Nov. 4, 1878.

C. **William Siemens**, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

C. T. Denton, Esq.
Miss E. Forster,
Avary William Holmes, Esq.
Hon. Rollo Russell,
Frederick S. Shenstone, Esq.

were *elected* Members of the Royal Institution.

The Managers reported, That at their Meeting this day, they appointed Mr. **Edward Albert Schäfer**, Fullerian Professor of Physiology for three years.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The India Office—C. R. Markham: Memoir of the Indian Surveys. 2nd edition. fol. 1878.
C. Chambers: Meteorology of the Bombay Presidency. 2 vols. 4to and fol. 1878.
British Museum Trustees—Catalogue of Greek Coins: The Seleucid Kings of Syria. 8vo. 1878.
Illustrations of Lepidoptera Heterocerca, Part 2. 4to. 1878.
Catalogue of Cheiroptera. 8vo. 1878.
Guide to Exhibition Rooms. 8vo. 1878.
Guide to Second Vase Room. 12mo. 1878.
Guide to Autograph Letters, &c. 16to. 1878.
Museum of Practical Geology—Catalogue of Library. 8vo. 1878.
The Warden of the Standards—Tables for the Verification of the Standards of Weights and Measures, by Dr. O. J. Broch. 8vo. 1878.
Twelfth Annual Report. 8vo. 1878.
The Prussian Government—Dr. J. F. Julius Schmidt: Chart der Gebirge des Mondes; mit Erläuterungsband. fol. and 4to. 1878.
United States Naval Observatory, Washington—Instructions for Observing the Total Solar Eclipse, 29th July, 1878. (M 8) 4to. 1878.

Actuaries, Institute of—Journal, Nos. 111, 112. 8vo. 1878.
American Academy of Arts and Sciences—Proceedings, Old Series, Vol. VIII. 8vo. 1873. New Series, Vol. V. (Old Series, Vol. XV.) 8vo. 1878.
American Philosophical Society—Catalogue of Library, Part 3. 8vo. 1878.
Proceedings, No. 101. 8vo. 1878.
Antiquaries, Society of—Proceedings, Second Series, Vol. VII. No. 4. 8vo. 1878.
Asiatic Society of Bengal—Journal, Vol. XLVI. Part I. Nos. 2, 3, 4; Part II Nos. 3, 4; Vol. XLVII. Part I. No. 1; Part II. Nos. 1, 2. 8vo. 1877–8.
Proceedings, 1877, Nos. 7–10. 1878, Nos. 1–6. 8vo.

Asiatic Society Royal—Journal, New Series, Vol. X. No. 3. 8vo. 1878.

Astronomical Society, Royal—Monthly Notices, Vol. XXXVIII. Nos. 8, 9. 8vo. 1878.

Barclay, J. G. Esq.—Astronomical Observations at the Observatory of J. G. Barclay, Esq. Leyton, Essex. Part 3 and Vol. IV. 4to. 1873–8.

Basel Naturforschende Gesellschaft—Theil VI. Heft 4. 8vo. 1878.

Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1878, Heft 1, 2. 8vo.
Abhandlungen. Band XIII. Abth. 1. 4to. 1878.
Dr. C. W. von Gümbel: Rede auf der Geognostische Durchforschung Bayerns. 4to. 1877.
Almanach für 1878. 16to.

Boston Society of Natural History—Memoirs, Vol. II. Part I. Nos. 2, 3; Part II. No. 1; Part IV. No. 6. 4to. 1872.
Proceedings, Vol. XIX. Parts 1, 2. 8vo. 1877.

British Architects, Royal Institute of—Sessional Papers, 1878. Nos. 14–17. 4to.

Canada Meteorological Office—Reports for 1877. 8vo. 1878.

Chamberlin, T. C. Esq.—Geology of Wisconsin. Vol. II. with Atlas. 1877.

Chemical Society—Journal for July–Oct. 1878. 8vo.

Civil Engineers' Institution—Minutes of Proceedings, Vol. LII. LIII. 8vo. 1878.

Connecticut Academy of Arts and Sciences—Transactions: Vol. III. Part 2; Vol. IV. Part 1. 8vo. 1877–8.

Coutts, J. (the Author)—The Kingdom of Force. 16to. 1878.

Dax: Société de Borda—Bulletins, 2^e Série, Troisième Année; Trimestre 1–3. 8vo. Dax, 1878.

Editors—American Journal of Science for July–Oct. 1878. 8vo.
Analyst for July–Oct. 1878. 8vo.
Athenæum for July–Oct. 1878. 4to.
Brain: a Journal of Neurology, No. 2. 8vo. 1878.
Chemical News for July–Oct. 1878. 4to.
Engineer for July–Oct. 1878. fol.
Horological Journal for July–Oct. 1878. 8vo.
Iron for July–Oct. 1878. 4to.
Journal for Applied Science for July–Oct. 1878. fol.
Nature for July–Oct. 1878. 4to.
Telegraphic Journal for July–Oct. 1878. 8vo.

Franklin Institute—Journal, Nos. 631, 632, 633, 634. 8vo. 1878.

French J. G. (the Author)—The Nature of Cholera Investigated. 2nd edition. 8vo. 1854.

Geographical Society, Royal—Proceedings, 1878, Nos. 4, 5, 6. 8vo.

Geological Institute, Imperial, Vienna—Verhandlungen, 1878. Nos. 1–10. 8vo.
Jahrbuch: 1878. Band XXVIII. Nos. 1, 2. 8vo.

Geological Society—Quarterly Journal, No. 135. 8vo. 1878.

Geological Survey of India—Records, Vol. XI. Parts 2, 3. 8vo. 1878.

Gladstone, Dr. J. H. F.R.S. M.R.I. (the Author)—Address to the Chemical Society, 30th March, 1878. (Journal of Chemical Society, 1878.) 8vo.

Glasgow Philosophical Society—Proceedings, Vol. XI. No. 1. 8vo. 1878.

Harlem, Société Hollandaise des Sciences—Archives Néerlandaises. Tome XIII. Liv. 1, 2, 3. 8vo. 1878.

Henry, Dr. James (Trustees of)—Æneidea or Critical, Exegetical, and Æsthetical Remarks on the Æneis, by James Henry. 2 vols. 8vo. 1877–8.

Hinrichs, Dr. G. (the Author)—Iowa Weather Report for 1877. 8vo, and Charts.

Hull Literary and Philosophical Society—Annual Report, 1877–8. 8vo. 1878.

Irish Academy, Royal—Transactions: Vol. XXV. Science, No. 20; Vol. XXVI. Science, Nos. 6–16; Vol. XXVII. Polite Literature, No. 1. 4to. 1875–7.
Proceedings, Series II.: Vol. I. No. 12; Vol. II. No. 7; Vol. III. No. 1. 8vo. 1877.

Iron and Steel Institute—Journal, 1878, No. 1. 8vo. 1878.

Linnean Society—Proceedings, No. 75, 98. 8vo. 1878.

Transactions: Second Series, Botany, Vol. I. Part 5; Zoology, Vol. I. Part 7. 4to. 1878.

Lisbon: Académie Royale des Sciences—Memorias: Sciencias Moraes, &c. Tomo IV. Parte 2. 4to. 1877.

Sciencias Mathematicas, &c. Tomo V. Parte 1. 4to. 1875.

Jornal, Tomo V. and Nos. 21, 22, 23. 8vo. 1874-6, 7, 8.

Portugalliæ Monumenta Historica. Vol. I. Fasc. 4 Legum and Index. fol. 1873.

Dr. P. F. Da Costa Alvarenga: Leçons sur les Maladies du Cœur. Traduit par Dr. E. Bertherand. 8vo. Lisbonne. 1878.

Lunacy Commissioners—Thirty-second Report. 8vo. 1878.

Manchester Geological Society—Transactions, Vol. XIV. Parts 20, 21, 22. 8vo. 1878.

Mechanical Engineers, Institution of—Proceedings, June, 1878. 8vo.

Medical and Chirurgical Society, Royal—Proceedings, Part 47. 8vo. 1878.

Meteorological Society—Quarterly Journal, No 27. 8vo. 1878.

Meteorological Office—Meteorology of the North Atlantic during August, 1873. 4to, with Charts. fol. 1878.

Quarterly Weather Report, 1875, Part 3. 4to. 1878.

Musical Association—Proceedings, Fourth Session, 1877-8. 8vo. 1878.

New York Citizen, U.S.A.—J. Miller: Metaphysics, or the Science of Perception. 8vo. 1875.

Norfolk and Norwich Naturalists' Society—Transactions, Vol. II. Part 4. 8vo. 1877-8.

Pharmaceutical Society of Great Britain—Journal for July-Oct. 1878. 8vo.

Photographic Society—Journal, New Series, Vol. III. No. 1. 8vo. 1878.

Physical Society of London—Proceedings, Vol. II. Part 4. 8vo. 1878.

Preussische Akademie der Wissenschaften—Monatsberichte: Mai, Juni, 1878. 8vo.

Royal College of Physicians—Roll of the Royal College of Physicians of London. By William Munk, M.D. 2nd edition. 3 vols. 8vo. 1878.

Royal Society of London—Proceedings, No. 188, 189. 1878.

Sabine, Robert, Esq. M.R.I. (the Author)—Electrical Experiments with Crystalline Selenium. (Phil. Mag. June, 1878.) 8vo.

Statistical Society—Journal, Vol. XLI. Parts 2, 3. 8vo. 1878.

Startin, James (the Author)—Two Lectures on Ringworm and other Diseases of the Skin due to Vegetoid Parasites. (K 102) 8vo. 1878.

St. Bartholomew's Hospital—Statistical Tables, 1877. 8vo. 1878.

St. Petersburg, Académie des Sciences—Bulletins, Tome XXV. Nos. 1, 2. 4to. 1878.

Symons, G. J.—Monthly Meteorological Magazine, July-Oct. 1878. 8vo.

Tuson, Professor R. V. (the Editor)—Cooley's Cyclopædia of Practical Receipts, Parts 5, 6, 7. 8vo. 1878.

Twining, Thomas, Esq. M.R.I.—Catalogue of Free Public Library, Sydney, N.S.W. 8vo. 1878.

Works on New South Wales. 8vo. 1878.

Science Made Easy. Parts, 5, 6. 4to. 1878.

United Service Institution, Royal—Journal, No. 96. 8vo. 1878.

University of London—Calendar, 1878-9. 8vo.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, 1878. Hefte 6, 7, 8. 4to.

Victoria Institute—Journal, No. 47. 8vo. 1878.

Wisconsin Academy of Sciences—Transactions, Vol. III. 1875-6. 8vo. 1878.

Yorkshire Philosophical Society—Annual Report, 1877. 8vo.

Zoological Society of London—Transactions, Vol. X. Parts 7, 8, 9. 4to. 1878.

Proceedings, 1878, Parts 2, 3. 8vo. 1878.

GENERAL MONTHLY MEETING,

Monday, December 2, 1878.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Viscount Bangor,
Patrick Black, M.D.
Capt. Robert Goff,
John Thornton Rogers, Esq.

were *elected* Members of the Royal Institution.

The Chairman announced, That Mr. Spottiswoode, having been elected President of the Royal Society on November 30th, felt compelled to resign the office of Secretary of the Royal Institution.

He further reported, That the Managers had agreed to the following Resolution, which, on the recommendation of the Managers, was unanimously adopted by the Meeting:—

"The Managers cannot accept the Resignation of their most highly esteemed Secretary without a formal expression of their high appreciation of his zealous services during a period of thirteen years successively as Treasurer and Secretary, and of their deep sense of the great advantages he has conferred on the Institution by his high scientific and practical knowledge, and by his uniformly courteous and efficient fulfilment of the arduous duties of these offices; and they ask the President to be so good as to embody these opinions in a letter signed by himself on behalf of the Members of the Institution."

The following Arrangements for the Lectures before Easter were announced:—

Christmas Lectures.

Professor Dewar, M.A. F.R.S.—Six Lectures on A Soap Bubble; on Dec. 28 (Saturday), 31, 1878; Jan. 2, 4, 7, 9, 1879.

Professor Edward A. Schäfer, F.R.S. Fullerian Professor of Physiology, R.I. —Twelve Lectures on Animal Development; on Tuesdays, Jan. 14 to April 1.

J. E. H. Gordon, Esq.—Four Lectures on Electric Induction; on Thursdays, Jan. 16 to Feb. 6.

Professor Tyndall, D.C.L. F.R.S. &c.—Eight Lectures on Sound, including its Recent Applications and Methods of Reproduction; on Thursdays, Feb. 13 to April 3.

Professor H. G. Seeley, F.L.S. F.G.S.—Three Lectures on Reptilian Life; on Saturdays, Jan. 18, 25, Feb. 1.

Reginald W. Macan, Esq. (Ch. Ch. Oxford).—Four Lectures on Lessing; on Saturdays, Feb. 8 to March 1.

Walter H. Pollock, Esq. M.A.—Two Lectures on Richelieu and Colbert; on Saturdays, March 8, 15.

F. Seymour Haden, Esq.—Three Lectures on Etching; on Saturdays, March 22 to April 5.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Lords of the Admiralty—Nautical Almanac for 1882. 8vo. 1878.

Académie des Sciences de l'Institut, Paris—Mémoires, Tome XXXIX. 4to. 1877.
  Mémoires présentés par Divers Savants. Tomes XXI. XXII. XXIII. 4to. 1875–7. Deuxième Série, Tomes XXIV. XXV. 4to. 1877.
  Recueil de Mémoires, etc., relatifs à l'Observation du Passage de Vénus sur le Soleil. Tome I. et Supplément; Tome II. Partie 1. 4to. 1877–8.

Actuaries, Institute of—Journal, No. 113. 8vo. 1878.

Author, The—Free Notes on Herbert Spencer's 'First Principles,' with Suggestions regarding Space, Time, and Force. (K 102) 8vo. 1878.

British Architects, Royal Institute of—1878–9: Proceedings, Nos. 1, 2. Transactions, No. 1. 4to.

Civil Engineers' Institution—Minutes of Proceedings, Vol. LIV. 8vo. 1878.

Clinical Society—Transactions, Vol. XI. 8vo. 1878.

Devonshire Association for the Advancement of Literature, Science, and Art—Report and Transactions, Vol. X. 8vo. 1878.

Editors—American Journal of Science for Nov. 1878. 8vo.
  Analyst for Nov. 1878. 8vo.
  Athenæum for Nov. 1878. 4to.
  Chemical News for Nov. 1878. 4to.
  Engineer for Nov. 1878. fol.
  Horological Journal for Nov. 1878. 8vo.
  Iron for Nov. 1878. 4to.
  Journal for Applied Science for Nov. 1878. fol.
  Nature for Nov. 1878. 4to.
  Telegraphic Journal for Nov. 1878. 8vo.

Elliot, John Lettsom, Esq. M.R.I.—History of India as told by its own Historians, Vol. VIII. 8vo. 1877.

Franklin Institute—Journal, No. 635. 8vo. 1878.

Genève, Société de Physique—Mémoires, Tome XXV. Partie 1; Tome XXVI. Partie 1. 4to. 1877–8.

Geological Society—Quarterly Journal, No. 136. 8vo. 1878.

Knox, George James, Esq. M.R.I.—Comm. B. W. Tracey: The Pillar of Witness; a Scriptural View of the Great Pyramid. (K 102) 8vo. 1876.

Linnean Society—Proceedings, Nos. 76, 99. 8vo. 1878.

Lœwenberg, Dr. B. (the Author)—Les Tumeurs Adénoides du Pharynx Nasal. (K 102) 8vo. 1878.

London Institution—Journal, No. 39. 8vo. 1877–8.

Manchester Geological Society—Transactions, Vol. XIII. Parts 1, 2; Vol. XIV. Part 17. 8vo. 1873–8.

Medical and Chirurgical Society, Royal—Transactions, Vol. LXI. 8vo. 1878.

North of England Institute of Mining Engineers—Transactions, Vol. XXVII. 8vo. 1877–8.

Pharmaceutical Society of Great Britain—Journal for Nov. 1878. 8vo.

Photographic Society—Journal, New Series, Vol. III. No. 2. 8vo. 1878.

Preussische Akademie der Wissenschaften—Monatsberichte: Juli, Aug. 1878. 8vo.

Robertson, Rev. W. A. Scott, M.A. (the Author)—British Contributions to Foreign Missions. (O 17) 12mo. 1878.

Royal Society of Literature—Transactions, Vol. XI. Part 3. 8vo. 1878.

Royal Society of London—Philosophical Transactions for 1878, Part 1. 4to. 1878.

Royal Society of New South Wales—Journal of Proceedings, Vol. XI. 8vo. 1878.
Rev. W. B. Clarke: Remarks on the Sedimentary Formations of New South Wales. 4th ed. 8vo. 1878.
J. Rae: Railways of New South Wales. fol. 1877.
Report on Mines of New South Wales, 1877. 8vo. 1878.

Shoolbred, J. N. Esq. (the Author)—On the Present State of Electric Lighting (K 103) 8vo. 1878.

Symons, G. J.—Monthly Meteorological Magazine, Nov. 1878. 8vo.

Tuson, Professor R. V. (the Editor)—Cooley's Cyclopædia of Practical Receipts. Part 8. 8vo. 1878.

United Service Institution, Royal—Journal: Index, Vol. XI.-XX.
Journal, No. 97. 8vo. 1878.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, 1878. Heft 8. 4to.

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